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AND
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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.

J. B. Pinelli ad Mazonium.

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ERRATA IN VOL. XI.

Page 415, line 10; insert $1\frac{1}{2}$ in blank space.
— 432, — 9, for $C_1 : C$ read $C : C$
— — — 18, for $C_1 : C$ read C_1
— — — 21, for $C_1 : C$ read C_1
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ERRATA IN VOL. XII.

Page 189, art. 4, line 2, for x^n read x^a
„ „ „ 4, for tz' read tz
Page 192, art. 17, line 4, *dele* the initial "3"
„ 195, art. 29, line 4, for $(1+x^3)$ read $(1+x^3)^2$
Pages 189, 192, 196, arts 1, 15, 36, *for* vol. xiii. *read* vol. xii. [The XIII. at foot of No. 170 of the Proc. L. M. S. is wrong.]

Page 301. In the title of Mr. A. Gray's letter, *for* "Number of Electrostatic Units in the Electromagnetic Unit of Electromotive Force," *read* "Number of Electromagnetic Units in the Electrostatic Unit of Electromotive Force."

Page 304, lines 9 to 13. In the values of $R \tan a$ given in the quotation from Mr. Shida's report, *insert* the factor 10^9.

Page 335, line 9 from bottom, *for* Plate XI. *read* Plate IX.

Page 348, lines 32, 40, and 43, *for* dimension *read* dimensions.

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[FIFTH SERIES.]

JULY 1881.

I. *Remarks on* Dr. Mills's *Researches on Thermometry.* *By* T. E. Thorpe, *Ph.D., F.R.S., and* A. W. Rücker, *M.A.*[*]

A PAPER by Dr. Mills, F.R.S., entitled "Researches on Thermometry," has recently been published in the Transactions of the Royal Society of Edinburgh for 1880. We are ourselves engaged in a comparison of the air- and mercurial thermometers; and therefore, when Dr. Mills was good enough to send us a copy of his paper, we naturally read it with considerable care and interest. Of the labour which he has expended on several difficult thermometer-corrections we cannot speak too highly; but there are a number of points concerning which it would be desirable to have further information. Some of the difficulties we have found may be due either to errors, or to insufficient explanation of the terms and symbols used; and in view of the latter possibility and to avoid misunderstanding, we have been compelled to discuss the very simple mathematical formulæ employed with what may be thought excessive minuteness. Dr. Mills divides his paper into six sections; and we shall head our remarks concerning any one of them by the title which he has affixed to it. The sections are:—

I. Calibration and the calibration unit.
II. The correction for the exposed thread.
III. The movement of the zero with (1) time and (2) temperature.

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[*] Communicated by the Authors.

IV. Poggendorff's correction.
V. Comparison of the mercurial with the air-thermometer.
VI. Effect of compression.

In the present paper we confine our observations to the second and third of these points; in a subsequent communication we may have something to remark on the others.

The Exposure Correction.

An account is given of a very lengthy series of experiments made to determine the correction due to the difference between the temperatures of the mercury in the bulb and in the stem of a thermometer when the latter is not immersed in the source of heat. This has usually been expressed as follows:—If N is the number of exposed scale-divisions, $T-t$ the difference between the readings of the principal thermometer and of a second thermometer attached halfway up the exposed column, and if y is the correction (of course expressed in scale-divisions), then

$$y = \cdot 0001545\,(T-t)N.$$

The number ·0001545 is the coefficient of the apparent expansion of mercury in glass; but there is no doubt (as Holtzmann, Mousson, and Wüllner have long since pointed out) that this makes the value of y too great.

Dr. Mills deduces from his experiments a corrected formula of the form

$$y = (\alpha + \beta N)(T-t)N.$$

The expression $\alpha + \beta N$, for shortness, he indicates by x. We have here two points to notice.

Dr. Mills finds that x has different values for different thermometers, and then says (on p. 571):—"The values of y agree [*i. e.* in the case of the undernoted thermometers], first when N has the following values:—

Thermometer.	N.
2	0·0
3	166·0
4	278·6
6	123·2"

It is evident that y is here a misprint for x, as y must vanish when $N=0$, and can vanish for no other positive value of N. Hence it is impossible for the correction for a thermometer with an exposed column 166 divisions long to be equal to that of another when no part of the column is exposed; for the former cannot (unless $T=t$), and the latter must, be zero.

For equal values of $T-t$, the correction for thermometers 2 and 3 will be equal when (using an obvious notation)

$$(\alpha_2+\beta_2 N_2)N_2=(\alpha_3+\beta_3 N_3)N_3.$$

If we put $N_3=166$ and use the coefficients given in the paper, we get a quadratic in N_2, viz.

$$(\cdot 00014148+\cdot 000000037880\,N_2)N_2$$
$$=(\cdot 00013197+\cdot 000000057030\times 166)166.$$

Solving this by approximation, we find that N_2 is (neglecting fractions) 159. Instead, therefore, of the correction being the same (as the unfortunate slip with respect to the substitution of y for x might easily lead an unwary reader to believe) when the number of exposed degrees differed by 166, it was the same when they were 159 and 166 respectively and therefore differed only by seven.

We have not thought it worth while to find the corresponding values of y for the other two thermometers employed, as the following method of comparing the results appears to us to be more satisfactory than that given above.

The interval between the freezing- and boiling-points was apparently divided into 400 divisions. If, in order to make the differences large, we make the very favourable suppositions that the whole scale was in each case exposed so that $N=400$ and that $T-t=100°$ C., we obtain for the values of the corrections for the thermometers 6·2652, 6·2312, 5·9443, and 6·3076. As each division was about a millimetre long, it must have been impossible to read with certainty to less than a tenth; and thus the first place of decimals only is significant. So far, therefore, from proving that each thermometer "has its own independent equation for exposure correction," these results seem to us to show that the exposure corrections below 100° C. are, in the case of three out of four similar thermometers, practically identical. If the formulæ are intended to apply to the measurement of temperatures higher than 100°, in which case the differences between the corrections would be greater, experiments ought surely to have been made above that temperature.

The second point we have to notice is a mistake in transforming the expression for the correction from scale-divisions of the thermometers to degrees Centigrade. It is evident, if we consider the case of the same thermometer having two different scales engraved upon it, that the value of the correction in terms of a scale-division must be inversely proportional to the length of a division, and therefore directly proportional to N, or the number of divisions exposed. Hence the factor

$\alpha+\beta\mathrm{N}$ in the expression

$$y=(\alpha+\beta\mathrm{N})(\mathrm{T}-t)\mathrm{N}$$

must be independent of the scale. Thus, if we change from a scale in which the length of the exposed column is measured by N scale-divisions to another in which it is measured by $\frac{\mathrm{N}}{n}$, we must change β to $n\beta$. As, therefore, a scale-division of the thermometers used corresponded to a quarter of a degree, N divisions on this scale correspond to $\frac{\mathrm{N}}{4}$ divisions when degrees Centigrade are employed; and thus we must multiply β by 4.

The mean expression for x in terms of scale-divisions obtained from the observations on the four thermometers is

$$x=\cdot 00013321+\cdot 000000053046\,\mathrm{N}.$$

Dr. Mills writes (p. 571), as the equivalent of this when the degree Centigrade is the unit,

$$x=\cdot 00013321+\cdot 000000013261\,\mathrm{N}.$$

He has therefore divided the coefficient of N by 4 instead of multiplying it by that number, and has thus not only allowed a serious error to creep into what is perhaps the most important formula in his paper, but has done himself the great injustice of making his own correction appear sixteen times less than it really is.

If the above expression for x is taken as it stands, the second term in the expression for y is

$$\cdot 000000013261\,(\mathrm{T}-t)\mathrm{N}^2\,;$$

and if

$$\mathrm{T}-t=\mathrm{N}=100,$$

this becomes ·013261.

Now a thermometer graduated for 100°, and on which it was possible to read to the hundredth of a degree, would be of inordinate length; and as the millimetre divisions on Dr. Mills's thermometers corresponded to $0^\circ\cdot 25$, he could not possibly have read to less than $0^\circ\cdot 02$. In other words, if the formula as given is correct, the term added by Dr. Mills to the expression for y may not only be neglected for all ordinary instruments, but would have been absolutely inappreciable on those he used.

The correct value of y in terms of the Centigrade scale, as deduced from Dr. Mills's experiments, is

$$y=(\cdot 00013321+\cdot 000000212184\,\mathrm{N})(\mathrm{T}-t)\mathrm{N}.$$

Zero Movements.

With regard to the investigation of the movement of the zero, we would remark that it would have been simpler to use a formula which expressed the use of the zero directly. The expression adopted by Prof. Mills is

$$y = A\alpha^x + B\beta^x,$$

where x measures the time, and A, B, α and β are constants. The quantity y is called the total remaining ascent, and "A + B represents the total ascent," *i. e.* the whole rise up to some fixed time.

The rise of the zero (Z) up to the time x is therefore evidently

$$Z = A + B - y;$$

and the tables would have been more useful had this quantity Z been given instead of y.

The account of the movement of the zero with temperature is so involved that it constitutes a riddle which it took us some time to read. We quote the opening sentence:—

"It has long been known that the immersion of a thermometer into boiling water almost invariably lowers the zero. The only consecutive series of observations of the effect of temperature that I have been able to find is due to Henrici *. His results are readily comprised in the equation

$$y = 2{\cdot}100({\cdot}981)^x - {\cdot}099(1{\cdot}360)^x;$$

y being the total remaining ascent, and the unit of x being 10° C. The starting-point of the observations was 50° C.; and the depressions were consecutively observed at every 10° to 100°.

"Table vii.

x.	Zero observed.	y.	y calc.
0 ……	0·00	2·00	…
1 ……	−0·10	1·90	1·92
3 ……	−0·25	1·75	1·73
4 ……	−0·40	1·60	1·61
5 ……	−0·60	1·40	1·45
Probable error of a single comparison ……			·023

"

The formula here employed is of the same form as that used above, except that one of the terms is negative. The quantity y is, as before, called "the total remaining ascent." The effect of heating the thermometer having been to depress its

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* "Pogg. *Ann.* l. 251."

zero, the total ascent one would naturally suppose would be equal to the total depression; or, if the secular elevation of the zero could not be neglected, the "total ascent," however it might be compounded of this and of the temporary effect due to heating, would at all events increase with the temporary depression.

The table, however, shows that y grows less as the depression of the zero increases; and it is only from it that we are able to gather the relation between y and the position of the zero. If

$$y = \mathrm{A}\alpha^x - \mathrm{B}\beta^x,$$

and Z be the change in the position of the zero (depressions being taken as negative), then

$$\mathrm{Z} = -\{\overline{\mathrm{A}-\mathrm{B}}-y\} = -2+y$$

in the example given.

But though this expression is algebraically similar to that obtained for the secular movement of the zero, the meaning of the quantity y is quite different. In the first case it is the amount by which the reading for the zero falls short of a position which may be attained at some future time. In the second case it is the amount by which the depression of the zero falls short of that which may be produced by some future heating. If therefore any such name is to be applied, it should here be the total remaining *descent*. Even with this modification, however, the nomenclature is very misleading, as there is nothing to show that the points from which the "remaining" ascent or descent is measured are really limits to the motions of the zero.

The experiments on three other thermometers (455, 3, and c) are then described:—

"The results for thermometer 3 are given in terms of its scale, one division of which was equal to 0°·280. The equations are—

$$y_{455} = 2{\cdot}869(\cdot998)^x - \cdot143(1{\cdot}324)^x,$$

$$y_3 = 4{\cdot}723(1{\cdot}006)^x - \cdot723(1{\cdot}1964)^x,$$

$$y_c = 1{\cdot}112(\cdot9986)^x - \cdot112(1{\cdot}299)^x.$$

the values of a unit of x being respectively 13°, 20°, 38°."

It will be observed that all these expressions are similar to that given above for Henrici's thermometer. In all y is positive for small values of x; in all it diminishes as x increases. The three formulæ are illustrated by a table; and for all three the tabulated values of y are of the same sign. Both formulæ and table would therefore lead us to expect that, as in the case of Henrici's thermometer, the zeros were depressed, and that the depression increased with the temperature to which the thermometer was subjected.

Immediately after giving the table, however, Dr. Mills says (p. 579):— "The starting-point of the observations with thermometer 455 was 168°·6. When in its equation $y=0$, $x=10{\cdot}61$, the ascent under the influence of heat was thus converted into a depression after 306°·5." We shall have presently to refer to this change of direction of the motion of the zero. It is for the moment sufficient to say that no experiments were made at a temperature so high as 306°, and that therefore over the whole range of the observations the zero ascended. It is, however, hardly fair to the reader to leave him to gather, from the remark just quoted, a fact of which neither the formula nor the table gave any hint, viz. that thermometer 455 behaved differently from Henrici's, and that the zero *ascended* under the influence of heat. As to what the nature of this ascent was we are in the dark. Does Prof. Mills mean that the first heating depressed the zero, and that by subsequent heatings this depression was diminished? Or does he mean that the first heating raised the zero, and that subsequent heatings increased this elevation? We presume the former hypothesis is the correct one; but, as far as we can see, there is nothing either in the formulæ, the table, or the text to assure us of this. The fact, however, that in some cases heating raised the zero, probably supplies the reason for retaining the name "total remaining ascent," a descent being considered a negative ascent. But even if this could be justified by the analogy of such expressions as *negative acceleration*, it certainly does not assist the reader to assume that he will understand that an ascent is really a going down, without informing him of the convention which has been made.

We have dwelt at length on these points, not from any wish to draw attention to mere faults of exposition, but because we have now a much more serious matter to refer to; and we wish to show that if in this we have misunderstood Dr. Mills, it is because he is not very easy to understand. On page 578 Dr. Mills says:—

"The depresssion at 100° in Henrici's instrument is by far the largest at present recorded. It can easily be shown from the equation, that $y=0$ when $x=9{\cdot}35$; so that the zero in this case would have begun to *rise* after the immersion of the bulb in a bath at 143°·5."

Remembering that

$$y=2{\cdot}100({\cdot}981)^x-{\cdot}099(1{\cdot}360)^x,$$

and that the relation between y and the position of the zero, as gathered from the table given above, is

$$Z=-2+y,$$

we are at a loss to understand this remark. The depression of the zero Z becomes greater as y becomes less; and this is no way affected by the passage of y through the value 0. For numerically increasing negative values of y we get increasing depressions. It seems, therefore, that the statement that the formulæ point to a change in the direction of the motion of the zero is due to a confusion between Dr. Mills's y and our Z, and that, as a matter of fact, no such change is indicated. If we are wrong in this conclusion, we can only apologize to Dr. Mills and ask him to state what y really means. If we should turn out to be right, we are afraid this error upsets all the inferences drawn by him from his formulæ. But even if we have misunderstood the meaning of Dr. Mills's y, the deductions in question appear to be open to grave criticism.

The conclusion drawn from the experiments is, that "the zero of an ordinary vacuous mercurial thermometer undergoes three distinct movements under the influence of heat. It is at first depressed under the action of temperature, varying with each particular case ..... The further application of heat causes the zero to *ascend* ....., after which it again *descends*."

If we call the temperatures at which these changes take place turning-points, the following table fairly sets forth the grounds on which the conclusions are based.

TABLE I.

Thermometer.	Henrici's.	Dr. Mills's.		
		455.	3.	*c*.
Lowest temperature to which the thermometer was heated	50°·0	168°·6	40°·0	48°·1
Corresponding value of y.........	2·000	2·726	4·000	1·000
Highest temperature to which the thermometer was heated	100°·0	296°·8	120°·0	239°·2
Corresponding value of y.........	1·400	0·554	3·356	0·686
Motion of zero after heating ...	descent	ascent	descent	ascent
Turning-points calculated by Dr. Mills........................	143°·5	306°·5	256°·7	379°·8

It will be noticed that in no case is a "turning-point" included within the range of temperatures actually observed; and that in all, therefore, the existence of the upper point is a theoretical deduction from the formulæ. We cannot, however, allow that such a deduction, even if algebraically correct, is legitimate. Any one who has experience in the mathematical expression of the results of experiment must soon be strongly impressed with the untrustworthiness of empirical formulæ when their application is extended beyond the range of the observations themselves. The same series of figures can often be represented with almost equal exactness by very different mathematical expressions; and the curves to which these are the equations, though closely concordant for the range of the experiments, may have outside that range very different geometrical properties. We may indeed, in some cases, determine the constants in a formula which rests upon a theoretical basis from a limited series of experiments, and then extend its application; but it is very dangerous to argue from the form of a purely empirical equation. Instances may be quoted where such a formula has *à posteriori* been proved to hold for a much greater range than could have been anticipated; but it must be admitted that a determination of the position of a maximum or minimum from an empirical equation is worth very little, even when it lies within limits of the experiments, unless special precautions are taken, and that deductions as to the position, or even as to the existence, of such points when lying far outside those limits are absolutely untrustworthy. No one, for example, believes that the point of maximum density of ethyl alcohol is either at −56°·6 or at −89°·5, as deduced by Muncke from two separate and fairly concordant expressions for its expansion between 0° and its boiling-point. Dr. Mills, however, seems to trust his formulæ implicitly. Henrici observed a thermometer between 50° and 100°; and Dr. Mills infers a point of maximum descent at 143°·5. Thermometers 3 and *c* were observed for ranges of 80° and 190° respectively; and in each case conclusions are drawn respecting points about 140° above the highest temperatures observed. We venture to think, not that there is grave doubt as to the exact positions of the alleged maxima and minima (that, of course, would be granted), but that the formulæ afford no evidence of their existence. Even in the case of thermometer 455 it is an open question whether a point of maximum ascent or a point of final repose was being approached.

We turn next to the explanation given of the motions of the zero-point. "The first effect of heat on the bulb is ordinary expansion attended with a 'set.' The second or further effect is to cause the thin part of the glass to become sufficiently plastic to yield to the influence of barometric pressure, which causes a gradual collapse. In the third, or final, stage, at which the vapour of mercury has a sensible tension, the bulb is enlarged by outward pressure due to that cause, and the zero descends. If the above view be correct, it would follow that a thermometer open to the air and kept always in a vertical position would always exhibit a descending zero; the descent in the first stage being due to expansion and set as before, but in the later stages simply to the weight of the mercury acting upon a bulb now rendered partially plastic by heat. I cannot find that other investigators have left any record of such an experiment." Dr. Mills then gives the following table to show that the zero of an open thermometer observed after heatings to 50°, 100°, 150°, and so on up to 350°, fell more and more after each heating :—

Zero (therm. 20).	Scale.	Depression.
Before experiment ...	+·262 C.	0·000
After 50°	·134	·128
„ 102	−·058	·320
„ 150	·203	·465
„ 200	·360	·622
„ 250	·547	·809
„ 310	·494	·756
„ 350	·570	·832

"The first seven observations were made consecutively on one day, the eighth nearly two days later. The temperatures are not corrected."

We may point out that observations 6 and 8 in the above table are discordant, and that, if we have to reject one, it should rather be the eighth, which was made two days after the others, than the sixth, which is more directly comparable with them. If we therefore reject the last experiment instead of the sixth, as Dr. Mills practically does, the table is in contradiction to his theory, and shows a maximum depression after the thermometer was heated to 250°.

In any case, as the increments of the depressions at higher temperatures are less than for the lower, we think Dr. Mills ought to have tried whether, if these results were expressed by a formula similar to that employed in the preceding cases, it would not have given a minimum at some higher temperature. The fact that it is impossible to carry on the expeririments much above 350° with mercury is no reason for regarding the formulæ as less trustworthy beyond that point than the others which were treated in this way.

If by a rising zero is meant a zero which is less depressed after every heating, some observations of our own prove that it is quite possible to obtain this with an open tube. The bulb of a Jolly's air-thermometer of about 90 cub. centim. capacity, the stem of which was graduated in millimetres, was exhausted by means of a Sprengel's air-pump and filled with mercury. It was then placed in melting ice; and when the position of the mercury in the stem had become constant, it was noted. The bulb and its contents were then immersed in boiling water, and the reading in melting ice was again determined. The zero was found to be considerably depressed; but if the bulb was left at the temperature of the air, this depression steadily diminished. The experiments of heating the bulb once and observing the zero were repeated after an interval of four, and again after another interval of thirteen days, with the result that after the second and third heatings the zero fell to the same position as it had taken up after the first. The method of experiment was then changed. The bulb was raised to about 100° C.; and the water with which it was surrounded, having been allowed to boil for a few minutes, was run off, and the bulb was left to cool for half an hour or thereabouts. This operation was repeated during three consecutive days, in all twenty times; and the zero-point was determined as before. The mean depression of the zero immediately after the three previous boilings had been 12·38 millim.; it was now found to be only 9·35 millim. Twelve days afterwards the alternate heating and cooling was again commenced, and in the course of five days was repeated fifty times. The depression of the zero immediately after the last boiling was 7·71 millim. It had therefore been still further reduced.

The experiments are summed up in the following table:—

TABLE II.

I.	II.	III.	IV.	V.	VI.
Days after heating.	I.	II.	III.	IV.	V.
0.........	12·40	12·35	12·39	9·35	7·71
1.........	...	...	9·66	7·61	
2.........	9·80	...	8·49		
3.........	...	...	8·30		
4.........	8·50	...	7·98	...	2·7
5.........	...	...	...	5·04	
6.........	...	7·08			
8.........	...	6·68			
9.........	...	...	...	4·23	
13.........	...	6·20			

There were in all, as has just been stated, 5 series of experiments. Column I. gives the number of days which had elapsed since the last heating to which the bulb had been subjected in any particular series, at the time when the observations of the position of the zero recorded in the other columns were made. The numbers in the other columns are, in all cases, the depressions of the zero measured in millimetres below the original position observed before the bulb had been heated for the first time, *i. e.* in the first series of experiments.

These observations, then, show that the depression of the zero of an open thermometer diminishes (1) with the lapse of time after it has been disturbed by heating, (2) with numerous and rapidly recurring heatings and coolings. The latter of these two phenomena is identical with what we understand Dr. Mills to mean by an ascent of the zero; and we have therefore proved (as has indeed been noted by other observers) that this can be produced by frequent heating; but the fact that it has thus been observed in an open tube is not in accordance with Dr. Mills's explanation of these movements.

His experiments differ from ours in the fact that the bulb was raised, not to the same, but to continually increasing temperatures; but before any certain conclusions can be drawn, it would be necessary to know something about the past history of the thermometers employed. The effect of heating is "almost invariably" to depress the zero; the effect of frequent heating is to diminish this depression; and thus the effect of any particular heating is dependent in part on those which preceded it. It has generally been supposed that by repeated heating and cooling the fluctuation of the zero could be reduced to a very small amount; and Dr. Mills's experiments agree with this, in that thermometer 455 (which, as far as his paper shows, was heated most frequently) is that which, according to his formula, was most nearly approaching a stationary point. However this may be, it is evident that it cannot be the atmospheric pressure which causes the zero of an open tube to rise when it is frequently heated, and therefore that it need not be atmospheric pressure which produces the same effect in a closed thermometer.

Summing up, then,—our criticisms on Dr. Mills's paper are as follows:—

(1) The method of comparing one factor only of the exposure correction for different thermometers, viz. x, instead of the whole correction y, tends to exaggerate the importance of the variation of the corrections. In the case of three out of the four thermometers observed, it is, within the limits of the error of experiment, the same for the largest possible exposure and for a temperature-difference which is probably greater than any of those for which observations were made.

(2) The formula for the exposure correction in terms of degrees Centigrade is given incorrectly.

(3) In the investigation of the movement of the zero with temperature, we think that, by a mistaken interpretation of the formula for y, Dr. Mills has inferred the existence of turning-points, of which in reality it affords no indication. If, owing to the ambiguous definition of y, we are wrong in the above supposition, we think that the supposed higher turning-points are too far outside the range of the experiments to admit of the application of the formulæ to these determinations.

(4) For the reasons given we consider Dr. Mills's experiments with the open thermometer inconclusive; and the inference drawn from them is rendered still more doubtful by the results of our own observations.

II. *On the Conservation of Electricity, and the Absolute Scale of Electric Potential. By* Professor SILVANUS P. THOMPSON, *D.Sc., B.A.*[*]

1. *THE Law of Conservation of Electricity.*—In art. 35 of Clerk Maxwell's 'Electricity and Magnetism' the following paragraph occurs:—

"While admitting electricity, as we have now done, to the rank of a physical quantity, we must not too hastily assume that it is, or is not, a substance, or that it is, or is not, a form of energy, or that it belongs to any known category of physical quantities. All that we have hitherto proved is that *it cannot be created or annihilated*; so that, if the total quantity of electricity within a closed surface is increased or diminished, the increase or diminution must have passed in or out through the closed surface."

In the paragraphs immediately preceding that quoted, it had been pointed out what experimental evidence there is for the conclusion that, when the electrification is in any way produced, the algebraic sum of the + and − electrifications is zero. It results at once from this that, whatever the ultimate nature of electricity may be, it so far behaves like an incompressible fluid as to be subject to a law, resembling the Hydrokinetic Law of Continuity, of the general form

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0,$$

where u, v, w are the three components of the flow of electricity resolved along rectangular coordinates.

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[*] Communicated by the Author.

The experimental evidence which exists forbids us, moreover, to think that the absolute quantity of electricity within a closed surface can be altered by the transformation of any other physical quantity into electricity. For, indeed, electricity belongs to a category by itself. It does not possess mass, and therefore is not a form of ordinary *matter*; it does not conform to the same physical dimensions* as *energy*, and therefore cannot be in itself a form of energy. Neither matter nor energy, then, can be transformed into electricity; nor can electricity be itself transformed into either of these physical quantities.

Therefore any system of electric bodies—that is to say, any system whose parts consist of definite quantities of electricity—must be regarded as a conservative system, and therefore subject, when regarded as a system, to a definite Law of Conservation.

2. These considerations have therefore led the present writer to propound elsewhere†, in general language, a doctrine of the *Conservation of Electricity* similar to those already recognized as holding good in the case of two other physical entities—Matter and Energy.

The author proposes in the present paper to trace out some of the bearings of the doctrine of the Conservation of Electricity, particularly in relation to the questions of the Distribution of Electricity in Space and of the Absolute Scale of Electrification.

3. Although the late Professor Clerk Maxwell used the clear and emphatic language quoted above, the idea of the ultimate Conservation of Electricity appears to have been rejected by him, in consequence of the negative results which attended his experimental attempt to discover whether an elec-

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* Matter has dimensions $[M]$, Energy $[ML^2T^{-2}]$, and Electricity $[M^{\frac{1}{2}}L^{\frac{3}{2}}T^{-1}]$. The latter value is obtained from a consideration of the Law of Coulomb, that $Q \times Q \div L^2 =$ force $= [MLT^{-2}]$; whence

$$Q = [(ML^3T^{-2})^{\frac{1}{2}}].$$

But the dimensions of self-attractive matter may be similarly considered by Newton's Law that $-M.M \div L^2 =$ force $= [MLT^{-2}]$, whence

$$M = [-L^3T^{-2}].$$

And if this value be put in place of M in the dimensions of Q above, we get $Q = [\sqrt{-1}(L^3T^{-2})]$, a quantity whose dimensions differ only from those of M in being prefixed by the imaginary quantity $\sqrt{-1}$. This seems to indicate an important relation.

† See reference in 'Nature,' vol. xxiv. p. 78, to Preface of the author's 'Elementary Lessons in Electricity and Magnetism,' and to the independent enunciation of the same doctrine by M. G. Lippmann, in *Comptes Rendus*, t. xcii. p. 1049.

tric current possesses momentum*. He expressed himself as of opinion that, were any such action to be discovered, we should be able to regard "one of the so-called kinds of electricity" as a "real substance," and "we should be able to describe the electric current as a true motion of this substance in a particular direction." The experiment which Maxwell chose wherewith to test this question consisted in starting a current suddenly in a horizontal conducting ring or coil, suspended by a thin wire in a space in which the horizontal component of magnetic force was absent. He then expected to find that, "if electricity were a fluid like water," then at the moment of starting the current, and while its velocity was increasing, there would be a mechanical reaction upon the conducting coil or ring which would set the coil in rotation in the opposite sense to that of the flow of the current, a cessation of the current being likewise accompanied by a rotational reaction in the same direction as the flow. No such phenomenon was, however, observed. It is rather difficult to understand how a mechanical reaction could have been expected. To start a current in the coil requires the application of electromotive force, not of ponderomotive force. The suggestion appears, indeed, to have arisen out of a dilemma which is stated in art. 568 of Maxwell's treatise, as to whether the energy of an electric current is "of that form which consists in the actual motion of matter (kinetic energy), or of that which consists in the capacity for being set in motion" (potential energy). The solution to the dilemma is, that energy is not necessarily of either of these kinds, but may also be either electrokinetic or electropotential if of that form which consists in the actual motion of *electricity* or of that which consists in the capacity of *electricity* for being set in motion. We must, of course, accept the view that a system containing an electric current "is a seat of energy of some kind"—and that this energy must be of a kinetic order "since we can form no conception of an electric current except as a kinetic phenomenon" (Maxwell, art. 552). Yet its energy is certainly not ponderokinectic, but electrokinetic, being the energy which moving electricity has in virtue of its motion.

One other consideration advanced by Maxwell, as against the supposition that the electricity in the conductor may be regarded as the moving body in which we are to find the energy, is that whereas the (electrokinetic) energy of the current depends upon the external medium and its conditions, as well as upon the current itself, the (ponderokinetic) energy of a moving mass "does not depend on anything external to

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* 'Electricity and Magnetism,' art. 574.

itself." But here again the reasoning is at fault; for the kinetic energy of a moving body is equal to the half-product of its mass into the square of the velocity in space of its centre of inertia, a velocity which must be reckoned as relative to something else, and which depends therefore upon the external conditions*.

It would appear, then, that kinetic energy may be of either of two kinds—ponderokinetic and electrokinetic; and that potential energy may also be of two kinds—namely, either the mutual energy of two masses of self-attracting matter, or the mutual energy of two quantities of self-repelling electricity (see below, § 5).

4. *Relations between Matter, Energy, and Electricity.*—Of the three conserved physical quantities, Matter, Energy, and Electricity, it would therefore appear that two, namely Matter and Electricity, stand each in very similar relation to the third, Energy. The following facts bring out this relation:—

(*a*) To set matter in motion requires the expenditure of energy.

(*α*) To set electricity in motion requires the expenditure of energy.

(*b*) When the motion of matter is arrested its (pondero-) kinetic energy reappears in the form of heat (or some other equivalent form).

(*β*) When the motion of electricity is arrested, its (electro-) kinetic energy reappears in the form of heat (or some equivalent form).

(*c*) Energy spent upon an elastic body, when it suffers a strain under the action of a stress, passes from the (pondero-)kinetic to the (pondero-)potential state.

(*γ*) Energy spent upon a medium which possesses electric elasticity (see Maxwell, Art. 50), such as glass, air, vacuum, or any non-conducting dielectric, causes it to experience a strain which is partly electrical (*i. e.* an electrical displacement) and partly mechanical, the (electro-)kinetic energy passing over into potential energy, part of it consisting in the displacement of electricity against an electric reaction (*i. e.* an electromotive force), part of it consisting in the displacement of matter against a mechanical reaction (*i. e.* a force). In the preceding case (*c*) of mechanical stress producing mechanical strain, as when an elastic metal rod is struck, there is also (as Volpicelli has shown†) a partial passing

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* See also Clerk Maxwell, 'Matter and Motion,' art. cx.

† Volpicelli, *Comptes Rendus,* t. xxxviii. 15 Mai, 1854.

of the mechanical kinetic energy into the energy of electric displacements.

(*d*) The vibration of matter when arrested changes into heat or some equivalent form of kinetic energy.

(δ) The vibration of electricity (*i. e. light*, according to the view adopted in § 7 below of the electromagnetic theory of light as interpreted by the doctrine of the Conservation of Electricity) when arrested or absorbed changes to heat (unless, as shown later, the vibrations are too rapid to be comparable to the periods of calorific oscillations).

(*e*) The force with which a particle of matter acts upon another particle of matter varies inversely as the square of the distance between them, when the distance is varied, and is of the nature of an attraction. (Law of Newton.)

(ϵ) The force with which a quantity of electricity (at a point) acts upon a similar quantity of electricity varies inversely as the square of the distance between them, when the distance is varied, and is of the nature of a repulsion. (Law of Coulomb.)

5. *Ponderomotive and Electromotive Forces.*—The force with which one quantity of electricity acts upon another quantity of electricity may be regarded either as an electromotive force or as a true ponderomotive force; for it tends to move electricity, and also to move matter, if matter be associated with the electricity. If measured as a (ponderomotive) force by any ordinary means of measuring forces, it will in general be expressed by a numerical value different from that in which it would be expressed as an electromotive force by any of the ordinary measurements of electromotive forces.

An instructive example of this relation between ponderomotive and electromotive force is afforded by a consideration of the action of a quantity of electricity at any point A upon a small sphere, electrified with an electrification of the same sign, at a distant point B. The action of A is a repulsion exerted upon the matter of the sphere, which repulsion can be measured by balancing it against the torsion of a fibre or any other suitable mechanical force. But A also acts upon the electricity with which the sphere at B is charged, and repels it, with an electromotive force. If the sphere is moved against a mechanical reaction, such as that of the torsion of a fibre, work will be done; and its value will be the product of the average force into the distance through which the sphere is moved. If the electricity is moved against the electric reaction of some other electromotive force, work will be done equally. This may be accomplished either, (*i*) by the electricity being repelled through a conductor against some existing electromotive force, thereby doing work (as, for example, by being sent through electrolytic cells, where it does electrochemical work against the electromotive force of the cells); or (*ii*) by the charged sphere being itself moved (mechanical forces being supposed absent), the work in this case being the half-product of the quantity of electricity moved into the difference of potentials. In any case the potential energy of the system, as at first supposed, will reappear as energy in the final configuration of the system, whether that energy be transferred from its initial distribution to its final distribution in the system by electrical or mechanical reactions.

It is well recognized that the kinetic energy of a moving mass may be transformed into other forms of kinetic energy, the value of the transformation depending on the conditions of the transformation. So, also, the potential energy of a system may be transformed into either of the two kinds of kinetic energy and be re-transformed into potential energy by transference through the system under appropriate conditions, the force exhibited during the operation being ponderomotive or electromotive according to whether, in the round of changes from the initial to the final state, the energy is applied to the reaction of matter upon matter, or to that of electricity upon electricity.

6. *Relation of Matter, Energy, and Electricity in respect of Distribution in Space.*—Suppose all the matter in the universe were to be redistributed with a uniform distribution throughout space. Such a distribution would possess a maximum of potential energy, and would therefore be in the condition of most unstable equilibrium possible. The Newtonian law of self-attraction, varying in intensity with the inverse square of the distance (though it is probably not an ultimate property of matter), would at once come into play, and cause the system to tend to a condition of aggregation in nuclei, the potential energy turning into kinetic as the separate masses gravitated towards one another. As these masses clashed together, a part at least of their energy being transformed into heat, the tendency would set in, as laid down in the theory of the Dissipation of Energy, to produce a uniform distribution of temperature throughout space; which process would be further aided by the process of radiation, as laid down in the Electric Theory of Radiation below (§ 7) as consisting in a transference of energy through the forms of electrokinetic and electropotential energy, alternately exhibited in the vibrations of the interstellar medium, until absorbed by matter to reappear again as heat.

Suppose, on the other hand, all the electricity in the universe to be distributed throughout space with a perfectly uniform distribution. By virtue of the law of self-repulsion, varying with the inverse square of the distance, such a distribution will be in the most stable equilibrium of all possible distributions, since its potential energy is a minimum. In any other distribution, by which electricity should be aggregated so as to produce *more* at one point and *less* at another, the potential energy of the system would be greater.

Suppose, further, that both matter and energy being uniformly distributed throughout space, they should become associated together by each particle of matter receiving a definite portion of electricity as a charge. The potential energy of the self-attracting matter is a maximum ; that of the self-repelling electricity a minimum. Now suppose aggregation to take place toward a nucleus or nuclei, each particle of matter carrying its charge of electricity with it. The potential energy of the matter will diminish, its kinetic energy increase. The potential energy of the electricity will, on the other hand, be increasing, and will be increasing at the expense of that of the matter. If the rate of increase in that of the electricity at any moment becomes equal to or exceeds the rate of decrease in that of the matter, the tendency to aggregation will be arrested. Whether such a state of things can occur depends, in the first place, upon whether such a supposed association between matter and electricity can take place or continue, and upon the relative total amounts of electricity and of matter in the universe—in other words, upon the absolute charge of matter, supposing both matter and electricity be initially distributed uniformly and homogeneously throughout space.

7. *Electric Theory of Radiation.*—According to Clerk Maxwell's theory, "light is an electro-magnetic disturbance propagated in the same medium (the æther) through which other electro-magnetic actions are transmitted." Now, as shown above, electricity tends to a uniform distribution through space; it does not possess mass, while at the same time it possesses a quality corresponding to elasticity, and can become the recipient of energy of both kinetic and potential forms. Hence it can answer the functions usually postulated of the so-called "æther of space," the existence of which is assumed on the one hand by Young and Fresnel for the purpose of explaining the phenomena of physical optics, and on the other hand by Clerk Maxwell for the purpose of explaining the phenomena of electric and magnetic actions at a distance. Further, the existence of such a medium consisting of self-repellent extra-mundane corpuscles was assumed by Lesage

as affording an intelligible explanation of universal gravitation—an explanation which would be no less tenable were it supposed that actual ultimate atoms of matter are impenetrable to electricity.

It would appear, also, that electricity thus uniformly distributed through the universe would exercise a definite pressure throughout space, such as has been attributed to the æther by Mr. Tolver Preston for the explanation of cohesion, and by Mr. J. MacFarlane Gray for the explanation of certain phenomena of thermodynamics. Doubtless, also, the alleged æther is subject to a law of conservation, since we have no evidence that we can create or annihilate it, either directly or by transformation of it into any other physical quantity.

There appears, therefore, no reason why the right name should not be given to this supposed medium, which, in order to avoid confusion with existing terms, might be called *the Electricity of Space.*

Electricity, then, fills all space. Its inequalities of distribution, where electro-kinetic forces have done work upon it, result in placing *more* of it at certain points and *less* at others, or, in other words, in giving it a + and a − distribution. Its flow resembles that of an incompressible fluid, being accompanied by relative motions along stream-lines, with their respective pressures and tensions between the various parts. Its centrifugal force gives rise to the rotatory phenomena of electromagnetism and of magneto-optics. Its vibrations are radiant light and heat. Just as the air can take up the vibrations of a distant tuning-fork, and can convey them in waves to a near one and set it in motion if tuned to the right note, so the electricity of space can take up the vibrations of a molecule of sodium in a distant star, and can convey it in waves to a molecule of sodium in our laboratories and cause it to vibrate—the energy of the waves in the intervening space being alternately electro-kinetic and electro-potential in form. The so-called actinic rays are but vibrations of a higher frequency in the electricity of space.

8. We have here an explanation that is, at any rate, probable of the action of actinic rays in bringing about chemical reactions, especially those which, like photographic reactions, consist in the splitting asunder of chemically-compound molecules, and in producing gradual reduction to a simple state. Where the vibrations impinge upon the surface of the sensitive film, the waves penetrate at least a little way into the film, and for at least a short depth it is exposed to the alternating displacements of the electricity of space, and is therefore under the same conditions as if transient and very

rapidly reversed electromotive forces were impressed upon it. Now the impression of an electromotive force upon a film possessing, *as do all photochemically active bodies,* electrolytic conductivity, will tend to produce at least partial electrolysis. Hence the liberation of silver from its less stable salts under the influence of light. The special power of the waves of extreme refrangibility to produce these actions may be due to the changes in vector-potential being relatively more frequent for these than for the less-refrangible waves, and consequently more rapid in proportion to the time of vibration of the individual molecules. For if the period of the vibrations of the rays be nearly in agreement with that of the molecules of any kind of matter, the (electro-kinetic) energy of the alternating electric displacements might pass into the (pondero-kinetic) energy of calorific oscillations. We know that the periods of the rays of lower refrangibility agree more nearly with the vibration-periods of matter, and that their absorption produces a rise of temperature. We know also that the periods of the rays of higher refrangibility are small in comparison, and that those rays do not in general produce calorific oscillations. It may well be accepted that instead they produce the more direct effect of impressing alternate transient electromotive forces upon the quantities of electricity with which the individual molecules appear to be associated.

9. *Absolute Scale of Electrification.*—If, then, electricity be a single physical quantity, and subject to a law of conservation, it follows that one or other of the two known kinds of electrification, either that called "vitreous" or "positive," or else that called "resinous" or "negative," must consist of a defect, or of an excess, above the ordinary distribution, or average absolute charge, in the neighbouring bodies. Various small specific differences have been observed from time to time in the behaviour of the so-called positive and negative distributions of electrification; but from no one of these is it easy to draw any conclusions as to whether either the "vitreous" or the "resinous" electrification really is the excess. If there be two bodies at temperatures, one of which may be above that of the surrounding atmosphere and the other equally below, we can say, without hesitation, which of them has an excess, and which a defect, of heat. If we could, with equal certainty, say which of the two electric states really is due to an excess of electricity, and which to a defect, we should at once be further led to expect the existence of an *absolute zero of electrification,* which would be the state of a body deprived of all electricity. That none of the observed

phenomena afford a very striking criterion on this point indicates that all known electrical phenomena take place at an absolute electrification far above that of the absolute zero of electrification.

10. It appears, however, worth while to inquire into some of the obscure phenomena by which the two electrical states differ from one another, in order to see what bearing they have upon the question which of these two states is to be regarded as the *true plus* state and which the *true minus*. The chief phenomena in question are tabulated below.

	Phenomenon.	For "resinous" electrification.	For "vitreous" electrification.
1.	To produce discharge between two unequal spheres requires weaker electrification......... (*Faraday, Experim. Res.* 1501.)	when the smaller is —	than when it is +.
2.	A small ball, when discharging, gives off apparently less discharge when the electrification .... (*Faraday, op. cit.* 1491.)	is —	than when it is +.
3.	The range of variation in amounts of charge required to produce discharge is *always*......................... (*Faraday, op. cit.* 1393.)	*less* when charge is —	than when it is +.
4.	Drops electrified are more pointed when electrification (*Faraday, op. cit.* 1593.)	is —	than when it is +.
5.	Card pierced by spark under atmospheric pressure is pierced at a point .... (*Lullin's experiment.*)	nearer the — discharger	than the +.
6.	Spark-discharge turns to brush *sooner* .... (*Faraday.*)	at a — surface	than at a +.
7.	Brush-discharge turns to glow *later* .... (*Faraday.*)	at a — surface	than at a +.
8.	Discharge by a heated body takes place at a higher temperature for .... (*Guthrie.*)	— electrification	than for +.
9.	Discharge *in vacuo* has the appearance of a force ...... (*Gassiot.*)	emanating from — electrode.	
10.	Radiometer-vanes in vacuous tube are repelled .... (*Geissler.*)	from — electrode	toward + electrode.
11.	In high *vacua* the direction of the discharge is normal to the surface of................ (*Crookes.*)	the — electrode.................	and independent of the position of the + electrode.

Table (*continued*).

	Phenomenon.	For "resinous" electrification.	For "vitreous" electrification.
12.	In discharge by gas-convection the temperature is *higher*...	at the − pole.....................	than at the + pole.
13.	In discharge by disintegration (as in voltaic arc) the temperature is *lower*.........	at the − pole.....................	than at the + pole.
14.	In electric osmose the liquid is displaced toward .... (*Porrett's Phenomenon.*)	the − electrode..................	and from the +.
15.	In the case of diaphragm-currents the direction of the electric current coincides with that of the water. (*Quincke.*)		
16.	When filings of a metal are projected against a polished plate of the same .... (*Becquerel.*)	the filings are −	the plate +.
17.	The more pointed ends of pyroelectric crystals are ...	− while cooling	+ while warming.
18.	In Lichtenberg's figures the	− figures are less branched	the + more so.
19.	In the brush-discharge the pitch of the sound is higher (*Faraday, op. cit.* 1468.)	for − electrification...	than for +.
20.	In the brush-discharge the frequency of the discharge (*Faraday, op. cit.* 1501.)	for − is 6 or 8 times	that of +.
21.	The rate of loss of charge of an electrified body is......... (*Faraday, Matteucci.*)	greater if the charge be −	than if it is +.
22.	In general, discharge takes place more readily for	− electrification.	than for +.

In looking at the facts tabulated above, the great balance of evidence appears to be in favour of the view that the "resinous," or so-called "negative," electrification is that which has the stronger claim to be regarded as indicating the *true plus*, or excess of electricity. The most important arguments in opposition to this view are those to be derived from the phenomena of electric osmose and of diaphragm-currents, perhaps the least thoroughly investigated phenomena of the whole. But, on the other hand, the evidence is overwhelming that "resinous" electrification tends more rapidly towards dissipation or discharge than does "vitreous" electrification. The inference to be drawn from the observed rates of loss of charge is most important, because the law of loss of charge is the precise analogue of the thermal law of cooling. In addition to Faraday's results, there is a series of observations obtained by Matteucci[*], made with a torsion-balance under

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[*] *Annales de Chim. et de Physique*, ser. 3, t. xxviii. p. 386.

the most careful conditions. These researches show that the rates of loss of charge for equal "vitreous" and "resinous" electrifications are for feeble charges imperceptibly alike, but that with stronger charges the rate of loss of charge was always greater for "resinous" than for "vitreous" electrification.

Now this is precisely what analogy with the law of dissipation of heat would lead us to expect if "resinous" be the *true plus* electrification. The careful researches of Dulong and Petit showed that Newton's law of cooling (that the rate of loss of heat is proportional to the excess of temperature of the body above that of the circumambient air and of an enclosure in which it is placed) is inexact; for they found the rate of cooling of a hot body to be higher (for same excess) at higher temperatures than at lower. The somewhat complicated law which they finally enunciated as representing their results gives the absolute amount of heat emitted as being proportional to

$$(1{\cdot}0077)^{\theta},$$

where θ represents the temperature in Centigrade degrees—a function which for higher temperatures has values a little higher than if the radiation were taken as proportional to the absolute temperature, which might, *à priori*, have been expected to be the law.

A formula given by Péclet to express the number of calories per hour lost by a square metre of surface of a substance whose temperature was T° (Centigrade), cooling in air of temperature t° C., is as follows :—

$$Q = kT(0{\cdot}9556 + 0{\cdot}0037t)(1 + 0{\cdot}0056T),$$

where k is a coefficient depending only on the nature of the body. Here the first of the two factors in brackets is a term *very nearly* proportional to the absolute temperature, since the formula was calculated from observations in which the temperature $= 12°$ C.

A still more decisive result was obtained by Provostaye and Desains*, who investigated the law of *rate of heating*—the body to be experimented upon being placed within a vacuous enclosure surrounded by a steam-jacket, the rise of temperature from about 60° C. to 100° C. being watched. They found that the complete law of exchange of heat between two bodies at different temperatures might be represented, either for heating or cooling, by an expression of the general form

$$V = ma^{T} - ma^{T'},$$

where T and T′ are the two temperatures. But they found that the coefficient *m had less values for the case of heating than for the case of cooling.* They gave the following cases in illus-

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* *Ann. de Chim. et de Physique*, ser. 3, t. xvi. p. 409.

tration of the point that, for equal differences between the initial temperatures, the ratio between the rate of cooling down from a temperature above that of the enclosure and the rate of heating up from a temperature below that of the enclosure, increases with the temperature-range.

Difference of temperature.	Rate of cooling.	Rate of heating.
5°	1·039	1·000
10	1·0797	1·000
500	1·2153	1·000

Assuming as the lower temperature 0° C. (which is, however, not exact), it will be seen that the ratio of the rate of cooling to the rate of heating is not very different from the ratio of the initial and final absolute temperatures; for

$$1+{\cdot}003665\times\ \ 5=1{\cdot}018,$$
$$1+{\cdot}003665\times\ 10=1{\cdot}036,$$
$$1+{\cdot}003665\times 500=2{\cdot}832.$$

The great discrepancy of the last value is accounted for by the impossibility of comparing a cooling from +500° with a heating up from −500°.

It is therefore clear that, though the rate of loss of heat is not rigorously proportional to the absolute temperature, those bodies whose absolute temperatures are highest part more quickly with their heat for a given difference of temperature. Suppose, therefore, two bodies of equal mass and surface, and of equal specific heat, one heated, the other cooled, until the excess of temperature of the one is exactly equal to the defect of the other. If we had no other means of ascertaining which was at the higher absolute temperature, we could be quite certain, provided we only observed the rates of diminution in their respective differences of temperature from that of the mean-temperature enclosure in which they were placed; for that one whose rate of change of temperature was the greater would be the one which had initially a true excess of heat.

It seems only legitimate, when we consider the innumerable analogies between the diffusion of heat and that of electricity (especially in diffusion by conduction), to conclude that, as the rate of loss of charge in "resinously" electrified bodies is greater than that of "vitreously" electrified bodies, the state of "resinous" electrification corresponds in reality to that of *excess* or to a *true plus* electrification; and this is the electrification possessed by our earth relatively to the surrounding space.

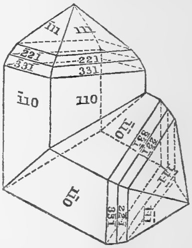
University College, Bristol,
May 24, 1881.

III. *Crystallographic Notes.* *By* L. Fletcher, *M.A.**

VIII. *A Twin of Zircon.*

ALTHOUGH zircon, rutile, and cassiterite have been long looked upon as isomorphous, zircon has been, till within the last three years, remarkable for the absence of the twin growth which is so usual a feature in the other two species. In 1878 Herr Otto Meyer†, in a paper relative to the rocks met with in the St. Gotthard tunnel, called attention to the fact that zircon was present in considerable quantity as a microscopic constituent of some of the crystalline schists, and further observed that the minute crystals were occasionally twinned according to the same twin plane (1 0 1) as the crystals of rutile and cassiterite. In the same year Herr Hussak‡ mentioned his discovery of simple microscopic twins of zircon according to the same law in the eklogite from Styria, and of both simple and polysynthetic twins in the mica schist of the St. Gotthard tunnel. One of the crystals was measured by him, and found to be 0·16 millim. long and 0·04 millim. broad.

Very lately large crystals of zircon have been found at Renfrew in Canada. The largest specimen which has yet reached this country weighs 408 grams: it is a simple crystal presenting the forms {1 1 0}, {1 1 1}, and {2 2 1}. A second specimen, weighing 52·785 grams, is of great interest as showing the twinning of zircon on a far from microscopic scale. The habit of the individual crystals and the general aspect of the twin will be evident from the accompanying figure. The forms present are the prism {1 1 0}, and the pyramids {1 1 1}, {2 2 1}, {3 3 1}; while on one quoin there is a well-developed face, which proved upon measurement to belong to the ditetragonal pyramid (3 1 1). The angle between two corresponding prism-faces of the different indivi- was found by measurement to be 44° 47′, a result sufficiently near to the calculated value 44° 50′ to prove that the growth is really due to twinning about the above-mentioned plane (1 0 1).



A twin of zircon.

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* Read before the Crystallological Society, May 30, 1881.

† *Zeitschrift der Deutschen geologischen Gesellschaft*, 1878, Band xxx. p. 10.

‡ Tschermak, *Mineralogische und Petrographische Mittheilungen*, 1878, p. 277.

The crystals are of a brownish colour, and are in parts quite transparent; the faces have an adamantine lustre; the specific gravity is 4·552, and approaches that of the specimens of the same mineral from the Ilmen Mountains.

IV. *On the Ascent of Hollow Glass Bulbs in Liquids.*

By Edmund J. Mills, *D.Sc., F.R.S.*[*]

1. When a hollow glass bulb is detained at some depth below the surface of a liquid and then released from its position, it rapidly acquires a sensibly uniform velocity; and the motion continues to be characterized by such velocity until the surface is reached or nearly reached, the velocity at that time being much accelerated. The bulb next rises somewhat out of the liquid and then falls; oscillating thus, until it finally comes to rest under the usual conditions of a floating body. In the present paper I propose to give an account of an experimental investigation which has reference to the stage of uniform velocity.

2. The bulbs I have generally employed consist of lead glass, and have, as will be seen in the accompanying figure, a somewhat pyriform shape; the stalk terminates in a hook, which carries an adjusted glass weight. A few small projections are sealed on to the body of the bulb in order to prevent too close accidental contact with a wetted boundary. In working with these bulbs, I have always effected the depression with a piece of glass tubing, and find that after some practice, it is seldom an experiment is lost by lateral divagations of the bulb. It is necessary that all the surfaces be clean.

Fig. 1.

Scale ½.

I. *Change of the Diameter of the Reservoir.*

3. A series of glass cylinders having been selected, their mean diameters between two fixed marks 220 centimetres apart were determined. These marks were ruled at places sufficiently distant from the surface and bottom of the liquid to ensure the uniformity of the motion observed between them. The liquids were first heated a little above the temperature desired for the experiments, then poured into the cylinders; and these were immersed in a much larger glass vessel containing water of very nearly the same temperature. The

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thermometers used could be read directly to tenths, and to hundredths by estimation. The times were taken with a metronome beating thirds of seconds.

4. The following experiments were made at 16° with a mixture of alcohol and water of sp. gr. ·9120 at that temperature; they are compared in the table with numbers calculated from the equation

$$\left.\begin{array}{l}(t-\tau)d^2=c,\\(t-6{\cdot}10)d^2=23{\cdot}59.\end{array}\right\}$$

TABLE I.

d.		*t* obs.	*t* calc.
5·66	....	6·56	6·84
4·76	....	6·83	7·18
3·37	....	8·17	8·18
2·79	....	8·62	9·13
2·20	....	12·08	10·97

Probable error of a single experiment, 0·44.

The experiments were actually made on a height of 11 centim.; and the values obtained for t were half those given in the table. Each result is a mean of seven determinations.

From the above comparison, it appears that the product of the number of seconds t (less a constant τ) into the square of the diameter d of the cylinder is a constant quantity c. The time of ascent through a cylinder of infinite diameter would have been $\tau=6{\cdot}10$ seconds. The law, however, shows indications of failure when the diameter is narrowed to 2·2 centim.

5. Table II. contains the results of some experiments with recently-boiled water, at 15°, and another bulb. The time is calculated to seconds. Each result is a mean of these determinations.

TABLE II.

d.		*t* obs.	*t* calc.
17·16	....	26·2	26·17
9·24	....	26·1	26·71
5·66	....	26·3	27·97
4·76	....	27·7	28·80
3·37	....	29·9	31·63
2·79	....	37·6	34·24
2·20	....	41·0	39·20

Equation, $(t-25{\cdot}95)d^2=64{\cdot}56$.
Probable error of a single experiment 1·29.
Sum of the errors (t obs. $-t$ calc.), $+0{\cdot}08$,

The last two numbers suggest the same remark as in (4).

6. A sample of hydrocarbon-oil was next tried, a different weight and bulb being used. Each result is a mean of six determinations. The sp. gr. of the oil was ·9120 at 16°, *i. e.* the same as that of the mixture of alcohol and water referred to in (4).

TABLE III.

d.	*t* obs.	*t* calc.
5·66	31·5	31·41
4·76	35·3	34·91
3·37	46·0	46·80
2·79	57·4	57·73
[2·20	90·8	78·88]

Equation $(t-22{\cdot}96)d^2=270{\cdot}65$.
Probable error of a single (1–4) experiment, 0·37.

II. *Change of the Unbalanced Pressure.*

7. The unbalanced pressure, to which the ascent of the bulb is due, can be conveniently changed by increasing or diminishing the weight attached to the bulb. If we take a centimetre as the unit of volume, and understand by δ the mean density of (bulb + contents + weight) referred to that of the liquid as unity, then $(1-\delta)=p$ may be termed the "unbalanced unit pressure."

The following experiments were performed with a bulb of the usual size, recently-boiled water at 11°·5 being the liquid. The diameter of the cylinder was 5·66 centim.; the times are stated in half-seconds; and each result is the mean of seven determinations.

TABLE IV.

t.	*p.*	tp^2.
20	·01139	·0025946
40	·00810	·0026244
81	·00569	·0026225

Equation, $tp^2=c=\cdot0026138$.
Probable error of c, 0·25 per cent.

8. Another set of experiments was carried out in the same cylinder, under nearly the same conditions, with five variations of weight. Each result is the mean of five observations of ascent through a height of 16·5 centim. The times are stated in thirds of seconds.

TABLE V.

t.		p.	tp^2.
15·5	....	·00438	·00029735
31·0	....	·00310	·00029791
60·0	....	·00223	·00029844
97·0	....	·00177	·00029705
146·2	....	·00143	·00029806

Equation, $tp^2=c=\cdot 00029776$.

Probable error of $c=\cdot 000000169=0\cdot 06$ per cent.

The product of the square of the unbalanced unit pressure into the time is therefore a practically constant quantity.

III. *Change of Content.*

9. In all the experiments above recorded the bulb contained air, probably at a pressure slightly below that of the atmosphere. The mean density of (bulb + contents + weight) is of course affected by the contents of the bulb, and then also the rate of ascent.

If we regard as correct the mathematical relation last arrived at (8), it is easy to deduce from it an equation which involves the specific gravity of the contained gas. Assuming other conditions to remain unchanged, the density of the gas is

$$\gamma=\mathrm{A}+\mathrm{B}\sqrt{\frac{t-\theta}{t}}.$$

In this expression t is the time of ascent through the constant height for any particular gas, θ the time of ascent corresponding to a gas of density A, and B the density of a gaseous content which just fails to let the bulb sink. It is convenient to put $\mathrm{A}=0$, which corresponds to the bulb being vacuous; B and θ are then deduced, as the "constants of the bulb," from two independent determinations.

10. In order to change at pleasure the contents of the bulb its form was slightly altered, as shown in fig. 2, and its volume enlarged. The two stoppers with which it was furnished were coated, on their ground surfaces, with a scarcely perceptible film of grease; the operator's hands were encased in gloves; and every experiment was carried out as speedily as possible. The gases were employed in the dry state, at an average barometric pressure of about 75·7 centim.; in the following table their densities are referred to hydrogen. Each result is a mean of several determinations; and the times are given in thirds of seconds.

Fig. 2.

Scale $\frac{1}{4}$.

TABLE VI.

Gas.	γ found.	γ.	t.
Carbonic dioxide ...	21·81	22·00	72·5
Oxygen	17·17	16·00	38·2
Air	14·13	14·50	30·6
Water-gas	6·44	6·00	23·0

The equation is $\gamma = 26{\cdot}032\sqrt{\dfrac{t - 21{\cdot}589}{t}}$.

Probable error of a single comparison, 0·51.

Great accuracy in work of this kind is, of course, out of the question; but for approximate determinations, when only very small volumes of a gas are available, the method will probably be advantageous.

11. The following set of experiments has reference to a medium consisting of two mixed liquids, the rate of ascent of a bulb in each of which separately is known. One of the liquids was castor-oil, the viscosity of which is very considerable; to 100 parts of this, successive quantities z of linseed-oil were added so as to gradually reduce the viscosity. The uniform temperature was 12°·4. By separate experiments with the oils it was found that, if t were the rate of ascent of the bulb in seconds per centimetre, λ the rate for linseed, α the unbalanced unit pressure in castor, and β the change in that pressure for every unit of linseed added, then

$$\left.\begin{aligned}&(t-\lambda)(\alpha+\beta z)^2 = c,\\&(t-{\cdot}32335)({\cdot}09122+{\cdot}00169z)^2 = {\cdot}034376,\end{aligned}\right\}$$

are equations which fairly represent the experiments.

TABLE VII.

t.		z taken.	z calc.
4·45454	....	0·0	0·0
3·10027	....	11·1	11·9
2·21798	....	25·0	25·7
1·52758	....	42·9	46·0
1·22140	....	66·7	61·8
0·32335	....	∞	∞

Probable error of a single comparison, 1·7.

Sum of the errors, +0·3.

12. Castor-oil presents the remarkable property of recovering its normal viscosity very slowly after heating. A sample of the oil, through which a bulb rose in a known number of seconds per 22 centim., was heated to a temperature not exceeding 80°, and cooled to its previous condition. The bulb

now took ten seconds less for its ascent. After a day's repose it rose again in the oil with its original velocity.

13. The fundamental formulæ given in this paper admit of easy extension to other cases. I have, for example, found in the ascent of a hollow glass bulb a good indication of the maximum density of water, and of the amount of dissolved matter in saline solutions; and it could obviously be applied to determine the specific gravity of solid bodies.

The experiments which I have recorded show clearly that the rate of ascent of a solid through a liquid depends, not only upon the density of the liquid, but upon some other property —probably the viscosity. When the ascending solid approximates in diameter to the diameter of the reservoir, special retardation occurs, and some new law controls the motion. The commencement of this retardation is earlier with oils than with water, earlier with water than with alcohol and water—earlier, in short, when viscosity is greater. In reservoirs of sufficient diameter, the rate is inversely proportional to the square of the altered condition.

My thanks are due to Messrs. Ellis and Smith for their aid in this investigation.

Glasgow, March 1881.

V. *Change of State: Solid-Liquid. By* J. H. POYNTING, *Late Fellow of Trinity College, Cambridge, Professor of Physics, Mason College, Birmingham*[*].

[Plate I.]

TWO distinct types of change of state from solid to liquid have usually been recognized. The most familiar of these is the ice-water type, in which, as the temperature rises, the solid remains quite solid up to the melting-point; when this is reached it begins to melt *at the surface*, and the temperature remains constant till the whole is liquid, when the temperature again rises. Corresponding to this change of state there is a definite latent heat. In the second class of bodies, of which sealing-wax and phosphorus are examples, there is a gradual softening as the temperature rises; and this softening takes place *throughout the mass*. There is no definite arrest of the rise of temperature, and no definite latent heat.

It has sometimes been supposed that the ice-water type is merely a limiting case of the sealing-wax type, where the softening takes place, but through a very small range of temperature. Prof. Forbes held this view, and by it attempted to explain regelation; but subsequent experiments have not

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[*] Communicated by the Physical Society.

supported the theory, and I believe it is now generally abandoned.

Since, in the ice-water form of change of state, fusion only takes place at the surface, it seems much more probable that it is an exchange phenomenon analogous to the change which takes place when water is evaporating, according to the kinetic theory. Just as in the case of water-steam, a steady state is reached when the number of molecules escaping from the surface of the water into the gas is equal to the number passing from the gas into the water, so in the case of water-ice a steady state (that is to say, the melting-point of ice) is probably reached when the number of molecules passing from the ice into the water is equal to the number passing from the water to the ice. For the analogue of the sealing-wax type of melting we must probably take the change of state which takes place in a liquid-gas above its critical point, where it changes gradually from a state rather liquid than gaseous to a state certainly gaseous.

In this paper I shall attempt to support this view of solid-liquid change of state. The following is a summary of the argument and the conclusions arrived at.

It is assumed that the maximum vapour-tension of a substance at any temperature is an indication of the number of molecules crossing its surface in a condition to escape. Now Regnault's experiments show that at 0° ice and water have the same vapour-tension; that is, the number of molecules crossing the surface of the ice ready to escape is equal to the number crossing the surface of the water in the same condition. Hence, when the two are in contact at 0°, the interchange of molecules is equal. For temperatures below 0°, Kirchhoff has shown that the vapour-tension of water is greater than that of ice, and above 0° it is less than that of ice—if ice can exist. (Another proof of this theorem is here given.) It is, then, easy to give a general explanation of the phenomena of melting and freezing by supposing that, if the temperature is not at the melting-point, the substance in the state with the greater vapour-tension will lose at the expense of the state with the less vapour-tension.

To explain the alteration of the melting-point by pressure, we must suppose that pressure alters the vapour-tension, and therefore the rate of escape of molecules, and that this alteration is different for the two states. Sir William Thomson has shown that a liquid in a capillary tube is in equilibrium with its vapour at a greater or less tension than at the plane surface according as the surface is convex or concave, upwards, and has given a formula for the difference. Accompanying this curvature of surface is a difference of pressure in the liquid; and I suppose the variation of vapour-tension to be due to the difference of pressure. A proof is given of Sir W. Thomson's formula, which seems to bring out more clearly the connexion of the phenomenon with the pressure, and which seems to apply to solids as well as liquids. According to this formula, the steady state (the melting-point) may be reached at any temperature if the pressure can be so adjusted that the vapour-tensions in the two states at that temperature and pressure are equal. The resulting lowering of the melting-point by pressure agrees in amount with that given by the well-known formula of Prof. J. Thomson.

It follows from this mode of regarding the subject, that, if in any way the ice can be subjected to pressure while the water in contact with it is not so subjected, then the lowering of the melting-point per atmosphere is about 11½ times as great as when both are compressed. I give the results of some experiments which I have made to test this, and which certainly seem to indicate that the fall of melting-point is much greater than the amount usually supposed if the ice alone be compressed.

The isothermals for ice-water are then discussed. It has been supposed that, if we could employ a sufficiently low temperature and high pressure, then ice would pass continuously into water; that is, the isothermals would have no horizontal part corresponding to a mixture of ice and water, and we should have a critical point. Assuming, however, that a mixture of ice and water completely freed from foreign gases can be subjected to great negative pressure or tension, it seems probable that there is another critical point at a temperature above 0° and at a high negative pressure; that is, the water-ice line is a closed curve. We know that below 0° the water isothermals can be prolonged below the horizontal portion, since water is unfrozen in certain cases,—and that the ice isothermals can be prolonged above the horizontal portion; for ice, at 0° say, can be suddenly compressed without melting in the interior. This suggests that the true form of the isothermals is a continuous curve, of the nature which Prof. J. Thomson has suggested in the case of liquids and their vapours.

If we suppose that the curves are continuous in the same manner for ice-water above 0°, then Prof. Carnelley's "Hot Ice" would seem to be represented by the prolongations upwards of the ice isothermals beyond the horizontal line to where they meet the line of no pressure. The critical point, which certain assumptions roughly fix at about 14° C., would then be an upper limit, or rather above the limit, to the temperature of hot ice in a vacuum.

In conclusion, it is pointed out that the sealing-wax type of melting is probably similar to the change of ice into water below the lower, or above the upper, critical points, if these exist.

Melting and Freezing of the Ice-water Type at ordinary Temperatures and Pressures.

It seems to have been conclusively proved by experiment that, in bodies of the ice-water type, change of state, either from solid to liquid, or the reverse, takes place only at the surface, or at a surface separating dissimilar portions. This would also seem to follow from the fact that the change of state always requires a certain finite amount of energy to be abstracted from, or supplied to, the mass without alteration of temperature. In the middle of a homogeneous body, where the temperature varies gradually, we must have the energy per unit of volume a continuous quantity as we pass from point to point. Hence, when at any point there is sufficient energy per unit of volume to change the state, either the surrounding temperature must be far above the ordinary temperature for change of state, or the surrounding substance must occupy an intermediate condition between the two states. On the former supposition we should certainly not have the ordinary change of state, though something of the sort may occur in the case of Dr. Carnelley's "hot ice;" and in the latter we should have the sealing-wax type, and no signs of this have been observed.

Since, then, change of state is a surface phenomenon, we are led at once to connect it with the escape of molecules which we know to be always taking place from the surface, as indicated by the definite vapour-tension which the body possesses, whether solid or liquid. Now Regnault's experiments have shown that at 0° ice and water have the same vapour-tension, and at the same time a mixture of ice and water at that temperature maintains the same proportion between the two constituents as long as no heat is allowed to pass into or out of it; that is, as many molecules escape from the water into the ice as pass in the opposite direction from the ice into the water. We seem, then, to be justified in assuming that *the number of molecules coming up to a given surface with a sufficient velocity to escape is indicated by the maximum vapour-pressure at that temperature.*

Now suppose that we have a mixture of ice and water below 0°. Kirchhoff has shown (Pogg. *Ann.* ciii. p. 206) that below 0° the vapour-tension of water exceeds that of ice by ·044 millim. of mercury per degree; and his reasoning will equally prove that it falls below it by the same amount above 0°, if ice can exist at such a temperature. Prof. J. Thomson has subsequently (Brit. Assoc. Report, 1872, p. 24; Proc. Roy. Soc. 1873; 'Nature,' ix. p. 392) arrived at a similar conclusion independently. A proof differing in arrangement from Kirchhoff's, and following out rather the line indicated by Thomson, will be given below.

In a mixture, then, of ice and water below 0°, since the water has the greater vapour-tension, more molecules will cross the surface from the water to the ice than in the opposite direction. The ice will therefore gain, while the water loses. At the same time the molecules will possess less energy when arranged as ice. Hence the temperature of the whole will rise, and this rise will go on till 0° is reached, when there is once more equilibrium—or till the whole is converted to ice, if that condition be previously reached. This seems sufficiently to explain the action of a small piece of ice dropped into water below 0°; and the fact that the change of state is a surface phenomenon seems to show that the presence of some ice is necessary to commence change of state.

If a mixture of ice and water at 0° be supplied with heat, as soon as the temperature rises ever so little above 0° the equilibrium of exchange is destroyed; for the vapour-tension of ice becomes greater than that of water, and therefore the number of molecules entering the water from the ice is greater than the number going in the opposite direction. But since the water arrangement requires more energy, heat is absorbed, and the mixture has a tendency to fall back to 0°.

Before going on to discuss the effect of pressure on the melting-point, I give a proof, with a somewhat more general result, of Kirchhoff's formula,

$$\frac{d\varpi'}{dt} - \frac{d\varpi}{dt} = \cdot 044 \text{ millim. of mercury,}$$

where ϖ' is the maximum vapour-tension of ice, and ϖ that of water.

Start with a volume v of water at temperature $-t°$. Let it evaporate, always at the temperature $-t°$, in a cylinder which it does not wet, at its maximum vapour-tension ϖ, which we suppose to be maintained by a piston. Let the ultimate volume of the water-vapour be V. Then the external work done in the expansion is $\varpi(\mathrm{V}-v)$.

Now let the vapour further expand, always at the same temperature and in equilibrium with the pressure, till we have reached a volume V′ at the maximum vapour-tension ϖ' of ice. Assuming Boyle's law to hold, the work done in this expansion is $\varpi'\mathrm{V}'\log\frac{\varpi}{\varpi'}$; and this would be 0 if $\omega=\omega'$.

Now introduce a particle of ice at $-t^\circ$ into the cylinder, and condensation into ice will go on till all the vapour has disappeared. If the ultimate volume of the ice is v', the work done on the substance is $\varpi'(V'-v')$.

Increase the pressure from ϖ' to $\varpi'+p$ till the melting-point is lowered to $-t^\circ$. If κ' is the coefficient of cubic compressibility of ice, $\frac{p^2}{2}\kappa'v'$ is the work done in the compression. Introducing a drop of water, allow the whole to melt into water under the pressure $\omega'+p$, the work done during the melting being

$$(\omega'+p)\{v'(1-p\kappa')-v(1-p\kappa)\},$$

where κ is the coefficient of cubic compressibility for water.

Now let the water expand to its original volume v by gradually reducing the pressure to ϖ. The external work done is $\frac{p^2}{2}\kappa v$.

We now have the substance in its original state; and the cycle through which it has been taken was reversible at every step; therefore

$$\int\frac{dQ}{T}=0.$$

But T is constant; therefore

$$\int dQ=0.$$

Then the total external work is zero, or

$$\varpi(V-v)+\varpi'V'\log\frac{\varpi}{\varpi'}-\varpi'(V'-v')$$
$$-(\varpi'+p)\left\{v'\left(1-\frac{p\kappa'}{2}\right)-v\left(1-\frac{p\kappa}{2}\right)\right\}=0.$$

By means of the equation

$$\varpi V=\varpi'V',$$

and neglecting products of ϖ and κ, this reduces to

$$\varpi V\log\frac{\varpi}{\varpi'}=p\left\{v'\left(1-\frac{p\kappa'}{2}\right)-v\left(1-\frac{p\kappa}{2}\right)\right\}+(\omega-\omega')v. \quad (1)$$

Neglecting the term $(\omega-\omega')v$, and putting for $\varpi'V$ $\omega_0V_0\alpha T$, where ω_0V_0 are the pressure and volume at 0° C., and T the absolute temperature, we have

$$\frac{\varpi}{\varpi'}=e^{\frac{p\left\{v'\left(1-\frac{p\kappa'}{2}\right)-v\left(1-\frac{p\kappa}{2}\right)\right\}}{\omega_0V_0\alpha T}}. \quad . \quad . \quad . \quad (2)$$

For temperatures near 0° C. we may neglect products of p

and κ, and we obtain as an approximation

$$\frac{\omega}{\omega'}=1+\frac{p(v'-v)}{\omega'V'},$$

or

$$\omega-\omega'=p\frac{v'-v}{V'}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

At 0°,

$$v'-v=\cdot 087, \quad V'=209037,$$

and the pressure required to lower the melting-point $t°$ is $\frac{760t}{\cdot 00733}$ millim. by the well-known formula. Substituting in equation (3), we get

$$\omega-\omega'=\cdot 044t,$$

or

$$\frac{d\omega'}{dt}-\frac{d\omega}{dt}=\cdot 044 \text{ millim. of mercury}, \quad . \quad . \quad (4)$$

which is Kirchhoff's result.

If the temperature be much below 0° C., we cannot make these approximations without further examination, as the terms containing κ and κ' in (2) may rise into importance.

It may be noticed that (2) could be used as an equation to determine p, the pressure required to produce a fall of the melting-point to T, if there were any accurate experimental method of measuring ϖ and ϖ'.

Effect of Pressure on the Melting-point.

If we are right in regarding the change from the solid to the liquid state as an exchange phenomenon in which the rate of exchange is indicated by the vapour-tension, we ought to be able to show that the pressure which lowers the melting-point to a certain temperature will so alter the rate at which the two states of the substance give off molecules from their surfaces, that at that temperature there will be an equilibrium of exchange. That is, we ought to be able to show that pressure alters the vapour-tensions of the two states, but alters them by different amounts, so that the equality of vapour-tensions now occurs at the new melting-point.

Now in the ordinary case, where the vapour-tension is measured we have the substance only under the pressure of its own vapour; but in the rise or fall of a liquid in a capillary tube we may have a substance in contact with its own vapour when the substance is at a very different pressure from the vapour in contact with it.

Sir William Thomson has shown (Proc. Roy. Soc. Edinb. 1870, vol. vii. p. 63; Maxwell's 'Heat,' 1877, p. 287) that if a liquid rises in a capillary tube so that its surface is concave upwards, and (we may add) the pressure of the liquid is less than at the plane surface, then the equilibrium vapour-tension is less than at the plane surface. If the liquid falls in the tube, so that the surface is convex and the pressure greater than at the plane surface, then the equilibrium vapour-tension is greater. It has been supposed that this difference of vapour-tensions is due to the curvature of the surface; and Fitzgerald has suggested that we may thus perhaps obtain a connexion between "two apparently unrelated quantities," the evaporation and the surface-tension (Phil. Mag. [5] viii. p. 384). But while a very slight impurity in a liquid can greatly alter the surface-tension, it has not been shown that it alters the evaporation to the same degree. I think that we must look for the explanation elsewhere than in the curvature of the surface; and I shall endeavour to show that we may account for the effect by the difference of pressures of the liquid at the curved and plane surfaces. The curvature of the surface is then, as it were, an accidental accompaniment of the difference of pressure, and not the cause of the variation in the vapour-tension. We might therefore expect to find the variation taking place also at flat surfaces if the pressure be altered, and with solid as well as with liquid bodies. We cannot directly investigate the vapour-tension of flat surfaces under pressure; but I shall assume that we may here take, instead, the rate at which exchange takes place when the solid and liquid are in contact with each other.

Sir W. Thomson's formula is

$$p=\varpi-\frac{2T\sigma}{r(\rho-\sigma)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where

p is the vapour-tension in contact with the concave surface,
ϖ is the vapour-tension in contact with the plane surface,
T is the surface-tension of the liquid,
ρ and σ the densities of the liquid and its vapour respectively,
r the radius of curvature of the curved surface.

If P be the difference between the hydrostatic pressures just beneath the curved surface and just beneath the plane surface, equation (5) may easily be put in the form

$$p=\varpi-P\frac{\sigma}{\rho}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

or a pressure P in the liquid increases the vapour-tension by an amount $P\frac{\sigma}{\rho}$.

The following proof of this formula, $p=\varpi-P\frac{\sigma}{\rho}$, is, I believe, applicable to both solids and liquids, and obtains a more general form for the result.

Let a volume v of a body (solid or liquid) be in a perfectly conducting cylinder (fig. 1, Plate I.) so arranged that the temperature is always constant. A porous plug, which the substance if liquid does not wet, is in the cylinder; and the holes in the plug are to be so fine that any required pressure can be applied to the liquid without forcing it beyond the further surface of the plug, the curved surface of the liquid there withstanding the pressure. A piston to which pressure can be applied is in contact with the substance; and beyond the plug is another movable piston to which any pressure can be applied, the arrows in the figure indicating the direction in which the external pressures are applied to the pistons.

Let the volume of the substance in the denser state at the pressure of its normal vapour-tension ϖ for the given temperature be v. Let V be the volume of the whole as vapour at the pressure ϖ. Let the equilibrium vapour-tension when the denser state is subjected to a greater pressure $\varpi + \mathrm{P}$ be p, and let the volume of the whole as vapour at the pressure p be V′. Let the coefficient of cubic compressibility be κ. Now take the body through the following cycle.

Increase the pressure to $\varpi + \mathrm{P}$ on the left-hand piston, and then let the substance evaporate through the plug to the right hand, pushing out the piston there at pressure p till the whole is evaporated to a volume V′. If p be greater than ϖ, let the vapour expand from V′, always in equilibrium with the pressure, finally arriving at a volume V and pressure ϖ. Now cover the porous plug, and, if necessary, commence condensation by introducing a small amount of the substance. Push in the right-hand piston at the pressure ϖ till the whole is condensed to volume v.

We have now conducted the substance through a cycle each step of which is reversible*. Then

$$\int \frac{d\mathrm{Q}}{\mathrm{T}} = 0.$$

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* It seems difficult to imagine a plug which would satisfy the condition of reversibility for the solid under great pressure in contact with its vapour. Perhaps the following would answer the requirements, if an ordinary porous plug is insufficient. Suppose the solid in a finely-divided state, and contained in a liquid which wets it but is of a very slightly greater specific gravity, and whose vapour-tension is negligible. During evaporation turn the cylinder with the vapour-chamber upwards. The particles of solid will rise up through the pores, and a small fraction of their surface will protrude, but they will otherwise be subjected to a pressure $\varpi + p$. For condensation reverse the cylinder. As the solid condenses on the surface it will rise up as fast as it is formed, and so increase the volume of the chamber and force back the piston.

But T is constant; then

$$\int dQ=0,$$

and the external work is, on the whole, zero. This gives us

$$\left(\omega+\frac{P}{2}\right)P\kappa v+(\omega+P)v(1-P\kappa v)-pV'-\varpi V\log\frac{p}{\varpi}$$
$$+\varpi(V-v)=0. \quad . \quad . \quad (7)$$

But since, at low temperatures such as we are here considering, Boyle's law is almost exact, we have

$$\varpi V=pV'.$$

Then, neglecting terms containing $\varpi\kappa$,

$$\omega V\log\frac{p}{\omega}=Pv\left(1-\frac{P\kappa}{2}\right),$$

or

$$\frac{p}{\omega}=e^{\frac{Pv}{\varpi V}\left(1-\frac{P\kappa}{2}\right)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

For ordinary values of P this gives

$$p-\omega=\frac{Pv}{V}=\frac{P\sigma}{\rho}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

which agrees with Sir W. Thomson's result in equation (6).

It may be worth while to point out the following result of the reasoning on which the above proof is based.

In a quantity of liquid at a uniform temperature, the number of molecules interchanged across a surface will increase as we descend, owing to the increase of pressure. If near the surface the number be proportional to the vapour-tension at the surface, then at any depth the number will be proportional to the pressure in an atmosphere of vapour at that level which, at the level of the surface, has the pressure of the vapour in equilibrium; that is, the liquid will behave as a non-vaporizing solid through whose interspaces the vapour can move freely.

Assuming, then, that equation (9) holds both for solids and liquids, let us apply it to the case of ice and water in contact with each other at a temperature $-t^\circ$ and at a pressure P, such that $-t^\circ$ is the melting-point.

Let ϖ be the normal vapour-tension of water at $-t^\circ$,
ϖ' „ „ „ ice at $-t^\circ$,
p be the altered vapour-tension of water,
p' „ „ „ ice,
ρ the density of water, v its specific volume,
ρ' „ „ ice, v' „ „
σ „ „ their vapour, V its specific volume.

Taking σ and V as the same for ice and water as an approximation, then equation (9) gives us

$$\left.\begin{aligned} p&=\varpi+\mathrm{P}\frac{\sigma}{\rho}=\varpi+\frac{\mathrm{P}v}{\mathrm{V}},\\ p'&=\varpi'+\mathrm{P}\frac{\sigma}{\rho'}=\varpi'+\frac{\mathrm{P}v'}{\mathrm{V}}.\end{aligned}\right\}\quad . \quad . \quad . \quad (10)$$

Subtracting, we have

$$p-p'=\varpi-\varpi'-\mathrm{P}\frac{v-v'}{\mathrm{V}}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

But by equation (3) we have

$$\omega-\omega'=\mathrm{P}\frac{v'-v}{\mathrm{V}};$$

then

$$p-p'=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Or, under the pressure P at the melting-point, the vapour-tension of ice equals that of water, and there is an equal interchange of molecules taking place. According to this, then, we may thus regard the alteration of melting-point by pressure. The pressure increases the number of molecules given off from the surfaces in contact with each other in both states; but the increase is greater in the case of the less dense state. Now, in the case of ice-water, ice is the less dense state, and below 0° it has the less vapour-tension. Hence a sufficiently great increase of pressure, while increasing both vapour-tensions, can make that of ice overtake that of water, or can lower the melting-point. For paraffin, the liquid is the less dense state. Then, increase of pressure can only render the two vapour-tensions equal above the normal melting-point when the liquid vapour-tension is less than that of the solid.

Suppose now only one of the two states (the ice) to be subjected to increase of pressure. For instance, let the ice be compressed on a porous plate through which the water can circulate freely. Then the pressure increases the rate at which molecules escape from the ice into the water, but does not affect the rate of escape of the water-molecules into the ice, and a much less pressure will suffice to produce equilibrium of exchange for a given temperature below 0° than when both ice and water are subjected to the pressure.

To calculate the fall in melting-point produced by a pressure P′ on the ice alone, we have, instead of (10),

$$\left.\begin{aligned} p&=\varpi,\\ p'&=\varpi'+\frac{\mathrm{P}'v'}{\mathrm{V}}.\end{aligned}\right\}\quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

If we have $p=p'$, we have the melting-point; and in this case, by subtracting, we obtain

$$\varpi-\varpi'=\frac{P'v'}{V}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

Now the pressure required to lower the melting-point to the same degree when both ice and water are compressed is given by

$$\varpi-\varpi'=\frac{P\,.\,v'-v}{V}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

or

$$\frac{P'}{P}=\frac{v'-v}{v}$$

$$=\cdot 087. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Or the fall in melting-point caused by a given pressure on the ice alone is about 11½ times as great as when both ice and water are compressed. That is, 1 atmosphere lowers the melting-point about ·0843° C., and 11·7 atmospheres lower it 1° C. This result may be obtained in the same way as Prof. J. Thomson's formula, on the supposition that the process is reversible; but as I was led to the result by the above considerations, I have given only this proof.

This seems to have an important bearing on ordinary cases of regelation, when two pieces of ice are brought into contact at one or two points. About that point the ice will be subjected to great pressure; but the melted water is not necessarily subject to the pressure, and accordingly the melting-point may be lowered by 11½ times as much as has been formerly supposed. I have made some experiments to test this result; and the best arrangement I have yet devised has been the following:—A block of ice, fitting in a hollow iron cylinder with open ends, was laid on a bed of sand on the top of which was placed one junction of a copper-iron thermopile; the other junction was placed in melting ice. When the two junctions reached the same temperature, as indicated by a galvanometer in the thermopile circuit, pressure was applied to the ice by a hydraulic press. The water from the melting of the ice was able to escape freely through the sand, and was therefore only at atmospheric pressure. The results so far have been very variable, sometimes indicating no greater lowering of the melting-point than that usually assumed—·0073° per atmosphere. But in several cases the lowering has been decidedly greater. The following experiment gives the greatest value I have yet obtained for the lowering of the melting-point. The galvanometer-deflection per

1° difference in the temperature was determined by separate experiments to be 9·4 divisions.

Time, April 30.	Pressure, in tmospheres.	Galvanometer deflections, in divisions.	Temperature of the cooler junction.	Calculated temperature, at ·0073 per atmosphere.
h m 12 51	18	4·5	−·48° C.	−·13°
12 53	18	5·3	−·56	−·13
12 57	18	5·0	−·53	−·13
1 30	9	2·5	−·27	−·065
1 32	9	3·3	−·34	−·065

It will be seen, by a comparison of the last two columns, that the lowering here was four or five times that given by the usual formula. I have not thought it necessary to give details of the other results, as I have not yet had time to investigate the causes of failure. I hope to pursue the subject shortly.

Perhaps the following imaginary experiment may serve as a simple illustration of the last two sections. Suppose two cylinders, one containing ice, the other water at the same temperature, to be connected above by a tube through which the vapour can pass, and let them only be in contact with their own vapour.

At 0°, or rather at +·0073, their vapour-tensions being equal, as soon as the pressure reaches 4·6 millim. then the ice and water will remain unaltered in amount as long as no heat is allowed to pass into or out of the cylinders. If the temperature be kept slightly below 0°, then, since the vapour-tension of water is now greater than that of ice, the water will gradually distil over into the ice-vessel and there condense as ice, the average temperature rising. If the temperature be kept constant, however, the whole of the water will in time go over into the ice-vessel. If the temperature be slightly above 0° (supposing it possible still to keep the ice solid), then the ice has the greater vapour-tension and will gradually distil over into the water-vessel, and the average temperature will fall. In time, if the temperature be kept above 0°, the whole of the ice will go over into the water-vessel.

If, now, the ice and water be subjected to pressure by porous pistons which the water does not wet (the pressure in each cylinder being the same), then, if the temperature be 0°, an increase of pressure will cause more evaporation from the ice than from the water; that is, the ice will distil over into the water-cylinder and form water there. To obtain equilibrium again, the temperature must be lowered to such a point that the pressure makes the two vapour-tensions once more equal, when the ice and water will remain unaltered in amount—that is, the melting-point will be reached. If now the ice alone be subjected to pressure, its vapour-tension will be increased while that of the water remains the same. And now the pressure required to produce equilibrium of vapour-tensions at a given temperature below 0° will only be about 2-23rds of that required when both are subjected to the same pressure.

The suppositions which I have made amount to this—that if the space filled with vapour be abolished and the ice and water be brought directly into contact with each other, then the rate of escape of molecules will be the same as before in each case, or bear the same proportion to it.

Isothermals of Ice-water: Critical Points.

If we draw the isothermals for ice and water on a pressure-volume diagram, they are of the general form shown in fig. 2, though the figure is entirely out of proportion.

If we may assume that the compressibility of water is considerably greater than that of ice, the horizontal part of the isothermals representing a mixture of ice and water will increase as the temperature falls below 0°, at least just at first. Then, if we call the line passing through the points where the isothermals turn to or from the horizontal part the ice-water line, this line will at first diverge as the temperature falls. Now, while ice contracts on cooling, its coefficient of expansion between $-19°$ and 0° being given as ·000122 by Brunner, Despretz has shown that water expands on cooling below 0° even more than it expands for an equal rise above 8°. Hence the isothermals for ice and water approach each other at ordinary pressures as the temperature falls.

Using Brunner's coefficient for ice, and for water Hällström's formula (Jamin, *Cours de Physique*, vol. ii.),

$$\frac{v_0}{v_t} = 1 + {\cdot}000052939\,t - {\cdot}0000065322\,t^2 + {\cdot}00000001445\,t^3,$$

and supposing that water could be cooled without freezing, it will be found that between $-120°$ and $-130°$ ice and water would have the same specific volume. This might lead us to suspect that the divergence of the two branches of the water-ice line would not continue if we could examine the isothermals at very low temperatures and high pressures, and that, as the temperature fell, the two states would at some point begin to approach (that is, the horizontal part of the isothermals would decrease), and that ultimately ice would pass gradually into water without any abrupt change of volume (that is, there would be a critical point). Below this critical point ice and water would probably be identical.

A similar conclusion is arrived at from the latent-heat equation. On the supposition that at the critical point the latent heat vanishes, the temperature given by that equation is −122°·5, with a pressure of over 16,000 atmospheres (Baynes, Thermodynamics,' p. 169).

It is usually assumed that we must stop the isothermal at the base-line of no pressure. But we know that water can be subjected to a negative pressure; as, for instance, when it rises in a capillary tube in a vacuum, or when it adheres to a barometer-tube at a height greater than that of the barometric column. It seems probable that, if perfectly freed from foreign gases, it might even be subjected to a very high negative pressure without the particles being torn asunder. So, too, a mixture of ice and water might probably be subjected to tension. It seems at least worth while to draw the isothermals for ice and water on such a supposition.

Prof. J. Thomson's result for the alteration of the melting-point by pressure would hold for at least a short distance above 0° when we replace pressure by tension. Assuming it to hold for 4°, we should have to put on a tension of 4÷·00733 atmosphere = 545 atmospheres. But if the expansion of water under a tension equals its compression under an equal pressure, the expansion is about $\frac{1}{21000}$ per atmosphere*; so that the volume of the water at 4°, under a tension of 545 atmospheres, will be 1·026. The ice, whose volume at 4° under no pressure would be 1·088, probably will not expand nearly so much under tension. The change of volume on melting will therefore probably be not very far from

$$1·088-1·026=·062,$$

against a change at 0° of ·087. Then the two branches of the ice-line will converge very considerably for temperatures above 0° and with negative pressures. At this rate of convergence the meeting-point is at about 14° C. At higher temperature the ice would pass gradually into water—that is, we should here have another critical point,—the two critical points being at opposite ends of the closed curve which represents the water-ice line.

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* Might not the truth of this supposition be tested by the propagation of sound through the water above a barometric column at a negative pressure?

On considering the isothermals below 0°, it will be noticed that the water-isothermals, at least as far as that for −20°, can be prolonged downwards past the horizontal line to meet the line of no pressure; for Despretz succeeded in cooling water to −20° in thermometer-tubes without freezing. These prolongations are represented by *a a′*, *b b′*, *c c′* (fig. 2). Similarly the ice-isothermals can be at least slightly prolonged upwards past the horizontal line. For, suppose we take a block of ice at 0° and suddenly subject it to great pressure. Since it expands on heating, then sudden compression produces, if any thing, a slight rise in the temperature. At the same time the melting-point is lowered, and the ice begins to melt at the surface, and in time the whole will be lowered to the new melting-point. But just at first, and until it falls to that temperature, we have the ice on the prolongation of the isothermals upwards as at A A′ or B B′ in fig. 2. In a certain sense, then, we have "hot ice."

Since, then, the water-isothermals may be prolonged downwards and the ice-isothermals upwards, we may probably here adopt Prof. J. Thomson's suggestion as to the true shape of the isothermals in the case of liquid-and-gas mixtures (Brit. Assoc. Report, 1871, p. 30; Maxwell's 'Heat,' p. 125). This is indicated by the dotted line for −2° in the figure. If the isothermals also have this shape above 0° (as indicated by the dotted line for the 4° isothermal), then at first the ice-isothermals will be prolonged upwards to meet the line of no pressure, as, for instance, that of 4° at H. This seems to be the place where we must put Dr. Carnelley's "hot ice," on the diagram, if its temperature be really proved to be above 0°.

But if the critical point for the higher temperature exist, it is evident that, before this temperature is reached, the prolongations of the ice-isothermals will cease to reach up to the line of no pressure, and the limit to the temperature of hot ice in a vacuum is that of the last isothermal which touches the line of no pressure. To obtain ice at still higher temperatures, it would apparently have to be subjected to great tension. If the above calculation for the critical point is at all near the truth, then the highest temperature possible for ice in a vacuum is something below 14° C.

The view here advocated as to the nature of the melting of ice, would show that its fixity is as much a "constant accident" as the fixed boiling-point of water. If we have a piece of ice at any temperature and allow no water to form on its surface, then I see no reason why it should melt if heat be supplied to it by conduction from bodies which, when melted, it does not wet. I think, then, we ought to expect its temperature to rise, as Dr. Carnelley has apparently found to be the case.

Dr. Lodge has pointed out ('Nature,' Jan. 20, 1881) that, as far as we know, "there is no definite subliming-point for a solid, any more than there is a definite evaporating-point for a liquid." Hence, with such a mode of supplying the heat as above described, the temperature might perhaps be expected to rise to that of the last isothermal which reaches the line of no pressure. When it has reached this point the whole will be in an unstable state, and we might expect a further supply of heat to cause a sudden change into water. If, however, at any point in this process of raising the temperature the vapour-tension is allowed to rise nearly to its maximum, it will exceed that of water, which has a lower maximum; then a layer of water will be formed on the ice, and we shall have melting with a tendency of the temperature towards 0°.

The Sealing-wax Type of Melting.

We have seen that there is some reason to suppose that ice would pass gradually into water at a sufficiently low temperature and with sufficiently high pressure; that is, there would be no abrupt change of volume at a constant temperature, and no definite latent heat. But these are just the characteristics of the melting of substances of the sealing-wax type; and I think it exceedingly probable that we have such substances at temperatures below their critical points, or at least that they are analogous to water-ice below its critical point. If sealing-wax have a critical point, then if we start with some in the solid state at ordinary temperature, and while raising the temperature we increase the pressure so as always to keep it solid till above the critical point, if we reduce the pressure again to a certain point and at the same time a small amount of liquid sealing-wax be introduced, we ought to have a liquefaction of the whole with a finite expansion of volume; that is to say, we should have changed the ordinary sealing-wax type of melting into the ice-water type. It might, perhaps, be possible to test the truth of this supposition experimentally.

VI. *On some Electromagnetic Experiments with Open Circuits.* *By* J. J. Thomson, *B.A., Fellow of Trinity College, Cambridge*[*].

It has been noticed incidentally by Helmholtz and others that a coil acts in many cases like a condenser, and possesses appreciable electrostatic capacity. I am not aware, however, of any experiments in which this property produced any very marked effect. In the experiments I am about to describe, where currents were induced in a secondary coil, the secondary circuit was generally left open; and it was found that, under certain circumstances, the condenser-action of the coil was great enough to produce local currents strong enough to powerfully magnetize a sewing-needle placed inside a spiral through which these local currents passed. The following is a description of the coils used, which closely resemble some used by Lord Rayleigh in some electromagnetic experiments described in his paper "On an Electromagnetic Experiment," Phil. Mag. June 1870.

Two wires, each about 300 yards long, were wound side by side on a bobbin: one of these wires was used as the primary circuit; the other, which was cut into three pieces each about 100 yards long, formed three separate coils which could be connected up in a great variety of ways, and which were used as the secondary circuits. For convenience, I shall denote the electrodes of the first of these secondary coils by the letters A and B; the electrodes of the second by C and D; and the electrodes of the third by E and F. As it will be convenient to have some notation to denote the way the secondary coils are arranged, I shall signify the arrangement by writing the letters denoting the electrodes in the order one would meet with them if one were to travel continuously along the wire of the coil. A small magnetizing-spiral, in which an ordinary sewing-needle could be placed, was also inserted in the secondary circuit. The position of this spiral will be denoted by placing a — between the letters denoting the electrodes between which it is placed: thus ABCD—EF denotes that the electrode B was connected with the electrode C, the electrode D to the electrode E, while the ends A and F were free; the magnetizing-spiral was placed between D and E. Again, AB—CDEF denotes that the electrode B was joined to D, the electrode C to E, while the ends A and B were free; the magnetizing-spiral was placed between B and D.

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[*] Communicated by the Author.

The current through the primary circuit was produced by three Grove cells connected in series. The primary current was broken six times whilst the same needle was in the magnetizing-spiral, in order to get rid of any irregularities in the breaking of the contact. It was found to be a matter of indifference whether the needle was left in the spiral whilst contact was made in the primary circuit, or whether it was taken out for each make; this is doubtless owing to the make being a much more gradual operation than the break. The intensity of the currents through the spiral was estimated by the magnetization of the needle placed inside it, which was determined by the number of times the needle oscillated in a minute when suspended by a silk fibre. A new needle was, of course, used for each experiment.

If now the secondary coils be arranged according to the scheme denoted by ABCDEF, then in whatever part of the secondary circuit the magnetizing-spiral be placed, no appreciable magnetization of the needle occurs. If, however, the secondaries were connected up according to the scheme AB—DCEF, then a needle which had been inside the magnetizing-spiral vibrated in one case $29\frac{1}{2}$ times a minute, in another 28. If the circuit was closed by joining the ends A and F, then a needle placed in the magnetizing-spiral vibrated 50 times a minute; so that the magnetism produced in the open circuit is quite comparable with that produced in the closed. In some experiments to be described later the magnetism produced in the open circuit was actually greater than the magnetism produced in the same circuit when closed. If the coils were arranged according to the scheme ABDCEF—, the order of the coils being the same as before, but the magnetizing-spiral being put at the free end of the secondary instead of in the middle, then a needle placed inside the magnetizing-spiral vibrated only 3 times a minute. If, however, every thing be kept the same as before, except that the ends A and F are connected with the plates of a condenser of about half a microfarad capacity, the magnetizing-spiral being between the end F and a plate of the condenser, then a needle, after being placed inside the magnetizing-spiral, vibrates 41 times per minute.

These experiments show that in some cases there is a considerable transference of electricity along some parts of the coil, even although it be open. This is due to the fact that the coil acts like a condenser; and the difference between the two cases mentioned above can be easily explained. For if we proceed along the secondary coil, the potential produced by the break of the primary circuit will increase continuously if we always go round the coil in the same direction. Now when the coils are arranged according to the scheme ABCDEF, two neighbouring windings of the coil can differ at most in their potentials by the rise in potential due to one layer of the wire; but when the coils are arranged according to the scheme AB—DCEF, the last layer of the first coil differs from the first layer of the second by the rise in potential due to the whole length of the second coil, or by the rise due to about 100 yards of wire. Thus in this case we have two places of very different potential near together; in other words, we have the condition necessary for a condenser of large capacity. The consequence of the coil possessing this electrostatic capacity, is that when the electromotive force produced by the breaking of the current on the primary acts upon it, the electricity is distributed in lumps, as it were, along the coil, being lumped up in the places of great electrostatic capacity. In consequence of this irregular distribution of the electricity, electrical oscillations, which will last until destroyed by the resistance of the wire of the coil, are set up, just as in a canal, if the water be heaped up in some places and depressed in others, the water does not sink at once to rest, but oscillates until the oscillations are destroyed by friction. The motion of the electricity during these electrical vibrations will constitute a current through the coil which will magnetize the needle.

The fact that the magnetism produced in a needle placed in the magnetizing-spiral is different when the spiral is placed in different parts of the secondary circuit, the arrangement of coils in the secondary remaining the same, shows that the current is not the same all along the secondary. If this be the case, the electricity must accumulate on the wire of the coil; and consequently the coil must act as a condenser. This was strikingly shown in the experiments referred to above, when, with the coils arranged according to the scheme AB—DCEF, the needle vibrated 29 times per minute; whilst with the arrangement ABDCEF— it only vibrated 3 times per minute. It is also well shown in the following case: with the coils arranged according to the scheme AB—DCEF, 29 vibrations per minute were produced; but if the magnetizing-spiral was placed between C and E instead of B and D, so that the arrangement was that denoted by ABDC—EF, only 15 vibrations per minute were produced.

It is not necessary in these experiments to have three secondary coils; many of the effects can be produced with two, as in the following experiments. The ends E and F of the third coil were joined, so that it was altogether out of the secondary circuit; then, when the remaining coils were arranged according to the scheme AB—DC, a needle placed in the magnetizing-spiral vibrated only 7 times a minute; whilst if the coils were arranged according to the scheme BA—DC, a needle vibrated 36 times per minute. This difference can be explained in the same way as the corresponding one in the case of three coils.

A very marked difference between the behaviour of closed and open circuits is shown if, instead of breaking the primary circuit by a key in the ordinary way, we connect the points where the circuit would be broken with the opposite plates of a condenser of large capacity, so that at the break, instead of a spark passing, we have the electricity rushing into and charging the condenser. With a closed secondary this has the effect of slightly increasing the intensity of magnetization of a needle placed in the magnetizing-spiral (see Lord Rayleigh's paper "On an Electromagnetic Experiment," Phil. Mag. June 1870); but with an open secondary circuit it has in general a most marked effect in the opposite direction, in some cases, indeed, appearing to produce hardly any magnetization at all. This is shown in the following statement of the result of some experiments:—

Scheme of coils.	Number of vibrations per minute with condenser in primary circuit.	Number of vibrations without condenser in the primary.
ABFE—DC	5	28
BA—DC	6	36
BA—DC	4	33

In the last experiments the coil EF did not form a part of the secondary circuit.

The effect is thus very marked. An exception to this, however, occurs when, instead of using the electrostatic capacity of the coil itself to produce the magnetization of the needle, we arrange the coil so that its electrostatic capacity is small, but increase it artificially by connecting the free ends of the coil with the opposite plates of a condenser. Under these circumstances the introduction of a condenser into the primary does not produce nearly so much difference in the magnetization of the needle. This is shown by the following experiments, in which the secondary coil was arranged according to the scheme ABCDEF, which gives the least electrostatic capacity to the coil; but the ends A and F were connected with the opposite plates of a condenser whose capacity was about one third of a microfarad. The magnetizing-spiral was placed between A and one of the plates of the condenser.

Number of vibrations per minute with condenser in primary.	Number without.
44	47
48	52
43	53

So that in this case the effect of putting a condenser in the primary circuit was not nearly so great as when the magnetization of the needle was due to the electrostatic capacity of the coil itself. When the condenser in the secondary circuit was taken away and the secondary circuit closed, a needle placed in the magnetizing-spiral vibrated 23 times per minute, and it was magnetized in the opposite way to what it was when the secondary circuit was open: this, indeed, was found to be a general rule; the magnetization of the needle produced by any arrangement of the secondary coils when open was always of the opposite character to that produced when the same circuit was closed. In the experiment just described it will be seen that the needle vibrated about twice as fast when the secondary was open as it did when the secondary was closed, which shows that in this case the intensity of magnetization was about four times greater with an open circuit than with a closed one.

For the explanation of these results we must go, I think, to the equations giving the motion of the electricity in the coil.

If L be the coefficient of self-induction of a coil, C the capacity of a condenser to which its ends are attached, then the kinetic energy of the coil when a current $\dot{x}$ is going through it is $\frac{1}{2}L\dot{x}^2$, the potential energy is $\frac{1}{2}\frac{x^2}{C}$; so if we neglect the resistance of the coil, as we may do if the time-constant of the coil be large compared with the time of electrical vibrations in the coil (for then the effects of resistance will be inappreciable during the first few electrical oscillations in the coil), the equation of motion is

$$Lx'' + \frac{x}{C} = 0,$$

or the period of electrical oscillation $= 2\pi\sqrt{LC}$. Now if an electromotive force whose period is $\frac{2\pi}{p}$, which we may represent by $F \sin pt$, acts on the wire of the coil, the equation satisfied by the current is

$$Lx'' + \frac{x}{C} = F \sin pt,$$

or

$$x=\frac{\frac{F}{L}\sin pt}{\frac{1}{LC}-p^2}.$$

Now, if $\frac{1}{LC}$ be very much greater than p^2, this will be very much less than $\frac{-F\sin pt}{Lp^2}$, the expression for x when there is no condenser in the circuit, and will be of the opposite sign; so that the quantity of electricity travelling along the wire of the coil at any time is very much less in this case than when the circuit is closed, and also the electricity moving along the wire at any time is of opposite signs in the two cases. These remarks will apply, I think, to the experiments last mentioned. When we connect the breaking-points of the primary circuit with the plates of a condenser, we produce an electrical oscillation in the primary circuit whose period would be $2\pi\sqrt{L_1C_1}$ if the secondary coil were not present, L_1 being the coefficient of self-induction of the primary, and C_1 the capacity of the condenser to the plates of which the breaking-points of the primary are attached. These electrical oscillations in the primary will give rise to an electromotive force in the secondary which we may express by $F\sin\frac{t}{\sqrt{L_1C_1}}$; but the secondary circuit itself possesses both self-induction and electrostatic capacity, and thus can be the seat of electrical oscillations whose free period would be $2\pi\sqrt{L_2C_2}$, L_2 being the coefficient of self-induction of a current in the secondary coil, and C_2 its electrostatic capacity. Now, since the currents in the secondary are local, L_2 will not be the coefficient of self-induction of the whole secondary circuit, but probably of only a comparatively small part of it, so that L_2 will be much less than L_1; and as the condenser in the primary is of large capacity and C_2 is only the electrostatic capacity of the coil itself, C_2 will in all probability be much less than C_1. Hence $2\pi\sqrt{L_2C_2}$ will be very much less than $2\pi\sqrt{L_1C_1}$, or the natural period of electrical vibrations in the secondary is very much less than the period of the electromotive force acting upon it. But the investigation we have just gone through shows that in this case the amount of electricity passing along the wire is very much less than it would be if the secondary were closed, and that the electricity passing at any time is of opposite signs in the two cases. This explains the great decrease in the magnetization of the needle, and the magnetization being of opposite sign to that produced when the secondary circuit is closed.

When, however, the coil is arranged according to the scheme ABCDEF, and the ends A and F are connected with the plates of a condenser of large capacity, so that now the coil itself possesses little or no electrostatic capacity, then the currents will evidently be much less local than before, and will probably be pretty uniform throughout the coil. Thus L_2, the coefficient of self-induction of the secondary circuit, will be greater than it was, and will probably not differ very much from L_1, the coefficient of self-induction of the primary circuit, as the coils are of approximately equal length; and since the capacity of the condenser in the secondary is large, C_2 will be greater than it was in the former case, when it was the electrostatic capacity of the coil itself. Thus both L_2 and C_2 are larger than they were before, and consequently the free period of electrical vibration in the secondary circuit will be much nearer the period of the electromotive force; thus the denominator in the expression for the current $\dot{x}$ will be much smaller than it was, or the current will be much greater. This agrees with the experiment last described, which showed that the diminution in the magnetization of the needle due to the introduction of a condenser into the primary was much less in this case than it was before. When there is no condenser in the primary, the equations to determine the initial currents are much the same, whether the secondary circuit be open or closed, the only difference being that when the secondary is open, as the currents are local and not the same all along the coil, the coefficients of self-induction and mutual induction will be less when the secondary is open than when it is closed; and as Lord Rayleigh has proved that in a secondary with a few windings of wire, a needle is magnetized more strongly than in one of more windings, we should expect, if there were only one of these local currents, that a needle placed in it would be magnetized more strongly than when the circuit was closed. The existence of several of these local currents complicates the question, and reduces, of course, the intensity of each of them. In some cases, however, the reduction of the intensity from this cause is not sufficient to counterbalance the increase due to the other cause, as in the case last described, when the needle vibrated twice as quickly after being placed in the open circuit as it did after being placed in the closed. It must be remembered, too, that when the secondary is open, the currents are oscillatory, and so are first in one direction and then in the opposite; this must have the effect of diminishing their magnetizing-power, though we do not know enough about the action on magnets of currents first in one direction and then the opposite to say how much.

The results obtained when the condenser was put in the primary appear to have important bearings on the theory of Ruhmkorff's coil. The secondary of a Ruhmkorff's coil will no doubt possess very considerable electrostatic capacity; this will, as we have seen, give rise to local currents, which exist although the secondary be closed. Now these local currents will interfere with the main current, and will fritter away energy. The experiments just described show that the introduction of a condenser into the primary circuit will diminish very materially the intensity of these local currents, and so cause much less energy to be frittered away. I therefore think it probable that part of the cause of the superior efficiency of a coil provided with a condenser in the primary is due to this effect of the condenser.

Some points of interest arise when we keep EF out of the secondary circuit, still keeping the magnetizing-spiral in the secondary. Thus, the intensity of magnetization of a needle placed in the magnetizing-spiral is found to be different, according as the coil EF is closed, has its ends free, or has them connected with the plates of a condenser.

We shall begin with the case when the secondary ABCD is closed. Then with EF closed, a needle vibrated 24 times a minute; with EF open, 44 times; and with the ends E and F connected with the plates of a condenser, 43 times. In another experiment a needle vibrated 23 times with EF closed, 33 times when EF was open, and 36 times when E and F were connected with the plates of a condenser. It is easy to see that the magnetization should be greater when the coil EF is open than when it is closed; for when it is closed, an induced current is formed in it, which, since the electrokinetic momentum of the coils is the same after the break as it was before, must be at the expense of the current in the secondary, and thus the current in the secondary is diminished. But it is not so evident, *à priori*, why joining the ends E and F to the plates of a condenser does not make the coil EF behave almost as if it were closed; we can, however, I think, find an explanation by going to the dynamical equations.

Let L denote the coefficient of mutual induction between the primary and secondary circuits, M the coefficient of self-induction of the secondary, and N the coefficient of mutual induction between the secondary and the coil EF; then if i, j, k denote the currents through the primary, secondary, and the coil EF respectively, the dynamical equation is

$$\frac{d}{dt}(\mathrm{L}i+\mathrm{M}j+\mathrm{N}k)=0.$$

Integrating this equation from before the break till after, we get, if there be no condenser in the primary,

$$\mathrm{M}j+\mathrm{N}k=\mathrm{L}\gamma,$$

where γ is the original current through the primary circuit;

$$\therefore\ j=\frac{\mathrm{L}\gamma-\mathrm{N}k}{\mathrm{M}}.$$

Now, if the coil EF be open, k will be zero, or, at any rate, very small; for though from its electrostatic capacity some current may exist in EF, yet, from the way EF is wound, its electrostatic capacity must be small, so that approximately

$$j=\frac{\mathrm{L}\gamma}{\mathrm{M}}.$$

Now, when the coil EF is closed,

$$j=\frac{\mathrm{L}\gamma-\mathrm{N}k}{\mathrm{M}},$$

where k is the current in the coil EF which is quite comparable with γ; so that in this case j is much less than when the coil EF was open; but if the coil EF has its extremities connected with the plate of a condenser, then, again

$$j=\frac{\mathrm{L}\gamma-\mathrm{N}k}{\mathrm{M}}.$$

Now in this case the current in the coil EF will be oscillatory, so that k may have any values, positive or negative, within certain limits. Thus, although the initial value of j is perhaps not much greater than when the coil EF is closed, yet after k has completed a second oscillation and attained its maximum negative value, j will then not only be greater than when the circuit EF is closed, but will be even greater than when it is open. These theoretical conclusions agree with the experiments mentioned above, which are examples taken at random from a great number of experiments.

If the secondary coil be open instead of closed, the phenomena are by no means the same. It was found, under these circumstances, that the magnetization of a needle placed in the secondary was much less intense when the coil EF was open than when it was closed, which is exactly opposite to what took place when the secondary was closed. The following results, taken at random from many experiments, will show how clearly this effect is marked.

First Experiment.

Scheme of coils.	Number of vibrations of needle per minute.	State of coil EF.
BA—DC	36	Ends E and F to plates of condenser.
BA—DC	21	Ends free.
BA—DC	34	Circuit closed.

Second Experiment.

BA—DC	35	Ends E and F to plates of condenser.
BA—DC	17	Ends free.
BA—DC	33	Circuit closed.

Third Experiment.

AB—DC	40	Ends to plates of condenser.
AB—DC	12	Ends free.
AB—DC	35	Circuit closed.

The following working may perhaps throw some light on these results.

If we have two coils, (1) and (2), with ends connected with the plates of two condensers whose capacities are C_1 and C_2 respectively, and if L and M be the coefficients of self-induction of the coils (1) and (2), and N the coefficient of mutual induction between them, then, if x and y are the quantities of electricity that are in the condensers at any time, the equations satisfied by x and y are

$$L\frac{d^2x}{dt^2}+N\frac{d^2y}{dt^2}+\frac{x}{C_1}=0,$$

$$N\frac{d^2x}{dt^2}+M\frac{d^2y}{dt^2}+\frac{y}{C_2}=0;$$

or if

$$x=A\sin pt,$$

$$y=B\sin pt,$$

then

$$A\left(\frac{1}{C_1}-Lp^2\right)-BNp^2=0,$$

$$-ANp^2+B\left(\frac{1}{C_2}-Mp^2\right)=0.$$

Eliminating A and B, we get

$$p^4-\frac{p^2}{LM-N^2}\left(\frac{L}{C_2}+\frac{M}{C_1}\right)+\frac{1}{(LM-N^2)C_1C_2}=0.$$

If the roots of this equation are p_1 and p_2, then

$$x = \mathrm{A}_1 \sin p_1 t + \mathrm{A}_2 \sin p_2 t,$$

$$y = \frac{\mathrm{A}_1\left(\frac{1}{\mathrm{C}_1} - \mathrm{L}p_1^2\right)}{\mathrm{N}p_1^2} \sin p_1 t + \frac{\mathrm{A}_2\left(\frac{1}{\mathrm{C}_1} - \mathrm{L}p_2^2\right)}{\mathrm{N}p_2^2} \sin p_2 t,$$

or, as it will be more convenient to write them,

$$\dot{x} = \mathrm{A}' \cos p_1 t + \mathrm{A}'' \cos p_2 t,$$

$$\dot{y} = \frac{\mathrm{A}'\left(\frac{1}{\mathrm{C}_1} - \mathrm{L}p_1^2\right)}{\mathrm{N}p_1^2} \cos p_1 t + \frac{\mathrm{A}''\left(\frac{1}{\mathrm{C}_1} - \mathrm{L}p_2^2\right)}{\mathrm{N}p_2^2} \cos p_2 t.$$

Now, if i and j are the initial currents in the coils (1) and (2) respectively, and if they are produced by breaking a primary circuit through which a current j flowed, i and j will be determined by the equations

$$\mathrm{L}i + \mathrm{N}j = \mathrm{F}\gamma,$$
$$\mathrm{N}i + \mathrm{M}j = \mathrm{G}\gamma,$$

where F and G are the coefficients of mutual induction between the primary and the coils (1) and (2) respectively. Thus i and j are independent of the capacities of the condensers in the circuit.

A′ and B′ are given by the equations

$$i = \mathrm{A}' + \mathrm{A}'',$$

$$j = \frac{\mathrm{A}'}{\mathrm{C}_1\mathrm{N}p_1^2} + \frac{\mathrm{A}''}{\mathrm{C}_1\mathrm{N}p_{//}^{\ 2}} - \frac{\mathrm{L}}{\mathrm{N}}(\mathrm{A}' + \mathrm{A}'');$$

or

$$(\mathrm{N}j + \mathrm{L}i)\mathrm{C}_1 = \frac{\mathrm{A}_1}{p_1^2} + \frac{\mathrm{A}_{//}}{p_2^2}.$$

$$\mathrm{C}_1\mathrm{F}j = \frac{\mathrm{A}_1}{p_1^2} + \frac{\mathrm{A}_{//}}{p_2^2};$$

$$\therefore \frac{i}{p_1^2} - \mathrm{C}_1\mathrm{F}j = \mathrm{A}''\left(\frac{1}{p_1^2} - \frac{1}{p_{//}^{\ 2}}\right),$$

$$\frac{i}{p_2^2} - \mathrm{C}_1\mathrm{F}\gamma = \mathrm{A}'\left(\frac{1}{p_2^2} - \frac{1}{p_1^2}\right).$$

Now, if p_2 be $> p_1$, then A″ is $>$ A′; so that in $\dot{x}$ the term of shortest period has the largest coefficient.

If the circuit (2) be closed, then

$$\dot{x} = i \cos pt,$$

$$\dot{y} = \frac{\mathrm{G}\gamma}{\mathrm{M}} + \left(j - \frac{\mathrm{G}\gamma}{\mathrm{M}}\right) \cos pt;$$

where

$$p^2 = \frac{M}{C_1(LM - N^2)}.$$

The equation to determine p in the most general case was

$$p^4 - \frac{p^2}{LM - N^2}\left(\frac{L}{C_2} + \frac{M}{C_1}\right) + \frac{1}{C_1C_2(LM - N)^2} = 0.$$

It is evident that this equation in p^2 has one root greater and one root less than $\frac{M}{C_1(LM - N^2)}$, the value when the circuit (2) was closed; and it is evident from the equation that if we diminish C_2, the capacity of the condenser, (2) we increase one, at any rate, of the roots of this equation.

Now, if the process of magnetization requires a time at all comparable with the period of an electrical vibration in the coil, so that we might expect the magnetization produced by an oscillating current to get less as the time of oscillation approached the time required for magnetization, then the effect of diminishing the capacity of the condenser in the coil (2) would be to diminish the magnetization produced in coil (1). For the effect of diminishing C_2 was, as we saw, to shorten the shortest of the periods of vibration, which, if the above hypothesis be right, would have the effect of diminishing the magnetization due to this part of the current $\dot{x}$; and as we proved the coefficient of the term of shortest period in $\dot{x}$ was the greater, the greater part of the energy would be due to this term. Thus the effect would be to diminish the magnetizing-power of the coil.

This corresponds to the experiments described if the coil (1) refer to the secondary circuit, and the coil (2) to the coil EF. When the coil EF has its ends attached to the plates of a condenser of large capacity, C_2 is very large; but when it is open, C_2 is equal to the electrostatic capacity of the coil itself, which will probably be very small comparatively. Thus the effect of disconnecting the ends of EF from the condenser and leaving them free, is to decrease very greatly the value of C_2. This, according to the reasoning just given, ought to diminish the magnetizing-power of the secondary: a reference to the result of the experiments will show that it actually did so to a very large extent. A similar explanation will explain why the magnetization ought to be greater when EF is closed than when it is open.

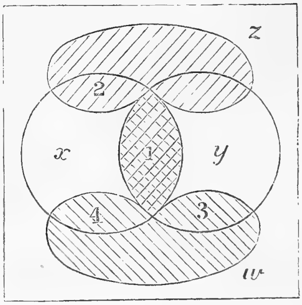
The above experiments were made in the Cavendish Laboratory, Cambridge.

VII. *Notices respecting New Books.*

Symbolic Logic. By JOHN VENN, *M.A., Fellow and Lecturer in the Moral Sciences, Gonville and Caius College, Cambridge.* London: Macmillan and Co.

MR. VENN'S work forms an important contribution to the Modern Logic, and is destined to give a great impetus to the study and development of that science.

Fig. 1.



One excellent feature of the work is the amount of information it contains about the results reached by the early mathematical logicians. These logicians flourished before logic had been divorced from mathematics; and some of them had got a good way towards establishing the science on an analytical basis—Lambert in particular. Indeed, he appears to me to be quite sound on a nice point with respect to which Mr. Venn is inclined to think him not free from error (p. 272). The question is as follows:—Given $xy=zw$, where each of the symbols denotes a class of things, is it correct to conclude that $\frac{x}{z}=\frac{w}{y}$? I illustrate the question by the accompanying diagram (fig. 1). Suppose we restrict our attention to the part of the page within the square: let x denote the part included in the one circle, y the part included in the other circle, z the part stroked downwards from right to left, w the part stroked downwards from left to right. Here we have $xy=zw$; and the diagram is quite general in every other respect. The principle upon which I should proceed is as follows: $\frac{x}{z}=\frac{w}{y}$ is a legitimate deduction, because the symbols are commutative in xy and in zw; and the meaning of $\frac{x}{z}$ and of $\frac{w}{y}$ must be fixed so as to suit this deduction. By the Boolian process of development,

$$\frac{x}{z}=xz+\frac{1}{0}x(1-z)+\frac{0}{1}(1-x)z+\frac{0}{0}(1-x)(1-z);$$

and

$$\frac{w}{y}=wy+\frac{1}{0}w(1-y)+\frac{0}{1}(1-w)y+\frac{0}{0}(1-w)(1-y).$$

The coefficients direct us to include the first term, to exclude the third term, and (we will assume) to take a part of the fourth term. Looking at the diagram, we then find that $\frac{x}{z}=\frac{w}{y}$ may be taken to mean

$$(1+2)+3+\frac{1}{0}x(1-z)=(1+3)+2+\frac{1}{0}w(1-y);$$

hence

$$\frac{1}{0}x(1-z)=\frac{1}{0}w(1-y),$$

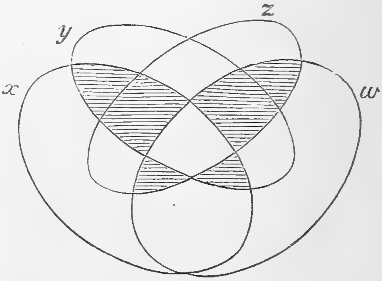
which appears to mean

$$4=4.$$

The most general meaning of $\frac{x}{z}$ cannot be considered to be a class which, on restriction by z, produces x; that is its meaning only when $x(1-z)=0$. It appears that we are entitled to equate the sum of the first and fourth terms on the one side to the sum of the first and fourth terms on the other side, and the second term to the second. But we are certainly not entitled to equate, as Mr. Venn thinks we are, the first to the first, and the fourth to the fourth. For let $x(1-z)=0$ and $w(1-y)=0$, then the state of affairs is still represented by $(1+2)+3=(1+3)+2$.

Another excellent feature of the work, and the one which appears to me to display the greatest amount of originality, is the invention of appropriate diagrams to represent the classes into which the whole is broken up when a given number of attributes are taken into consideration. Intersecting circles suffice in the case of two or three attributes; but when the number is increased to four, we encounter the difficulty of making four closed plane figures intersect so that each of the resulting classes is continuous. Mr. Venn succeeds, by means of ellipses, in drawing figures which make the final classes continuous, though some of the intermediate classes are detached. He has also shown how to apply these diagrams to express data, by shading the classes which are given to be non-existent. A paper describing this method appeared in the Philosophical Magazine for July 1880. To investigate the above problem, the method gives us the four-ellipse diagram (fig. 2); and the condition expressed in the equation $xy=zw$ is expressed by the shading out represented. It indicates the same conclusion as the other figure.

Fig. 2.



A difficulty arises in the application of this process of shading-out in the case of the contrary class (pp. 271, 281), owing to the fact that Mr. Venn does not represent the whole class of things considered by an enclosure such as the square in fig. 1. Not only does the shading-out process require this, but the logical-diagram machine insists upon it (p. 122). This point is intimately connected with the view taken of the *Universe of Discourse*, to the discussion of which our author devotes a chapter. Mr. Venn does not give an exact definition of the meaning he assigns to this term; but from pp. 128 and 185 I gather that he considers it formed by setting limits to the sum-total of logical existence. The conception of a universe is held to be, strictly speaking, extra-logical,—a question of the application of our formulæ, not of their symbolic statement; and on the symbolic system it is said to be universally admitted that the distinction between subject and predicate is lost. "We may extend our universe so as to embrace the sum-total of logical existence, in which case European includes things other than men, and not-European includes the unlimited myriads of entities which people that heterogeneous domain." What I object to is, having any thing to do with the sum-total of logical existence or any limited portion of it: I object to considering any such heterogeneous subject. The term *universe* may be used to denote a more exact and more useful idea, which is really the idea required to make Boole's processes intelligible. It may be used to denote a collection of things all of the same type, such as is denoted by a substantive. We hold that the logical function of an adjective is different from that of a substantive, and so different that the distinctive cannot be lost sight of in any exact symbolic system. The logical function of an adjective is to express an attribute—in the language of the grammarians, to qualify a substantive. "Black" has a certain meaning in itself; but it can no more denote a class of things before being applied to a particular type than can a number such as 2 denote a quantity before being applied to some unit. But Mr. Venn says (p. 181) that "we all know what is in most cases to be included under the name 'black.'" I reply that we know only, provided we are given the logical type to which the attribute refers. Otherwise the supposed class must include black men, black berries, black birds, black coats, blacking, &c.,—and not only the black men, but the black hairs on the heads of the black men, their black skins, but, in general, not their teeth—and not only the black hairs, but the black particles of each black hair—and so on *ad infinitum*. Here, at all events, there is danger of counting twice, and of having some impossible subtractions to perform. Man, berry, bird, coat, particle of blacking, hair, skin, tooth, each denotes a kind of logical unit, to any one of which *black* may be applied as a qualification. This conception of a universe, instead of being extra-logical, seems to me to be one of the fundamental ideas of the science.

'Symbolic Logic' possesses a feature which could scarcely be anticipated in a work coming from so great an authority on the Theory of Probability, namely an aversion to all quantitative considerations. "So far from being fairly open to the charge of being too numerical, we are really more open to that of being almost prudishly averse to being seen or thought to enumerate, as will be found when we discuss the treatment of particular propositions" (p. xxii). Turning to p. 169, we find:—"To exclude them (particular propositions) from our rules would only be a slightly greater encroachment upon the full freedom of popular speech than has already been brought about by the exclusion of such terms as 'many,' 'most,' and others of a somewhat quantitative character." Mr. Venn consequently restricts himself generally to what may be called universal propositions. But suppose that we have the data $xy=20$ and $x=20$; from these numerical propositions, though each is despicable in itself, we obtain, when combined, the important universal proposition $x=xy$. To exclude number is to draw a very arbitrary line. Connected with this is the standpoint involved in the title 'Symbolic Logic,' a standpoint with which as it is explained in the work we are not wholly satisfied. It appears to us that Boole's work, properly understood, leads rather to the conception of an Algebra of Logic; not an Algebra of 0 and 1, as Boole understood it, but an Algebra in which an elementary symbol may have any value between 0 and 1. In every properly expressed equation the quantitative value of the one member is equal to the quantitative value of the other member. The identity of two groups of the same kind of logical unit surely involves equality in the numbers of those groups. Thus viewed, the science is every whit as quantitative as ordinary Algebra.

Chapter XII. contains a series of original problems which are used to illustrate the subject. The various neat methods employed there and throughout the work lead us to the conception, not of a limited *system* (a word which is a fallacy in itself), but to the conception of a science which is limited only in the sense that the portion which has been made actual knowledge is limited.

A. MACFARLANE.

An Introduction to Logic. By W. H. S. MONCK, *M.A., Professor of Moral Philosophy in the University of Dublin.* Dublin: Hodges, Foster, and Figgis, Grafton Street. 1880.

Professor Monck's work consists of two parts—the one devoted to an elementary exposition of the subject, the other to the discussion of questions more suitable to the advanced student,—the two together forming an excellent manual on the Aristotelian Logic. But while Prof. Monck describes with exactness and precision the whole province he has traversed, we cannot but consider that province artificially limited. "The fact is that Logic does not *draw* inferences at all, but only lays down rules for drawing them" (p. x). In accordance with this, Logic cannot help us to conclude from "John is the *father* of Thomas" that "Thomas is the *son* of John;" but suppose we know that "If John is the father of Thomas, Thomas is the son of John," and that "John is the father of Thomas," then Logic can assure us that Thomas is the son of John. In accordance with this theory of the function of Logic, we have (at p. 70), "An argument of this form—three-fourths of the army were killed; three-fourths of the army were Prussians; therefore some of the Prussians were killed—is logically incomplete; and to complete it we must state in terms that any two fractions of the same whole, each amounting to three-fourths (or to over one-half) must have a common part,—a truth so obvious that few persons would think it necessary to state it, but which nevertheless is taught by arithmetic, not by logic." If the science of Logic must hand over so simple a matter as this to Arithmetic, it cannot be surprising to any one that every book on Logic devotes several pages to discussing the time-honoured question of the utility of what it has to offer to the reader. But Prof. Monck is perhaps not aware that if he hands over the proof of this piece of reasoning about the killed Prussians to the algebraist, he hands over along with it the proof of every syllogistic mood. This was shown by De Morgan; but the proof has been given in a more complete form in Macfarlane's 'Algebra of Logic,' where the validity of each mood is proved from the principle which suffices to prove the above piece of reasoning, and the necessary reductions required to bring a given mood under that principle are shown to correspond to the Aristotelian process of Reduction.

In one of his advanced chapters Prof. Monck deals very effectively with Analytical and Synthetical judgments; and at page 134 we have some remarks on the difference in function of the subject and the predicate which are worthy of the attention of symbolic logicians.

Six Lectures on Physical Geography. By the Rev. S. Haughton, *F.R.S., M.D. Dubl., D.C.L. Oxon.* Dublin: Hodges, Foster, and Co. London: Longmans and Co. 1880.

Physical Geography comprises so great a variety of subjects, that it is somewhat difficult to compress in a moderate compass the leading facts and principles of the science. Being generally taught in schools and colleges, and forming one of the subjects of science examinations, many text-books have been prepared, varying in size and character according to the views entertained by the different authors as to the extent and bearing of Physical Geography. To these may now be added the above treatise by Prof. Haughton; although recently published, it is chiefly based on a short course of lectures given five years ago, the substance of which, carefully revised and considerably enlarged, with numerous notes, maps, and diagrams, forms the present volume.

The first two lectures treat of the past history (including the nebular hypothesis of Laplace) and future prospects of the Earth as inferred from the phases which the Moon, Mars, and Venus have undergone or are now passing through,—and also of the origin, form, and distribution of continents, oceans, and mountain-chains, as well as the phenomena exhibited by volcanos as connected with the contraction of the earth's crust due to secular cooling.

In the third lecture, besides the law of atmospheric and oceanic circulation, the author treats of the scale of geological time during the Azoic, Palæozoic, and Neozoic periods, and shows, from calculations given (pp. 90–92), that by comparing the percentages of the maximum thicknesses of the stratified rocks with the percentages of time found from the theory of a cooling globe, there is a remarkable agreement, and strongly justifies the principle held by many geologists, that the proper relative measure of geological periods is the maximum thickness of the strata formed during those periods.

The fourth and fifth lectures are devoted to the distribution and origin of the rivers and lakes of Europe, Asia, S. America, and Africa, including details as to their drainage-areas, rate of denudation of the different rain-basins, and matter discharged by the rivers. Orographical maps of the two latter continents are given, in order to show the great superiority of S. America over Africa in the manufacture of rivers, as fully explained (pp. 204–243).

The geographical distribution of animals and plants forms the subject of the concluding lecture, in which the author gives an outline of the more important facts, and the attempts that have been made to explain them. The groups of animals selected for illustration are the King-Crabs, Ostrich family, non-placental Mammals, the Edentates, Humming-birds, and the Monkeys and Apes. The distribution of the subdivisions of the above groups is given, as also of their fossil ancestors as bearing on the origin of the present forms.

The zoological regions are adopted from Mr. Wallace, but with different names, viz. the Europasian, N. and S. American, African, Australian. These five natural regions are related to the ancient land-systems of the globe defined by the meridian axes of elevation, of which there are two in the northern and three in the southern hemisphere, as described by the author (pp. 20, 29), and which date back as far as the earliest sedimentary deposits, modified, as in the case of Europasia, by an east-and-west small-circle chain, which is only of Tertiary origin. The characteristic animals of these different zoological provinces are given, and their relation to the preglacial fauna of the same regions.

To the above five regions an Oriental, or supplemental one is added, as it does not owe its origin or fauna to any of the primary continents of the northern or southern hemisphere, but seems to have been supplied by immigration. It contains all tropical Asia east of the Indus, with the Malay islands as far as Java, Borneo, and the Philippines.

In the geographical distribution of plants the author follows Bentham, who recognizes three great floras, the Northern, Southern, and the Equatorial Tropical. Details of the characteristic forms of these floras are given; but with regard to the entire flora of the globe having one origin only, as believed by some botanists, he considers it to have had two origins, from both poles (pp. 288, 313). A comparison of the present and Miocene floras and climates of the Arctic regions concludes this lecture (pp. 313–345). From the facts stated, it is inferred:—

1. The Miocene July temperature of Grinnell-land, Spitzbergen, was 28° F. higher than at present.

2. The Gulf-stream, which is *now* the cause of the difference

between the climates of the above places, occupied in *Miocene times* the same position as at present, and performed the same friendly function to Spitzbergen.

3. The Atlantic Ocean occupied its present position; and the *impossible* bridge between Greenland and Europe did not exist.

Climate in past and present times, and its influence on the distribution of animal life, is frequently referred to; and the deductions on the causes of geological climates deserve a careful, if not a critical, consideration. From a series of calculations on the effects of sun-heat, earth-heat, and atmospheric conditions as the sole causes respectively of changes in geological climates, the author gives the following probable conclusions:—

1. We must reject any solution based upon a change of position, either in space or within the earth's body, of the axis of rotation, within the limits of geological time.
2. We must reject any solution based upon the secular cooling of the earth (with a fixed axis of rotation), regarded as the sole and immediate cause of the change of climate.
3. The chief factor in changes of Geological Climate appears to have been the slow secular cooling of the sun, in consequence of which the earth's surface cooled gradually down.

Generally speaking, the broad facts and leading principles of Physical Geography, and their explanations, are differently treated than in many other manuals on the subject.

Practical Plane Geometry. By JOHN WILLIAM PALLISER.
Simpkin, Marshall, and Co. 1881.

This is an excellent treatise on Constructive Geometry, and far more comprehensive than most elementary books on the same subject. It treats of the straight line, angles, circles, proportional parts, equivalent figures, the generation of various curves (not often introduced into even more pretentious books), and the construction of *scales*. It is well adapted to the requirements of pupils preparing for examination at South Kensington, and the Preliminary examinations for entrance to Sandhurst and Woolwich. The constructions are well drawn, and the directions for reproducing the various problems clear and sufficient. We may reasonably make exception to fig. 6 (p. 7); for the points G and H are not mathematically determined. There is, too, a much simpler method of inscribing a square in any triangle than the one given on p. 12, though the latter is perfectly sound and demonstrable. In speaking of *Representative fractions* of plans (p. 59), the author says:—"thus $\frac{1}{12}$ attached to a plan would show that the drawing is *one-twelfth* the real size." If the word *drawing* be taken to apply to the plan or map as a whole, and not to a given straight line in it, it would do nothing of the kind*. Other equally vague expressions occur in various parts of the book. The introduction to Elementary Solid Geometry might, we venture to think, be omitted without in any way detracting from the value of the book.

* On the same page $365\frac{1}{2}$ ought to be $36\times 5\frac{1}{2}$.

Natural Philosophy, for general readers and young persons. Translated and Edited from Ganot's Cours Elémentaire de Physique *(with the Author's sanction) by* E. ATKINSON, *Ph.D., F.C.S., Professor of Experimental Science in the Staff College.* Fourth Edition. London: Longman, Green, and Co. 1881.

It may be safely asserted that no book on Physics has had a more deserved popularity during the past fifteen years among teachers and pupils than Dr. Atkinson's larger work, now in the tenth edition. And the reason for this is very obvious: basing the work on Ganot's well-known treatise, Dr. Atkinson, by well-chosen additions and amplifications of his own, strove to produce a good book, and abundantly succeeded. Improvements on old methods, and new discoveries, have in like manner found places from time to time in Dr. Atkinson's excellent book.

In the work now before us Dr. Atkinson is wisely pursuing the same plan. Thus, in art. 516 we find a lucid description of the Telephone and its applications in the transmission of sound. Reis in 1862 made the first successful experiment in transmitting musical sound some distance by aid of electricity. Simple as his apparatus was, that used by Professor Graham Bell is still simpler; for he dispenses with the electrical battery, and substitutes a small steel magnet.

The subject-matter of the book is greatly elucidated by well-executed woodcuts, of which there are no fewer than 471; some few of which, we venture to suggest, might be dispensed with without in any way detracting from the value of the book.

An Appendix furnishes upwards of 300 questions on the properties of matter, liquids, gases, sound, heat, light, magnetism, and electricities. Many of these are easy common-sense questions; whilst others can only be answered after a thoughtful perusal of the various subjects. They are all very suggestive to both teachers and pupils; and we commend them to the former as eminently adapted to elementary class-work.

Textbook of Systematic Mineralogy. By HILARY BAUERMAN, *F.G.S.* Longmans, Green, and Co. 1881.

As in most modern treatises on Mineralogy, a very large portion of this work is taken up by crystallography. In this part of the volume there is some amount of original treatment, while full use has been made of the existing literature on the subject, from Haüy to Mallard inclusive. It has been brought well up to date, and contains some things which have not usually found their way into English treatises on Mineralogy.

Among these may be mentioned a short account of reticular point systems, and, again, an explanation of the practical application of rationality by means of the anharmonic ratio of a zone. The parallelism of geometrical and crystallographic symmetry is perhaps more fully utilized than even by Miller, or at all events is more fully developed, considerable attention being given to the geometrical discussion of hemihedrism and tetratohedrism. These somewhat difficult points are illustrated by shaded drawings, which give the student a very clear idea of what hemihedrism really means.

The notations used are those of Miller and Naumann, side by side throughout—the figures bearing Miller's numbers on their faces (except for the hexagonal system, in which Bravais's modification is adopted), while Naumann's symbols are used in the descriptive text. Explanations are also given of other systems, such as those of Weiss and Schrauf.

We notice as minor deficiencies in the geometrical portion of the work, first, that it might have been mentioned a little more distinctly that the unclosed forms, which occur in all except the cubic system, can only occur in combination; secondly, that in the account of twin crystals (p. 167) the distinction between the twin-plane and the composition-plane is not very fully explained, and is not illustrated specifically; although a figure of the Carlsbad twin, in which these two planes do not coincide, is given elsewhere (p. 181).

These are, perhaps, not very important points: the first will speedily force itself on the attentive student; and as regards the second, it may be said that no one has a right to assume the identity of two things which are arrived at by different considerations without proving them to be identical.

The optical properties of crystals are considered at somewhat greater length than is usual in elementary treatises; and comprehensive tables are furnished of the optical constants of the transparent minerals.

In the chemical portion of the work, the author has adopted the classification followed by Rammelsberg in the second edition of his *Handbuch der Mineral. Chemie*, which is now regarded as the standard authority upon the chemistry of minerals.

The present volume, which completes the systematic part, is to be followed by another upon descriptive Mineralogy. If the forthcoming volume be prepared with the same ability and painstaking care which is exhibited in that now before us, the two will constitute a reliable elementary textbook which cannot fail to be of the greatest value to students of mineralogy.

VIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xi. p. 541.]

April 6, 1881.—J. W. Hulke, Esq., F.R.S., Vice-President, in the Chair.

THE following communications were read:—

1. "The Microscopic Characters of the Vitreous Rocks of Montana, U. S." By F. Rutley, Esq., F.G.S.; with an Appendix by James Eccles, Esq., F.G.S.

The specimens described were collected by Mr. J. Eccles, F.G.S. They consist of various obsidians and rhyolites, some of them porphyritic or spherulitic, which appear to throw some light on the epoch at which these structures have been set up. In a black porphyritic obsidian is a crystal which the author believed to be olivine. Zirkel has already noticed the occurrence of this mineral in a trachyte. The structure of some of the above indicates that it is extremely difficult to draw hard and fast lines between trachytic rhyolites and felstones. A tuff containing fragments of a rhyolite, some perlitic, was also described. The spaces included within the boundary of some of these perlitic cracks exhibit depolarization and sometimes interference-crosses. The author considered these to be the result of strain in contraction, and connected with incipient crystallization.

Andesites, from two localities in the northern part of the Yellowstone district, were also described.

In an appendix Mr. Eccles briefly described the geology of the region from which the above specimens were collected, referring for greater detail to the memoirs of Dr. Hayden and his fellow workers. In the Yellowstone-Park region trachyte and obsidian (the latter being the upper) form an irregular plateau, resting on rocks of Carboniferous age. No vents were observed; but Mount Washburne, a few miles distant, is a broken-down volcanic cone, from which both trachyte- and basalt-flows (the latter the newer) have proceeded.

2. "On the Microscopic Structure of Devitrified rocks from Beddgelert, Snowdon, and Skomer Island." By F. Rutley, Esq., F.G.S.

The first specimen described was found about a quarter of a mile from Beddgelert, on the Capel-Curig road. Examined microscopically, it showed traces of perlitic structure, with small spherulites, both isolated and in bands, not exhibiting radial structure, but apparently composed of very minute chlorite and a garnet, probably spessartine. Hence the rock must be a devitrified obsidian or pitchstone. The second specimen is a banded greenish-grey "felstone," at Clogwyn du'r Arddu, of Bala age, which also has probably been vitreous. The third specimen, from near Pont y Gromlech, is a schistose felsitic rock. This was compared microscopically with an obsidian from Hungary and a rhyolite from Gardiner's River (N. America), and was shown to have been probably once a glassy rock. In conclusion the author discussed the limits of the terms felstone, rhyolite, trachyte, and obsidian.

An appendix was added upon the microscopic characters of some rocks from Skomer Island, off the coast of Pembrokeshire. These were shown to be devitrified obsidians, some of them exhibiting spherulitic and perlitic structures. A trachytic rock and a basalt from the same locality were also described.

3. "The Date of the last Change of Level in Lancashire." By T. Mellard Reade, Esq., C.E., F.G.S.

The author described some observations made by him at Blundell sands, on the coast of Lancashire, near Liverpool, according to which, judging from the position of high-water mark, the land had gained considerably upon the sea between 1866 and 1874. At one end of a length of 350 yards, spring-tide high-water mark had receded

15 yards, and at the other end 5 yards. The author estimated that the deposit of sand that had accumulated in 8 years amounted to an average of 10 yards width and 2 yards depth. Allowing a further depth of 1 yard for sand that may have been blown over the top, he finds 10,500 cubic yards as the quantity of sand deposited in 8 years on a shore-frontage of 350 yards, or 3·75 cubic yards per yard of frontage per annum. Applying this unit of measurement to the 16 miles of coast forming the western boundary of the deposit, he gets 105,600 cubic yards as the quantity of sand annually moved; 22 square miles of sand, 12 feet thick, give 272,588,800 cubic yards of sand accumulated, which, divided by the annual quantity, will give 2580 years as the age of the whole deposit of blown sand. The author adduced other evidence in support of his view, and concluded that if the last change of level in South-west Lancashire was a downward one, it could not have taken place within 2500 years.

April 27.—Robert Etheridge, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the precise Mode of Accumulation and Derivation of the Moel-Tryfan Shelly Deposits; on the Discovery of similar high-level Deposits along the Eastern Slopes of the Welsh Mountains; and on the Existence of Drift-zones showing probable Variations in the Rate of Submergence." By D. Mackintosh, Esq., F.G.S.

The author commenced by giving a sketch of the progress of discovery connected with the Moel-Tryfan deposits. He then described certain phenomena connected with these deposits, to which little or no attention has been devoted by other observers. After identifying the local stones and indirectly local erratics, he traced the derivation of the far-travelled erratics which came from the N. and N.W. He drew particular attention to an extensive exposure of slaty laminæ, the edges of which have been bent by a force assailing the hill from the N.W.; and as these edges have been shattered so as to form parcels of slate-chips covered by or rolled up in laminated sand, along with parcels of clay, he endeavoured to prove that a stranding of the floating ice which must have brought part of the erratics (including numerous chalk-flints) will alone account for the phenomena. After describing patches of gravel and sand in other parts of Caernarvonshire, referring to the Three-Rock Mountain deposits in Ireland (which must have come from the N.W.), and briefly noticing the drifts on Halkin Mountain, Flintshire, he entered upon the main subject of his paper, namely the discovery of an extensive series of marine drifts, including (besides deposits on flat ground) about twelve hillocks or knolls, consisting of rounded gravel and sand, and, in at least two instances, containing gravel-pits with numerous shell-fragments. They extend along the east side of the northern part of the mountain-range which runs between Minera and Llangollen Vale, and are situated at levels between 1100 and 1300 feet above the sea. The gravel is largely made up of rounded

Eskdale-granite pebbles; and during his last or fourth visit to the district, he found a large granite boulder on the axial summit of the ridge, about 1450 feet above the sea, showing a submergence of the mountain to at least that extent. He went on to assign reasons for believing that the sea lingered longer at the level of the sand and gravel knolls than lower down and higher up, so as to allow time for the extra rounding of the pebbles, accumulation of erratics, and multiplication of Mollusca; for he could discover no reason for supposing that the mollusks which left the shells did not live on or near the spot in the littoral or sublittoral zone. He then described a small exposure of high-level rounded gravel and sand near Llangollen, and dwelt on the remarkable fact that the marine deposits on Moel Tryfan, Three-Rock Mountain (Ireland), Minera Mountain, and in Macclesfield Forest occur at about the same altitude above the sea-level. After proposing a provisional classification of the drift-deposits of North Wales and the Penine Hills into zones, showing probable variations in the rate of submergence, he concluded by discussing the question whether the submergence was caused by the subsidence of the land or the rising of the sea, without venturing to express any decided opinion on the subject, but inclining to the former idea.

2. "On the Correlation of the Upper Jurassic Rocks of England with those of the Continent." By the Rev. J. F. Blake, M.A., F.G.S. Part I. The Paris Basin.

This was an attempt to settle the many questions of correlation arising out of the detailed descriptions given of the various localities in the Paris basin where Upper Jurassic rocks are developed, by a consecutive survey of them all, undertaken by the aid of a grant from the "Government Fund for Scientific Research." In previous papers the names used for the great subdivisions and their boundaries were adopted without material modification; in the present such modifications were proposed as may bring the English and continental arrangements into harmony.

Five distinct areas were considered in this paper. 1. The southern range; 2. The Charentes; 3. Normandy; 4. The Pays de Bray; 5. The Boulonnais.

1. *The Southern Range.*—This is continuous from the Ardennes through the Meuse, Yonne, &c., to the Cher. In the Ardennes the "Ferruginous Oolite" corresponds to our Osmington Oolite, and to the Lower Limestones and Passage-beds of Yorkshire, the underlying "Middle Oxfordian" being equivalent to our Lower Calcareous Grit. Above comes immediately the Coral Rag with *Cidaris florigemma*; and the stratigraphical and palæontological break is constantly between the Coral Rag and Ferruginous Oolite when that occurs. The Corallian is a well-marked formation, though its character is variability. It is divisible generally into two groups—Coral Rag and Supracoralline beds, the latter usually being the "*Diceras*-beds;" but in the Yonne there is a great development of *Diceras*-beds below, associated with *Cidaris flori-*

gemma and massive corals, which is gradually introduced in going west. This part of the series in the Haute Marne has been described as very different; but the author did not at all agree with M. Tombeck's stratigraphical determination, and considers the "Oolite de la Nothe" no more than the continuation of the Supracoralline *Diceras*-beds, which he considers to uniformly *overlie* and never to underlie the *Am. marantianus* marls, which latter are Oxfordian. In fact nothing abnormal occurs in this Department. The whole series has a tendency to degenerate into barren lithographic limestones, in which distinctions are lost. The Astartian and Virgulian beds were traced through this range, the latter seldom showing any well-marked Pterocerian division, and the former being most connected with the overlying series. Above these are limestones hitherto called "Portlandian," in which two zones are constant; but above all are vacuolar Oolites, which alone may be truly correlated with the Portland rocks of England. The whole of the beds in this range are eminently calcareous, a true clay being scarcely anywhere seen.

2. *The Charentes.*—In these two Departments the lower portion is very calcareous, and the distinction of one part from another very slight; but the highest portion, both near Cognac and on the Ile d'Oleron, yields beds which may be paralleled with our true English Portland rocks.

3. *Normandy.*—The complete sequence has here been made out, from the true Oxford Clay of Dives to the Virgulian of Havre; and the similarity of the whole to the sequence in Dorsetshire is very remarkable. "The Trouville Oolite" is the exact representative of the "Osmington Oolites" with the Nothe Grits below; but the place of the Sandsfoot clay is taken by the true Coral Rag, whose right position in the Weymouth section is hereby determined. The Supracoralline beds are the sands of Glos; and the Astartian beds are the *Trigonia*-beds of Havre, which are the exact representatives of the "Kimmeridge passage-beds."

4. *The Pays de Bray.*—Nothing below the Virgulian is here seen; and the commencement of the so-called "Portland beds" was considered by the author to be at a lower level than it is placed by M. Lapparent, on account of the similarity to beds at Boulogne. The true Portland rocks occur as ferruginous sandstones with *Trigonia gibbosa*.

5. *Boulonnais.*—The Houllefort limestone was correlated with the Osmington Oolite. The Coral Rag of Brucdale was considered equivalent to that of the Mont des Boucards, the so-called limestones of the latter place being Supracoralline. The Nerinæan Oolite and the Grès de Wirvigne represent the Astartian. The higher parts of the series have been already correlated.

From this study it was proposed:—that the "Lower Calcareous Grit" and almost all the Coralline Oolite should be placed in the Oxfordian series as the upper division, under the name "Oxford Grit" and "Oxford Oolite;" that the Corallian consists of two parts, the Coral Rag and the Supracoralline beds; that the Kimmeridgian should include the Astartian and Virgulian, the Pterocerian being a subzone; that the "Upper Kimmeridge" and the Hartwell clay, with the "Portland sand," should make a new subdivision to be called Bolonian, the northern and southern types being both represented at Boulogne, which may be divided into Upper and Lower; and that the true Portland limestone and the Purbeck be united into one group, as Lower and Upper Portlandian, the fact of the latter being freshwater being paralleled by parts of the true Portland having that character.

3. "On fossil Chilostomatous Bryozoa from the Yarra-Yarra, Victoria, Australia" By Arthur William Waters, Esq., F.G.S.

June 8.—Robert Etheridge, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. "The Reptile-Fauna of the Gosau Formation, preserved in the Geological Museum of the University of Vienna." By Prof. H. G. Seeley, F.R.S., F.L.S., F.G.S.; with a Note on the Geological Horizon of the Fossils, by Edward Suess, F.M.G.S.

2. "On the Basement-beds of the Cambrian in Anglesey." By Prof. T. McKenny Hughes, M.A., F.G.S.

In this paper the author first pointed out that there was in Anglesey (1) an upper slaty group, in which he had fixed two life zones, which showed that the series belonged to the Silurian (Sedgwick's classification), and (2) a lower group of slates and sandstones in which Arenig fossils had been found in several localities, and Tremadoc had been less clearly recognized, while by the correction of the determination of a species of *Orthis* there was now a suspicion of even Menevian forms. These all rested upon the basement-beds of the Cambrian, of which the paper chiefly treated. They were made up of conglomerates, grits, and sandstones, with Annelids and Fucoids.

The Basement-beds varied in thickness and character according to the drift of currents along the Pre-cambrian shore and the material of the underlying rocks. Near Penlon, where they rested on a quartz-felspar rock, they consisted chiefly of a quartz-grit and conglomerate, almost exactly like that of Twt Hill. Near Llanerchymedd, where there was a mass of greenish schistose rock succeeding the Dimetian, the Cambrian basement-bed contained a large number of fragments of that rock, certain bands being chiefly composed of it. Near Bryngwallen, where the underlying Archæan consisted of gneissic rocks, the Cambrian basement-beds were made up of quartz conglomerate. Tracing it still further to the S.W. he found bosses of conglomerate among the sand dunes of Cymmeran Bay, full of fragments of green schistose rock, like that of Bangor, and telling of the further development of Pebidian at the S.W. end of the Anglesey axis. In several localities these conglomerates were associated with and passed into fossiliferous grits and sandstones. He exhibited slices of the more important rocks, which, he showed, confirmed the results arrived at from other evidence. He pointed out that the observations now made confirmed the views he had expressed on a former occasion with regard to the basement-beds of the Cambrian between Caernarvon and Bangor, where the deposits which rested upon the granitoid rocks of Twt Hill were either a kind of arkose or chiefly composed of quartz with a few pieces of mica-schist and jasper; but as we followed them a few miles to the N.E. we found that the quartz had got pounded into smaller grains, and the larger pebbles were chiefly of felsite, which here formed the shore, while further towards Bangor fragments of the still higher Bangor volcanic series helped to make up the Cambrian shingle-beach.

3. "Description and Correlation of the Bournemouth Beds.—Part II. Lower or Freshwater Series." By J. S. Gardner, Esq., F.G.S.

This was in continuation of a former paper by the author (Q. J. G. S. vol. xxxv. p. 209). The beds described are exposed east and west of Bournemouth and near Poole harbour, over a distance of about four miles. The author referred them to the Middle Bagshot, and stated that they are distinguished from the Lower Bagshot by the absence of the extensive pipe-clay deposits and the presence of brick-earths, and from the overlying beds by the absence of flints. They reach their extreme limit in the western area of the London basin, and are represented by the lignitic beds 19–24 of Prof. Prestwich's section. Lignites can be traced partly across the bay. The cliffs present an oblique section across a delta divisible roughly into four masses, one of which, from its confused bedding and want of fossils, is supposed to have been formed by the silting-up of the main channel. The total thickness of the series was estimated at 600 to 700 feet. The inferences drawn by the author were as follows:—1. from the beds cut through showing a steep side to the west, that the river flowed from that direction; 2. from the absence of boulders or coarse sediment, that the area was flat; 3. from the absence of lignite, that there were catchment basins; 4. from the absence of flint and the quartzose nature of the beds, that no chalk escarpments were cut through, and that the deposits came from a granitic area; and 5. from the presence of wood bored by *Teredo* that the beds belong to the lower part of the river in proximity to tidal water.

The flora was stated to be confined to local patches of clay. Those at the western end of the section are very rich, and distinguished from the rest by absence of palms and rarity of ferns. The beds near Bournemouth are still richer and very distinct; those east of Bournemouth are characterized by *Eucalypti*, Aroids, and *Araucariæ*; and those at the western end of the section by abundant Polypodiaceæ. It is remarkable that nearly every patch contains a flora almost peculiar to it; but the flora as a whole seems to pass upward to the Oligocene, but not down to the Lower Bagshot.

IX. *Intelligence and Miscellaneous Articles.*

NOTE BY DR. C. R. ALDER WRIGHT ON MR. SHIDA'S PAPER "ON THE NUMBER OF ELECTROSTATIC UNITS IN THE ELECTROMAGNETIC UNIT," AND ON HIS RECENT NOTE THEREON.

THE note by Mr. R. Shida, in the June Number of the Philosophical Magazine (p. 473), calls for some remark, in view of the great interest and importance attaching to the exact valuation of the different electrical magnitudes and their correlations. Mr. Shida stated, in his original paper*, that he measured the E.M.F. of a gravity Daniell cell electromagnetically by determining the current generated by it in a circuit the *total* resistance of which (inclusive of the internal resistance of the cell) was measured, the product of the current into the resistance being the required E.M.F.; also that he compared, by means of a quadrant electrometer, the E.M.F. of the gravity-cell with that of a 30-cell Daniell battery, which had been just measured by means of an absolute electrometer—this electrostatic valuation being made just before, and also just after, the electromagnetic valuation. [It is noticeable, in passing, that, through an obvious slip in copying his results, Mr. Shida gives in each of the papers the electrostatic E.M.F. of the 30-cell battery as 0·904187 C.G.S. units, and that of the gravity-cell as $\frac{0{\cdot}904187}{26{\cdot}229}=0{\cdot}034381$ C.G.S. units, instead of only one tenth of these quantities in each case respectively; otherwise the final value of v deduced would be only one tenth of the quantity found.] In his recent note, Mr. Shida says that he omitted to state in the original paper that the electrostatic valuation depended upon by him was made "*while the current was actually flowing through the tangent-galvanometer,*" as it was during the electromagnetic valuation; and he consequently infers that he eliminated the possible source of error incidentally suggested by me (whilst discussing the subject of "polarization" of so-called constant batteries) as existing in his experiments, due to the E.M.F. of the cell during the electromagnetic valuation whilst a current was passing not being identical with that subsisting during the electrostatic valuation made (as would be naturally supposed on reading his paper) when no current was passing.

It is noteworthy, however, that in striving thus to avoid the Scylla of possible error through battery-polarization, Mr. Shida has fallen into the Charybdis of a yet more serious error, due to the circumstance that the potential difference registered by a quadrant-electrometer to which a cell is applied *is not the E.M.F. of that cell if it generate a current, but is something less.* That this is so, is manifest from the following considerations. Let the actual E.M.F. of the cell be E_1, and the E.M.F. obtained by the quadrant-electrometer be E_2. Let C be the current generated, R_1 the internal resistance of the cell, and R_2 the external resistance of the rest of the circuit. Then $C=\frac{E_1}{R_1+R_2}$. The total work which this current

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* Phil. Mag. Dec. 1880, p. 431; also Brit. Assoc. Reports, 1880, p. 497.

does in the circuit in a time t, is $C^2(R_1+R_2)t$; of which an amount C^2R_1t is done inside the cell, and the balance C^2R_2t outside. This latter amount is necessarily equal to the product of the quantity of electricity passing, Ct, into the difference of potential between the ends of the external resistance, which latter quantity is E_2; that is,

$$C^2R_2t=E_2Ct.$$

Hence

$$E_2=CR_2=\frac{R_2}{R_1+R_2}E_1;$$

or, E_2 is less than E_1 by an amount which is the greater the larger is R_1 in reference to R_2: if $R_1=R_2$, $E_2=\frac{1}{2}E_1$. In Mr. Shida's experiments R_1 was 2·02 ohms, whilst R_2 was successively 80·86, 110·86, and 130·86 ohms, averaging 107·53 ohms. The mean electrostatic value obtained by Mr. Shida was consequently nearly 2 per cent. too small; so that the value of v finally deduced by dividing the electromagnetic value by the electrostatic value must have been just as much in excess of the truth.

THE PHENOMENON CALLED THE "CRY OF TIN."

BY J. C. DOUGLAS.

If a piece of tin be bent it emits a sound; this, being regarded as a property peculiar to tin, has been termed "the cry of tin." This phenomenon is explained by the peculiar crystalline structure of the metal. If the explanation be the true one, then other metals which are obviously crystalline in structure should also exhibit the phenomenon under favourable conditions. But it is exceedingly difficult to place other metals in a crystalline state under proper conditions; *e. g.* cast iron and cast zinc in thin rods break before they can be bent sufficiently to emit audible sounds, while rolled zinc has had its crystalline structure destroyed by rolling, and so is not in a condition to emit sound when bent. Rolled zinc is very tough as compared with cast zinc; and its fracture is not crystalline, but of an even fine-grained bluish tint destitute of the brilliant lustre presented by this metal in a crystalline state. If, however, a piece of rolled zinc be heated for a few minutes to a temperature somewhat below its melting-point, the metal becomes much less tough, and its fracture is decidedly crystalline. On bending a piece of zinc so treated it emits a sound weaker than, but of the same nature as, the sound emitted by tin. Cast zinc cannot be bent readily; but if it be pinched between the teeth or with pliers it emits the sound distinctly.

It appears, therefore, that the cry of tin is due to crystalline structure, that it is not characteristic of tin as generally accepted, but may be emitted by zinc and probably by other metals when crystalline in structure; that rolling in the case of tin and zinc, and probably in other cases, destroys the property with the alteration of texture; that, in the case of zinc which has been rolled, the crystalline texture may be produced without melting the metal but by merely heating it, and this is so readily done that it affords a ready illustration of the effects of high temperature on rolled metal. If, as supposed, this sound is characteristic of the crystalline structure of metals, it may afford a means of great practical use, whereby, by the sound a metal emits, we may draw conclusions as to its texture, and hence its fitness for certain purposes; or by the sound emitted by a beam when bent we may draw conclusions as to its safety, the microphone or other appliance being called in to aid us where the sounds are exceedingly weak.—*Proceedings, Asiatic Society of Bengal, February* 1881, *communicated by the Author.*

DISCUSSION OF THE THEORY OF THE THREE FUNDAMENTAL COLOUR-SENSATIONS. BY A. ROSENSTIEHL.

The notion of the three fundamental colour-sensations arose from the study of the properties of the eye which is imperfectly organized in regard to the perception of colours. The theory which connects the observed phenomena leads to an hypothesis on the structure of the normal eye.

But since Maxwell's experiments* on the solar spectrum, more than twenty years since, this subject has not again been the object of any investigation, the experimental method for studying the laws of colour-vision upon the normal eye being wanting. By determining, with the aid of rotating disks, the distribution of the complementary colours in a chromatic circle, I believe I have supplied that deficiency. The position of the three colours corresponding to Young's fundamental sensations has been thereby determined with much more precision than was hitherto possible. Young's theory receiving from this fact a support which takes it out of the domain of hypothesis, it has appeared to me useful to discuss its consequences, and to point out those which are susceptible of exact experimental verification. It will follow from this discussion, on the one hand, that certain properties attributed to the primary colours do not belong to them exclusively, and, on the other, that their true distinctive character has not yet been enunciated.

The primary colours (that is, those corresponding to the fundamental sensations) possess, by their very definition, the following properties:—

1. *On being mixed two by two, they produce all the colours perceptible to our eye.*

This property belongs to all the colours which are not complementary; but at the same time the sensation of white will be produced; so that the following limiting condition must be added:—

They produce at the same time the sensation of white in a less degree than the other colours.

I have already, indirectly, made use of this very important character†. Its experimental verification, however, presents great practical difficulties, because the quantity of white light emitted by a coloured surface cannot be measured with precision. It would be necessary to execute a chromatic circle with all its colours of

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* Proc. Royal. Soc. vol. x. pp. 404-409 (1860).

† *Comptes Rendus,* t. xcii. p. 357; Phil. Mag. April 1881, p. 305.

equal intensity and emitting, under equal illumination, the same amount of white light. The circle with which I have worked does not possess these qualities; and I have renounced taking this course; moreover the difficulty can be easily turned.

2. *The sensation of pure white results from the equal excitation of the three fundamental sensations.*

Each of these colours has for its complement the mixture of the other two at equal intensity.

The graphic construction representing the whole of the perceptible colours usually takes the form of an equilateral triangle, the vertices of which are occupied by the primary colours. The corresponding complementary, which is always a binary colour, is placed in the middle of the opposite side. All the other binary colours have their places also on the sides of the triangle, so that the two complementaries are situated at the two extremities of a straight line passing through the point of intersection of the median ones.

The properties above enunciated, and the consequences I have enumerated, and which are admitted, are not characteristic of the primary colours; they belong to an indefinite number of colours, on the condition, however, that they be selected according to a certain rule. An assemblage of three colours possessing this property I call a *triad*. In the equilateral triangle above described *the secondary triads will be represented by all the equilateral triangles which can be inscribed in it*. The smallest of these triangles is that formed by the triad of the complementaries of the primary colours. To each secondary triad corresponds in like manner a triad composed of complementary colours; and as we can choose them as near together as we please, there is really no limit to their number. The distribution of the complementary colours in a triad is susceptible of experimental verification; and I shall have occasion to revert to it.

It is expedient to add, as a corollary, that three colours not constituting a triad may still by their mixture produce the sensation of white; but this sensation no longer results from their mixture in equal quantities. It follows that, in the graphic construction, their complements are then not situated in the middle of the opposite side. This point, like the preceding, has been verified experimentally.

I now come to an essential character. If, in an equilateral triangle, each vertex be joined to the middle of the opposite side, it will be divided into six equal right-angled triangles. The colours included in two of these triangles with their vertices opposed, are reciprocally complementary.

Given the distance b of a binary colour at the vertex of the equilateral triangle, the position of the complementary colour will be given by the distance x which separates it from the corresponding vertex, which distance is represented by

$$x = c \frac{\sin \alpha}{\sin(30^\circ + \alpha)},$$

a formula in which c is constant and α the angle formed by the straight line joining the two complementary colours to the corresponding median line: α is moreover obtained from the equation

$$b = c \tan \alpha.$$

The discussion of this equation shows that, as soon as α exceeds 45°, b increases much more rapidly than x. From this it follows that the colours situated in the vicinity of a vertex will occupy in the construction a larger space than their complementaries, which are found all assembled towards the middle of the opposite side. That is to say, *the colours situated on both sides of a primary colour, and which to the eye are equidistant, have their complementaries so close to one another that it becomes difficult to distinguish those which are consecutive.*

This remarkable character is that which constitutes the fundamental property of the primary triad. It was this phenomenon that attracted my attention when studying the distribution of the complementary colours in a chromatic circle. It has not, therefore, to be verified experimentally; thanks to it, I have been able to fix the position of the primary colours within very narrow limits; and it only remains for me to prove that within those limits there are three colours possessing the characters of a triad. This I will do soon, by showing that they obey the law of the complementary colours* which I have formulated above.

The cycle of the demonstrations will then be closed; all the proofs, synthetic and analytic, will then have been given; and the accordance between the results of experiment and that of calculation will be such that Young's theory may be regarded as established on solid scientific foundations.—*Comptes Rendus de l'Académie des Sciences*, May 30, 1881, t. xcii. pp. 1286–1289.

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* Von Bezold (Poggendorff's *Annalen*, vol. cl. p. 71, 1873), believes he has given the mathematical law of the complementary colours. He has taken as his experimental starting-point the investigation made by Helmholtz of the distribution of those complementary colours in the spectrum. Unfortunately he has not been able to avoid a confusion which moreover exists in all the special treatises, and which is rendered so easy by the usual language referring to the colours.

He introduces into his calculations two absolutely heterogeneous data. One is the number of vibrations representing a colour—a value of a purely physical order, and independent of the properties of the eye; the other is the law of the mixture of colours, the hypothesis of Young, the notion of the complementary colours—a notion essentially physiological, since it depends on the structure of the eye, and varies according as the latter is or is not normally constituted. The results, too, obtained by von Bezold present sensible discrepancies with experiment; but he attributes to the perturbations induced by the fluorescence of the media of the eye the want of agreement which he necessarily found.

The number of vibrations cannot have any meaning in the present case, unless employed simply as a means of nomenclature of the colours and not as a mathematical value to be introduced into the calculations. The colours chosen by von Bezold as primary are also very far from possessing the properties of the fundamental sensations.

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[FIFTH SERIES.]

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X. *On the Electromagnetic Theory of Light.* *By* LORD RAYLEIGH, *F.R.S., Professor of Experimental Physics in the University of Cambridge*[*].

THE claims of the theory propounded by Maxwell, according to which light consists of a disturbance in a medium susceptible of dielectric polarization, are so considerable that it is desirable to extend its application as far as possible to various optical phenomena. The question of the velocity of propagation in vacuum and in singly or doubly refracting transparent dielectrics was considered by Maxwell himself; and the agreement with experiment, though far from perfect, is sufficiently encouraging. More recently it has been shown by Helmholtz[†], Lorentz[‡], Fitzgerald[§], and J. J. Thomson[‖], that the same theory leads to Fresnel's expressions for the intensity of light reflected and refracted at the surface of separation of transparent media, and that the auxiliary hypotheses necessary in this part of the subject agree with those required to explain the laws of double refraction. In this respect the electromagnetic theory has a marked advantage over the older view, which assimilated luminous vibrations to the ordinary transverse vibrations of elastic solids. According to the latter, Fresnel's laws of double refraction, fully confirmed by modern

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[*] Communicated by the Author.

[†] Crelle, Bd. lxxii., 1870.

[‡] Schlömilch, xxii., 1877.

[§] Phil. Trans. 1880.

[‖] Phil. Mag. April 1880.

observation*, require us to suppose that in a doubly-refracting crystal the rigidity of the medium varies with the direction of the strain; while, in order to explain the facts relating to the intensities of reflected light, we have to make the inconsistent assumption that the rigidity does not vary in passing from one medium to another. A further discussion of this subject will be found in papers published in the Philosophical Magazine during the year 1871.

If the dielectric medium be endowed with sensible conductivity, the electric vibrations will be damped; that is to say, the light will undergo absorption, with a rapidity which Maxwell has calculated. By supposing the conductivity to be so great that practically complete absorption takes place within a distance comparable with the wave-length, we may obtain a theory of metallic reflection which is not without interest, although the phenomena of abnormal dispersion show that it cannot be regarded as complete.

For an isotropic medium at rest we have the equations (Maxwell's 'Electricity and Magnetism,' §§ 591, 598, 607, 610, 611)

$$u=p+\frac{df}{dt}, \text{ \&c.}, \qquad (1)$$

$$f=\frac{K}{4\pi}P, \text{ \&c.}, \qquad (2)$$

$$p=CP, \text{ \&c.}, \qquad (3)$$

$$P=-\frac{dF}{dt}-\frac{d\Psi}{dx}, \text{ \&c.}, \qquad (4)$$

$$a=\frac{dH}{dy}-\frac{dG}{dz}, \text{ \&c.}, \qquad (5)$$

$$a=\mu\alpha, \text{ \&c.}, \qquad (6)$$

$$4\pi u=\frac{d\gamma}{dy}-\frac{d\beta}{dz}, \text{ \&c.}; \qquad (7)$$

in which f, g, h are the electric displacements, p, q, r the currents of conduction, u, v, w the total currents, P, Q, R the components of electromotive force, K the specific inductive capacity, C the conductivity, α, β, γ the components of magnetic force, a, b, c the components of magnetization, μ the magnetic capacity, F, G, H the components of electrokinetic momentum, and Ψ the electric potential.

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* Glazebrook, Phil. Trans. 1879.

From (2), (4), and (5) we get

$$4\pi\left(\frac{d}{dy}\frac{f}{\mathrm{K}}-\frac{d}{dx}\frac{g}{\mathrm{K}}\right)=-\frac{d}{dt}\left(\frac{d\mathrm{F}}{dy}-\frac{d\mathrm{G}}{dx}\right)=\frac{dc}{dt}\ \&\text{c.} \quad . \quad (8)$$

In the case of K constant, equation (8) expresses that the electric displacement $\int(f\,dx+g\,dy)$ round a small circuit in the plane of xy corresponds to the electromotive force round the circuit, represented by dc/dt.

Again, from (1), (2), (3), (6), (7),

$$4\pi\left(\frac{df}{dt}+\frac{4\pi\mathrm{C}}{\mathrm{K}}f\right)=\frac{d}{dy}\frac{c}{\mu}-\frac{d}{dz}\frac{b}{\mu}\ \&\text{c.} \quad . \quad . \quad . \quad (9)$$

From equations (8) and (9) the problem of reflection can be investigated. In order to limit ourselves to plane waves of simple type, we shall suppose that K, μ, and C are independent of z, and that the electric and magnetic functions are independent of z and (as dependent upon the time) proportional to e^{int}. The two principal cases will be considered separately, (1) when the electric displacements are perpendicular to the plane of incidence, (2) when they are executed in that plane.

Case 1. This is defined by the conditions

$$f=0, \quad g=0, \text{ and (accordingly) } c=0.$$

Thus

$$ina=-4\pi\frac{d}{dy}\frac{h}{\mathrm{K}}, \quad inb=4\pi\frac{d}{dx}\frac{h}{\mathrm{K}}, \quad . \quad . \quad (10)$$

$$4\pi\left(in+\frac{4\pi\mathrm{C}}{\mathrm{K}}\right)h=\frac{d}{dx}\frac{b}{\mu}-\frac{d}{dy}\frac{a}{\mu}. \quad . \quad . \quad . \quad (11)$$

Eliminating a and b from (10) and (11), we get

$$\frac{d}{dx}\left(\frac{1}{\mu}\frac{d}{dx}\right)\frac{h}{\mathrm{K}}+\frac{d}{dy}\left(\frac{1}{\mu}\frac{d}{dy}\right)\frac{h}{\mathrm{K}}+n^2\mathrm{K}\left(1-in\frac{4\pi\mathrm{C}}{\mathrm{K}}\right)\frac{h}{\mathrm{K}}=0. \quad (12)$$

Case 2. Here the special conditions are

$$h=0, \quad a=0, \quad b=0.$$

We have

$$4\pi\left(\frac{d}{dy}\frac{f}{\mathrm{K}}-\frac{d}{dx}\frac{g}{\mathrm{K}}\right)=inc, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$4\pi\left(in+\frac{4\pi\mathrm{C}}{\mathrm{K}}\right)f=\frac{d}{dy}\left(\frac{c}{\mu}\right), \quad 4\pi\left(in+\frac{4\pi\mathrm{C}}{\mathrm{K}}\right)g=-\frac{d}{dx}\left(\frac{c}{\mu}\right); \quad (14)$$

whence by elimination of f and g,

$$\frac{d}{dx}\left\{\frac{1}{\mathrm{K}n^2(1-4\pi n^{-1}\mathrm{C}\mathrm{K}^{-1})}\frac{d}{dx}\left(\frac{c}{\mu}\right)\right\}$$
$$+\frac{d}{dy}\left\{\frac{1}{\mathrm{K}n^2(1-4\pi n^{-1}\mathrm{C}\mathrm{K}^{-1})}\frac{d}{dy}\left(\frac{c}{\mu}\right)\right\}+\mu\left(\frac{c}{\mu}\right)=0. \quad (15)$$

Equations (12) and (15) simplify considerably in their application to a uniform medium, assuming the common form

$$\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+n^2\mu\mathrm{K}(1-4\pi n\mathrm{CK}^{-1})=0. \quad . \quad (16)$$

To express the boundary conditions let us suppose that $x=0$ is the surface of transition between two uniform media. From (12) we learn that the required conditions for case 1 are that

$$\frac{h}{\mathrm{K}} \text{ and } \frac{1}{\mu}\frac{d}{dx}\left(\frac{h}{\mathrm{K}}\right)$$

must be continuous.

In like manner, for case 2 we see from (15) that

$$\frac{c}{\mu} \text{ and } \frac{1}{\mathrm{K}(1-4\pi n^{-1}\mathrm{CK}^{-1})}\frac{d}{dx}\left(\frac{c}{\mu}\right)$$

must be continuous.

If the media are transparent, or but moderately opaque, we have to put $\mathrm{C}=0$. The differential equation is of the form

$$\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+n^2\mu\mathrm{K}=0. \quad . \quad . \quad . \quad . \quad (17)$$

In case 1 the boundary conditions are the continuity of the dependent variable and of $\frac{1}{\mu}\frac{d}{dx}$, and in case 2 the continuity of the dependent variable and of $\frac{1}{\mathrm{K}}\frac{d}{dx}$. Analytically, the results are thus of the same form in both cases. If θ and θ_1 are respectively the angles of incidence and refraction, the ratio of the reflected to the incident vibration is in case 1

$$\frac{\dfrac{\tan\theta_1}{\tan\theta}-\dfrac{\mu}{\mu_1}}{\dfrac{\tan\theta_1}{\tan\theta}+\dfrac{\mu}{\mu_1}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

and in case 2

$$\frac{\dfrac{\tan\theta_1}{\tan\theta}-\dfrac{\mathrm{K}}{\mathrm{K}_1}}{\dfrac{\tan\theta_1}{\tan\theta}+\dfrac{\mathrm{K}}{\mathrm{K}_1}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

in which K, μ relate to the first, and K_1, μ_1 to the second medium; while the relation between θ_1 and θ is

$$\mathrm{K}_1\mu_1 : \mathrm{K}\mu=\sin^2\theta : \sin^2\theta_1. \quad . \quad . \quad . \quad . \quad (20)$$

As Helmholtz has remarked, Fresnel's formulæ may be obtained on two distinct suppositions. If $\mu_1=\mu$,

$$(18)=\frac{\sin(\theta_1-\theta)}{\sin(\theta_1+\theta)},$$

and

$$(19)=\frac{\tan(\theta_1-\theta)}{\tan(\theta_1+\theta)};$$

but if $K_1=K$, then (19) identifies itself with the sine-formula, and (18) with the tangent-formula. Electrical phenomena, however, lead us to prefer the former alternative, and thus to the assumption that the *electric* displacements are perpendicular to the plane of polarization. The formulæ for the refracted waves, which follow from those of the reflected waves in virtue of the principle of energy alone, do not call for detailed consideration.

In the problem of perpendicular incidence, we have from (12), if μ be constant and C zero,

$$\frac{d^2}{dx^2}\frac{h}{K}+n^2\mu K\left(\frac{h}{K}\right)=0. \quad . \quad . \quad . \quad . \quad . \quad (21)$$

For an application of this equation to determine the influence of defective suddenness in the transition between two uniform media, the reader is referred to a paper in the eleventh volume of the Proceedings of the Mathematical Society.

In order to obtain a theory of metallic reflection, C must be considered to have a finite value in the second medium. The symbolical solution is not thereby altered from that applicable to transparent media, the effect of the finiteness of C being completely represented in both cases by the substitution of $K(1-i4\pi nCK^{-1})$ for K. Thus, if μ be constant, the formula for the amplitude and phase of the reflected wave in case 1 is to be found by transformation of (18), in which the imaginary angle of refraction θ_1 is connected with θ by the relation

$$K_1(1-i4\pi nCK_1^{-1}):K=\sin^2\theta:\sin^2\theta_1. \quad . \quad . \quad . \quad (22)$$

In like manner the solution for case 2 is to be found by transformation of (19) under the same supposition.

With regard to the proposed transformations, the reader is referred to a paper by Eisenlohr* and to some remarks thereupon by myself†. The results are the formulæ published without proof by Cauchy. From the calculations of Eisenlohr it appears that Jamin's observations cannot be reconciled with the formulæ without supposing $K_1:K$, *i. e.* the real part of the square of the complex refractive index, to be negative—a

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* Pogg. *Ann.* t. civ. p. 368.

† Phil. Mag. May 1872.

further proof that much remains to be done before the electrical theory of metallic reflection can be accepted as complete*.

The same fundamental equations (8) and (9) will now be applied to the problem of determining the effect on a train of plane waves of a small variation in the quantities K and μ which define the medium. A similar method will be adopted to that already used for light in a paper "On the Scattering of Light by small Particles"†, and in my book 'On the Theory of Sound,' § 296, the principle of which consists in an approximation depending upon the neglect of the higher powers of the small variations $\Delta\mathrm{K}$ and $\Delta\mu$.

Let us suppose that a train of plane waves, in which the electric displacement is parallel to z, and magnetization parallel to y, propagates itself parallel to x undisturbed until it falls upon a region where the generally constant values of K and μ become $\mathrm{K}+\Delta\mathrm{K}$ and $\mu+\Delta\mu$. If $\Delta\mathrm{K}$ and $\Delta\mu$ were zero, the wave would pass on as before; but under the circumstances secondary waves are generated, which diverge from the region of disturbance, and are ultimately, when $\Delta\mathrm{K}$ and $\Delta\mu$ are small enough, proportional in magnitude to these quantities. As the expression of the primary waves we may take

$$h_0=e^{int}\,e^{ikx}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

and corresponding thereto, by (8),

$$b_0=4\pi kn^{-1}\mathrm{K}^{-1}\,e^{int}\,e^{ikx}, \quad . \quad . \quad . \quad . \quad . \quad (24)$$

in which, if λ denote the wave-length, $k=2\pi/\lambda$, and n/λ is the velocity of propagation $(\mathrm{K}\mu)^{-\frac{1}{2}}$. The complete values of the functions being represented, as before, by f, g, h, a, b, c, we shall put

$$f=f_0+f_1+f_2+\ldots\text{\&c.}, \quad a=a_0+a_1+\ldots\text{\&c.},$$

$f_0\ldots a_0\ldots$ being independent of $\Delta\mathrm{K}$ and $\Delta\mu$, $f_1\ldots a_1\ldots$ being of the first order, $f_2\ldots a_2\ldots$ of the second order, and so on, in these quantities. In the actual case f_0, g_0, a_0, c_0 vanish, and only h_0 and b_0 are finite.

From (8) and (9) with $\mathrm{C}=0$, we get

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* July 15.—I see that Lorentz, in a pamphlet *Over de Theorie der Terugkaatsing en Breking van het Licht* (Arnhem, 1875), has developed a theory of metallic reflection similar to that indicated in the text, and has noticed the same difficulty in the application to experiment.

† Phil. Mag. June 1871.

$$\left.\begin{aligned}
4\pi\left\{\frac{df}{dy}-\frac{dg}{dx}+\mathrm{K}\frac{d}{dy}(f\Delta\mathrm{K}^{-1})-\mathrm{K}\frac{d}{dx}(g\Delta\mathrm{K}^{-1})\right\}&=\mathrm{K}\frac{dc}{dt},\\
4\pi\left\{\frac{dg}{dz}-\frac{dh}{dy}+\mathrm{K}\frac{d}{dz}(g\Delta\mathrm{K}^{-1})-\mathrm{K}\frac{d}{dy}(h\Delta\mathrm{K}^{-1})\right\}&=\mathrm{K}\frac{da}{dt},\\
4\pi\left\{\frac{dh}{dx}-\frac{df}{dz}+\mathrm{K}\frac{d}{dx}(h\Delta\mathrm{K}^{-1})-\mathrm{K}\frac{d}{dz}(f\Delta\mathrm{K}^{-1})\right\}&=\mathrm{K}\frac{db}{dt}.
\end{aligned}\right\}\quad(25)$$

$$\left.\begin{aligned}
\frac{dc}{dy}-\frac{db}{dz}+\mu\frac{d}{dy}(c\Delta\mu^{-1})-\mu\frac{d}{dz}(b\Delta\mu^{-1})&=4\pi\mu\frac{df}{dt},\\
\frac{da}{dz}-\frac{dc}{dx}+\mu\frac{d}{dz}(a\Delta\mu^{-1})-\mu\frac{d}{dx}(c\Delta\mu^{-1})&=4\pi\mu\frac{dg}{dt},\\
\frac{db}{dx}-\frac{da}{dy}+\mu\frac{d}{dx}(b\Delta\mu^{-1})-\mu\frac{d}{dy}(a\Delta\mu^{-1})&=4\pi\mu\frac{dh}{dt}.
\end{aligned}\right\}\quad .\ (26)$$

By differentiation of the first equation of (26) and substitution from (25), we get, having regard to

$$\frac{df}{dx}+\frac{dg}{dy}+\frac{dh}{dz}=0,\quad .\ .\ .\ .\ .\ .\quad (27)$$

which is a consequence of (1), (3), (7),

$$\begin{aligned}
\mu\mathrm{K}\frac{d^2f}{dt^2}=\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}&+\mathrm{K}\left(\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right)(f\Delta\mathrm{K}^{-1})\\
&-\mathrm{K}\frac{d^2}{dx\,dy}(g\Delta\mathrm{K}^{-1})-\mathrm{K}\frac{d^2}{dx\,dz}(h\Delta\mathrm{K}^{-1})\\
&+\frac{\mu\mathrm{K}}{4\pi}\frac{d^2}{dy\,dt}(\mathrm{C}\Delta\mu^{-1})-\frac{\mu\mathrm{K}}{4\pi}\frac{d^2}{dz\,dt}(b\Delta\mu^{-1}),
\end{aligned}$$

or, remembering that the functions as dependent upon time vary as e^{int},

$$\begin{aligned}
\nabla^2f+k^2f+\mathrm{K}&\left(\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right)(f\Delta\mathrm{K}^{-1})\\
&-\mathrm{K}\frac{d^2}{dx\,dy}(g\Delta\mathrm{K}^{-1})-\mathrm{K}\frac{d^2}{dx\,dz}(h\Delta\mathrm{K}^{-1})\\
&+\frac{in\mu\mathrm{K}}{4\pi}\frac{d}{dy}(c\Delta\mu^{-1})-\frac{in\mu\mathrm{K}}{4\pi}\frac{d}{dz}(b\Delta\mu^{-1})=0,\quad(28)
\end{aligned}$$

with two similar equations in g and h.

Introducing now the expansion in powers of $\Delta\mathrm{K}$ and $\Delta\mu$, we get as the first approximation

$$\nabla^2f_1+k^2f_1-\mathrm{K}\frac{d^2}{dx\,dz}(h_0\Delta\mathrm{K}^{-1})-\frac{in\mu\mathrm{K}}{4\pi}\frac{d}{dz}(b_0\Delta\mu^{-1})=0,$$

or, on substitution for b_0 in terms of h_0 from (23), (24),

$$\nabla^2 f_1 + k^2 f_1 - \mathrm{K}\frac{d^2}{dx\,dz}(h_0\Delta\mathrm{K}^{-1}) - ik\mu\frac{d}{dz}(h_0\Delta\mu^{-1}) = 0, \qquad (29)$$

and

$$\nabla^2 g_1 + k^2 g_1 - \mathrm{K}\frac{d^2}{dy\,dz}(h_0\Delta\mathrm{K}^{-1}) = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (30)$$

$$\nabla^2 h_1 + k^2 h_1 + \mathrm{K}\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2}\right)(h_0\Delta\mathrm{K}^{-1}) + ik\mu\frac{d}{dx}(h_0\Delta\mu^{-1}) = 0. \quad . \quad . \quad . \qquad (31)$$

The solution of (29) is

$$f_1 = -\frac{\mathrm{K}}{4\pi}\iiint\frac{e^{-ikr}}{r}\,\frac{d^2}{dx\,dz}(h_0\Delta\mathrm{K}^{-1})\,dx\,dy\,dz - \frac{ik\mu}{4\pi}\iiint\frac{e^{-ikr}}{r}\,\frac{d}{dz}(h_0\Delta\mathrm{K}^{-1})\,dx\,dy\,dz, \quad . \quad . \qquad (32)$$

where r, equal to $\sqrt{\{(\alpha-x)^2+(\beta-y)^2+(\gamma-z)^2\}}$, is the distance of the element of volume $dx\,dy\,dz$ from the point α, β, γ at which f_1 is to be estimated.

In applying (32) to the calculation of a secondary wave at a distance from the region of disturbance, we may conveniently integrate it by parts. Thus,

$$f_1 = -\frac{\mathrm{K}}{4\pi}\iiint h_0\Delta\mathrm{K}^{-1}\,\frac{d^2}{dx\,dz}\left(\frac{e^{-ikr}}{r}\right)dx\,dy\,dz + \frac{ik\mu}{4\pi}\iiint h_0\Delta\mathrm{K}^{-1}\,\frac{d}{dz}\left(\frac{e^{-ikr}}{r}\right)dx\,dy\,dz.$$

From the general value of r,

$$\frac{d}{dz}\left(\frac{e^{-ikr}}{r}\right) = \frac{\gamma-z}{r}\,\frac{e^{-ikr}(1+ikr)}{r^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (33)$$

$$\frac{d^2}{dx\,dz}\left(\frac{e^{-ikr}}{r}\right) = \frac{\alpha-x}{r}\,\frac{\gamma-z}{r}\,\frac{e^{-ikr}(3+3ikr-k^2r^2)}{r^3}. \quad . \qquad (34)$$

If r be sufficiently great in comparison with λ, only the highest power of kr in the above expressions need be retained; and if r be also great in comparison with the dimensions of the region of disturbance, supposed to be situated about the origin of coordinates, $(\alpha-x)/r$ &c. may be replaced by α/r &c. Thus,

$$\frac{d}{dz}\left(\frac{e^{-ikr}}{r}\right) = \frac{\gamma}{r}\,\frac{ike^{-ikr}}{r};$$

$$\frac{d^2}{dz\,dx}\left(\frac{e^{-ikr}}{r}\right) = -\frac{\alpha\gamma}{r^2}\,\frac{k^2e^{-ikr}}{r};$$

and the expression for f_1 becomes

$$f_1 = \frac{k^2}{4\pi r}\left[K\frac{\alpha\gamma}{r^2}\iiint h_0 \Delta K^{-1} e^{-ikr}\,dx\,dy\,dz \right.$$
$$\left. -\mu\frac{\gamma}{r}\iiint h_0\,\Delta\mu^{-1} e^{-ikr}\,dx\,dy\,dz\right].$$

For the sake of brevity we will write this

$$f_1 = \frac{k^2}{4\pi r}\left[KP\frac{\alpha\gamma}{r^2} - \mu Q\frac{\gamma}{r}\right], \quad . \quad . \quad . \quad (35)$$

where

$$\left.\begin{aligned} P &= \iiint h_0 \Delta K^{-1} e^{-ikr}\,dx\,dy\,dz, \\ Q &= \iiint h_0 \Delta\mu^{-1} e^{-ikr}\,dx\,dy\,dz. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (36)$$

In like manner from (30) and (31),

$$g_1 = \frac{k^2}{4\pi r}\left[KP\frac{\beta\gamma}{r^2}\right], \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

$$h_1 = \frac{k^2}{4\pi r}\left[-KP\frac{\alpha^2+\beta^2}{r^2} + \mu Q\frac{\alpha}{r}\right]. \quad . \quad . \quad . \quad . \quad (38)$$

Equations (35), (37), (38) express the electric displacement in the secondary waves. Since $\alpha f + \beta g + \gamma h = 0$, the displacement is perpendicular to the direction of the secondary ray. The general expression for the intensity is found by adding the squares of f, g, h; but it will be sufficient for our present purpose to limit ourselves to the case where the secondary ray is perpendicular to the primary ray, *i. e.* to the case $\alpha = 0$. Then

$$f^2 + g^2 + h^2 = \frac{k^4}{16\pi^2 r^2}\left[K^2P^2\frac{\beta^2}{r^2} + \mu^2Q^2\frac{\gamma^2}{r^2}\right]. \quad . \quad . \quad . \quad (39)$$

If P and Q are both finite, there is no direction along which the secondary light vanishes. We find by experiment, however, that the light scattered by small particles on which polarized light impinges does vanish in one direction perpendicular to the original ray; and thus either P or Q must vanish. Now, when the particles are very small, we have

$$P = h_0 \Delta K^{-1} e^{-ikr}\iiint dx\,dy\,dz, \quad Q = h_0 \Delta\mu^{-1} e^{-ikr}\iiint dx\,dy\,dz; \quad (40)$$

so that if P vanishes, $\Delta K = 0$; and if Q vanishes, $\Delta\mu = 0$. The optical evidence that either ΔK or $\Delta\mu$ vanishes is thus very strong; while electrical reasons lead us to conclude that it is $\Delta\mu$.

If we write T for the volume of the small particle, we get from (40), as the special forms of (35), (37), (38) applicable to this case,

$$f_1=\frac{\pi T}{\lambda^2 r}e^{i(nt-kr)}\left[K\Delta K^{-1}\frac{\alpha\gamma}{r^2}-\mu\Delta\mu^{-1}\frac{\gamma}{r}\right], \quad . \quad . \quad (41)$$

$$g_1=\frac{\pi T}{\lambda^2 r}e^{i(nt-kr)}\left[K\Delta K^{-1}\frac{\beta\gamma}{r^2}\right], \quad . \quad . \quad . \quad . \quad . \quad (42)$$

$$h_1=\frac{\pi T}{\lambda^2 r}e^{i(nt-kr)}\left[-K\Delta K^{-1}\frac{\alpha^2+\beta^2}{r^2}+\mu\Delta\mu^{-1}\frac{\alpha}{r}\right]. \quad (43)$$

If $\Delta\mu=0$, as we shall henceforward suppose, $f:g=\alpha:\beta$, showing that the electrical displacement is in the plane containing the secondary ray and the direction of primary electrical displacement (z), and

$$f_1^2+g_1^2+h_1^2 \propto \frac{\alpha^2+\beta^2}{r^2};$$

so that the intensity is proportional to the square of the sine of the angle between the secondary ray and the direction of the primary electrical displacement. The blue colour of the light scattered from small particles is explained by the occurrence of λ^2 in the denominators of the expressions for f_1, g_1, h_1; but for further particulars on this subject the reader must be referred to my previous papers.

Equations (35), (36), &c. are rigorously applicable, however large the region of disturbance, if the square of ΔK may really be neglected. From them we see that, under the circumstances in question, each element of a homogeneous obstacle acts independently as a centre of disturbance, and that the aggregate effect in any direction depends upon the phases of the elementary secondary disturbances as affected by the situation of the element along the paths of the primary and of the secondary light. In fact,

$$P=\Delta K^{-1}e^{int}\iiint e^{ikx}e^{-ikr}\,dx\,dy\,dz.$$

If θ, ϕ be the angles defining (in the usual notation) the direction of the secondary ray, and r_0 correspond to the origin of coordinates, we have

$$P=\Delta K^{-1}e^{i(nt-kr_0)}\iiint e^{ik(x+x\sin\theta\cos\phi+y\sin\theta\sin\phi+z\cos\theta)}\,dx\,dy\,dz; \quad (44)$$

and the question now before us for consideration is the value of the integral in (44) as dependent upon the size of the obstacle and the direction of the secondary ray. It is evident that the formulæ are applicable only when the whole retardation of the primary light in traversing the obstacle can be neglected in comparison with the wave-length; but if this condition be satisfied, there is no further limitation upon the size of the obstacle. In the case where the secondary ray forms the prolongation of the primary, or deviates sufficiently little from this direction, the exponential in (44) reduces to unity, signifying that every element of the obstacle acts alike, any retardation of phase at starting due to situation along the primary ray being balanced by an acceleration corresponding to a less distance to be travelled along the secondary ray. At a greater or less obliquity, according to the size of the obstacle, opposition of phase sets in; and at still greater obliquities the resultant can be found only by an exact integration. Its intensity is then less, and generally much less, than in the first case—a conclusion abundantly borne out by observation.

The simplest example of this kind is that afforded by an infinite cylinder (*e. g.* a fine spider-line), on which the light impinges perpendicularly to the axis, so that every thing takes places in two dimensions. This case is indeed not strictly covered by the preceding formulæ, on account of the infinite extension of the region of disturbance; but a moment's consideration will make it clear that each elementary column here acts according to the laws already described—that is to say, gives rise to a component disturbance whose phase is determined by the situation of the element along the primary and secondary rays. If the angle between the two rays be called χ, we have to consider the value of

$$\iint e^{ik(x+x\cos\chi+y\sin\chi)}\,dx\,dy.$$

Introducing polar coordinates r, θ, we find

$$x+x\cos\chi+y\sin\chi=2r\cos\tfrac{1}{2}\chi\ \cos(\theta-\tfrac{1}{2}\chi);$$

so that the integral

$$=\iint e^{ikr\,.\,2\cos\frac{1}{2}\chi\,.\,\cos\theta}\,r\,dr\,d\theta$$

$$=\int_0^a\int_0^{2\pi}\{\cos(2kr\cos\tfrac{1}{2}\chi\ \cos\theta)$$

$$+i\sin(2kr\cos\tfrac{1}{2}\chi\ \cos\theta)\}\,r\,dr\,d\theta$$

$$=2\pi\int_0^a \mathrm{J}_0(2kr\cos\tfrac{1}{2}\chi)\,r\,dr, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (45)$$

J_0 denoting the Bessel's function of zero order.

The integration with respect to r indicated in (45) can be effected by known properties of Bessel's functions; and the result is expressible by a function of the first order. We get

$$\frac{\pi a}{k\cos\frac{1}{2}\chi}\mathrm{J}_1(2ka\cos\tfrac{1}{2}\chi); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (46)$$

and J_1 is defined by

$$J_1(z)=\frac{z}{2}\left(1-\frac{z^2}{2.4}+\frac{z^4}{2.4^2.6}-\frac{z^6}{2.4^2.6^2.8}+\dots\right). \quad (47)$$

If $\cos\frac{1}{2}\chi=0$ (*i. e.* in the direction of original propagation), (46) becomes πa^2, every element of the area acting alike. This is the maximum value. When χ is such that

$$2ka\cos\tfrac{1}{2}\chi=\pi\times 1{\cdot}2197,$$

the secondary light vanishes, at a greater angle revives, then vanishes again, and so on, the angles being of course functions of the wave-length. If we conceive the cylinder to increase in size gradually from zero, the scattered light vanishes first in the backward direction $\chi=0$, in which direction evidently the greatest differences of phase occur. Every thing is determined by the course of the function J_1; and (46) within the limits of its application embodies the theory of Young's eriometer.

We will now consider the case of an obstacle in the form of a sphere. If z be a coordinate measured perpendicularly to the plane containing the primary and secondary rays, formula (46), multiplied by dz, will represent the effect of a slice o the sphere, whose radius is a and thickness dz, and what remains to be effected is merely the integration with respect to z. For this purpose we write $z=c\sin\phi$, $a=c\cos\phi$, where c is the radius of the sphere. The integral then takes the form

$$\frac{2\pi c^2}{k\cos\frac{1}{2}\chi}\int_0^{\frac{1}{2}\pi} J_1(2kc\cos\tfrac{1}{2}\chi\cos\phi)\cos^2\phi\,d\phi, \quad . \quad (48)$$

or, if we expand J_1 by (47), and integrate according to a known formula,

$$\frac{2\pi c^3}{3}\left\{2-\frac{m^2}{5}+\frac{m^4}{7.5.4}-\frac{m^6}{9.7.5.4.6}+\frac{m^8}{11.9.7.5.4.6.8}\dots\right\}, \quad (49)^*$$

in which m is written for $2kc\cos\frac{1}{2}\chi$. It will be understood that (49), after multiplication by $e^{int}\Delta K^{-1}$, gives merely the value of P in (36), and that to find the complete expression for the secondary light in any direction other factors must be introduced in accordance with (35), (37), (38). The angle χ,

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* July 15.—I find for the first root of (49), $m=4{\cdot}50$, giving as the smallest obliquity $(\pi-\chi)$ at which the secondary light vanishes,

$$\pi-\chi=2\sin^{-1}(4{\cdot}50/2kc).$$

being that included between the secondary ray and the axis of x, may be expressed by

$$\sin\chi=\sqrt{(\beta^2+\gamma^2)}\div r. \quad . \quad . \quad . \quad . \quad (50)$$

Our theory, as hitherto developed, shows that, whatever the shape and size of the particles, there is no scattered light in a direction parallel to the primary electric displacements, except such as may depend upon squares and higher powers of the difference of optical properties. In order to render an account of the "residual blue" observed by Tyndall when particles in their growth have reached a certain magnitude, it is necessary to pursue the approximation. By (28), with $\Delta\mu$ neglected, we have

$$\nabla^2 f_2+k^2f_2+\mathrm{K}\left(\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\right)(f_1\Delta\mathrm{K}^{-1})$$
$$-\mathrm{K}\frac{d^2}{dx\,dy}(g_1\Delta\mathrm{K}^{-1})-\mathrm{K}\frac{d^2}{dx\,dz}(h_1\Delta\mathrm{K}^{-1})=0, \quad (51)$$

and two similar equations in g_2 and h_2. On the supposition that f_1, g_1, h_1 are known throughout the region of disturbance, these equations may be solved in the same way as (29), (30), and (31). For the sake of brevity we may confine ourselves to the particular direction for which the terms of the first order vanish. Thus at a sufficient distance r' along the axis of z,

$$f_2=-\frac{k^2\mathrm{K}}{4\pi r'}\iiint f_1\Delta\mathrm{K}^{-1}e^{-ikr'}\,d\alpha\,d\beta\,d\gamma, \quad . \quad . \quad (52)$$

$$g_2=-\frac{k^2\mathrm{K}}{4\pi r'}\iiint g_1\Delta\mathrm{K}^{-1}e^{-ikr'}\,d\alpha\,d\beta\,d\gamma, \quad . \quad . \quad (53)$$

$$h_2=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

We have now to find the values of f_1 and g_1 within the region of disturbance, to which of course (35) &c. are not applicable. In the general solution (32), h_0 is a function of x only; so that the elements of the integral vanish in the *interior* of a homogeneous obstacle, and we have only to deal with the surface. Integrating by parts across this surface, we find

$$f_1=\frac{\mathrm{K}}{4\pi}\iiint\frac{d}{dz}(h_0\Delta\mathrm{K}^{-1})\frac{d}{dx}\left(\frac{e^{-ikr}}{r}\right)dx\,dy\,dz$$
$$=-\frac{\mathrm{K}}{4\pi}\frac{d}{d\alpha}\iiint\frac{d}{dz}(h_0\Delta\mathrm{K}^{-1})\,.\,\frac{e^{-ikr}}{r}\,dx\,dy\,dz, \quad (55)$$

r being a function of x and α only through $(\alpha-x)$. In like manner

$$g_1 = -\frac{K}{4\pi}\frac{d}{d\beta}\iiint \frac{d}{dz}(h_0\Delta K^{-1}) \, . \, \frac{e^{-ikr}}{r}\, dx\, dy\, dz. \quad . \quad . \quad (56)$$

In the case of a small homogeneous sphere, whose centre is taken as origin of coordinates, these formulæ lead to fairly simple results. The triple integral in (55), (56) may readily be exhibited in its real character of a surface-integral. Thus

$$\iiint \frac{d}{dz}(h_0\Delta K^{-1})\frac{e^{-ikr}}{r}\, dx\, dy\, dz = -\Delta K^{-1}\iint \frac{h_0 z}{c}\frac{e^{-ikr}}{r}\, dS, \quad (57)$$

where dS is an element of the surface whose radius is c. This applies to a sphere of any size; but we have now to introduce an approximation depending on the supposition that kc is small. As far as the first power of kc,

$$-\Delta K^{-1}\iint \frac{h_0 z}{c}\frac{e^{-ikr}}{r}\, dS = -\Delta K^{-1}\frac{e^{int}}{c}\iint\left(\frac{z+ikzx}{r} - ikz\right) dS$$

$$= -\Delta K^{-1}\frac{e^{int}}{c}\iint \frac{z+ikzx}{r}\, dS,$$

in which the double integral is the common potential of matter distributed over the spherical surface with density $(z+ikzx)$. Calling this for the moment V, we have (Thomson and Tait, 'Nat. Phil.' § 536) at any internal point (α, β, γ),

$$V = 4\pi c(\gamma + \tfrac{1}{3}ik\gamma\alpha);$$

so that

$$\iiint \frac{d}{dz}(h_0\Delta K^{-1})\frac{e^{-ikr'}}{r}\, dx\, dy\, dz$$

$$= -4\pi\Delta K^{-1}e^{int}(\gamma + \tfrac{1}{3}ik\gamma\alpha). \quad . \quad (58)$$

Thus by (55), (56),

$$f_1 = \tfrac{1}{3}K\Delta K^{-1}ik\gamma e^{int}, \quad g_1 = 0. \quad . \quad . \quad . \quad . \quad . \quad (59)$$

We are now prepared to calculate f_2, g_2 from (52), (53). These formulæ apply to both directions along the axis of z; but in what follows it will be convenient to suppose that it is the positive direction which is under consideration. In this case, if ρ denote the distance from the centre of the sphere, $r' = \rho - \gamma$ and $e^{-ikr'} = e^{-ik\rho}(1+ik\gamma)$ approximately; so that

$$f_2 = -\frac{k^2(K\Delta K^{-1})^2 e^{i(nt-k\rho)}}{12\pi\rho}\iiint ik\gamma(1+ik\gamma)\, d\alpha\, d\beta\, d\gamma$$

$$= \frac{k^4(K\Delta K^{-1})^2}{12\pi}\frac{e^{i(nt-k\rho)}}{\rho}\iiint \gamma^2\, d\alpha\, d\beta\, d\gamma;$$

or if, as before, T be the volume of the sphere,

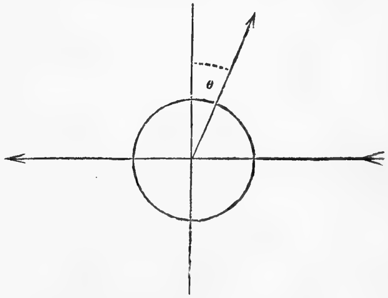
$$\left.\begin{aligned} f_2 &= \frac{\pi \mathrm{T}}{\lambda^2\rho}\, e^{i(nt-k\rho)}(\mathrm{K}\Delta\mathrm{K}^{-1})^2\frac{k^2c^2}{15}, \\ g_2 &= 0. \end{aligned}\right\} \quad . \quad . \quad (60)$$

Comparing (60) and (41), we see that the amplitude of the light scattered along z is not only of higher order in ΔK, but is also of the order k^2c^2 in comparison with that scattered in other directions. The incident light being white, the intensity of the component colours scattered along z varies as the inverse 8th power of the wave-length, so that the resultant light is a rich blue.

There is another point of importance to be noticed. Although when the terms of the second order are included the scattered light does not vanish along the axis of z, the peculiarity is not lost, but merely transferred to another direction. Putting together the terms of the first and second orders, we see that the scattered light will vanish in a direction in the plane of xz, inclined to z (towards x) at a small angle θ, such that

$$\theta = -\mathrm{K}\Delta\mathrm{K}^{-1}\frac{k^2c^2}{15} = \frac{\Delta\mathrm{K}}{\mathrm{K}}\,\frac{k^2c^2}{15}. \quad . \quad . \quad . \quad (61)$$

In the usual case of particles optically denser than the surrounding medium, ΔK is positive, from which we gather that the direction in which the scattered light vanishes to the second order of approximation is inclined backwards, so that the angle through which the light may be supposed to be bent by the action of the particle is *obtuse*.



The fact that, when the primary light is polarized, there is in one perpendicular direction no light scattered by very small particles, was stated by Stokes*; but it is, I believe, to Tyndall that we owe the observation that with somewhat larger particles the direction of minimum illumination becomes oblique. I do not find, however, any record of the direction of the obliquity (that is, of the sign of the small angle θ), and have therefore made a few observations for my own satisfaction.

In a darkened room a beam of sunlight was concentrated

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* Phil. Trans. 1852, § 183.

by a large lens of 2 or 3 feet focus; and in the path of the light was placed a beaker glass, containing a dilute solution of hyposulphite of soda. On the addition of a small quantity of dilute sulphuric acid a precipitate of sulphur slowly forms, and during its growth manifests exceedingly well the phenomena under consideration. The more dilute the solutions, the slower is the progress of the precipitation. A strength such that there is a delay of four or five minutes before any effect is apparent, will be found suitable; but no great nicety of adjustment is necessary. By addition of ammonia in sufficient quantity to neutralize the acid, the precipitation may be arrested at any desired stage. More time is thus obtained to complete the examination ; but the condition of things is not absolutely permanent, the already precipitated sulphur appearing to aggregate itself into large masses.

In the optical examination we may, if we prefer it, polarize the primary light; but it is usually more convenient to analyze the scattered light. In the early stages of the precipitation the polarization is complete in a perpendicular direction, and incomplete in oblique directions. After an interval the polarization begins to be incomplete in the perpendicular direction, the light which reaches the eye when the nicol is in the position of minimum transmission being of a beautiful blue, much richer than any thing that can be be seen in the earlier stages. This is the moment to examine whether there is a more complete polarization in a direction somewhat oblique ; and it is found that with θ positive there is in fact an oblique direction of more complete polarization, while with θ negative the polarization is more imperfect than in the perpendicular direction itself.

The polarization in a distinctly oblique direction, however, is not perfect, a feature for which more than one reason may be put forward. In the first place, with a given size of particles, the direction of complete polarization indicated by (61) is a function of the colour of the light, the value of θ being three or four times as large for the violet as for the red end of the spectrum. The experiment is, in fact, much improved by passing the primary light through a coloured glass held in the window-shutter. Not only is the oblique direction of maximum polarization more definite and the polarization itself more complete, but the observation is easier than with white light, by the uniformity of the colour of the light scattered in various directions. If we begin with a blue glass, we may observe the gradually increasing obliquity of the direction of maximum polarization; and then by exchanging the blue glass for a red one, we may revert to the original condition of things, and observe the transition from perpendicularity to obliquity over again. The change in the wave-length of the light has the same effect as a change in the size of the particles; and the comparison gives curious information as to the rate of growth.

But even with homogeneous light it would be unreasonable to expect an oblique direction of perfect polarization. So long as the particles are all very small in comparison with the wave-length, there is complete polarization in the perpendicular direction; but when the size is such that obliquity sets in, the degree of obliquity will vary with the size of the particles, and the polarization will be complete only on the very unlikely condition that the size is the same for them all. It must not be forgotten, too, that a very moderate increase in dimensions may carry the particles beyond the reach of our approximations.

The fact that at this stage the polarization is a maximum when the angle through which the light is turned *exceeds* a right angle is the more worthy of note, as the opposite result would probably have been expected. By Brewster's law this angle in the case of a plate is *less* than a right angle; so that not only is the law of polarization for a very small particle different from that applicable to a plate, but the first effect of an increase of size is to augment the difference.

We must remember that our recent results are limited to particles of a spherical form. It is not difficult to see that, for elongated particles, the terms in $(\Delta K)^2$ may be of the same order with respect to kc as the principal term; so that if $(\Delta K)^2$ be sensible, mere smallness of the particle will not secure complete evanescence of scattered light along z. The general solution of the problem for an infinitesimal particle of arbitrary shape must raise the same difficulties as beset the general determination of the induced magnetism developed in a piece of soft iron when placed in a uniform field of force. In the case of an ellipsoidal particle the problem is soluble; but it is perhaps premature to enter upon it, until experiment has indicated the existence of phenomena likely to be explained thereby.

For an infinitesimal particle in the form of a sphere, we may readily obtain the complete solution without any approximation depending upon the smallness of ΔK. We know by the analogous theory of magnetism, that a dielectric sphere situated in a uniform field of electric force will undergo electric displacement of uniform amount, and in a direction parallel to that of the force. Thus the complete solution applicable to an infinitely small sphere is obtained from (29), (30), (31) by writing h for h_0; where by h is denoted the actual displacement (parallel to z) within the particle, and by h_0 the displacement in the enveloping medium under the same electric force. If K′ be the specific inductive capacity for the particle, the ratio of $h : h_0$ is $3\mathrm{K}' : \mathrm{K}' + 2\mathrm{K}$; and in this ratio the results expressed in (41), (42), (43) are to be increased. If we extract the factors $\mathrm{K}\Delta\mathrm{K}^{-1}$ which there occur, we get

$$\frac{3\mathrm{K}'}{\mathrm{K}'+2\mathrm{K}}\mathrm{K}\Delta\mathrm{K}^{-1} = \frac{3\mathrm{K}'\mathrm{K}}{\mathrm{K}'+2\mathrm{K}}\left(\frac{1}{\mathrm{K}'}-\frac{1}{\mathrm{K}}\right) = -\frac{3(\mathrm{K}'-\mathrm{K})}{\mathrm{K}'+2\mathrm{K}};$$

so that

$$f = -\frac{3(\mathrm{K}'-\mathrm{K})}{\mathrm{K}'+2\mathrm{K}}\frac{\pi\mathrm{T}}{\lambda^2 r}\frac{\alpha\gamma}{r^2}e^{i(nt-kr)}, \text{\&c.} \quad . \quad . \quad . \quad (62)$$

We learn from (62) that our former result as to the evanescence of the secondary light along z is true for an infinitely small spherical particle to *all* orders of ΔK.

We will now return to the two-dimension problem with the view of determining the disturbance resulting from the impact of plane waves upon a cylindrical obstacle whose axis is parallel to the plane of the waves. There are, as in the problem of reflection from plane surfaces, two principal cases—(1) when the electric displacements are parallel to the axis of the cylinder taken as axis of z, (2) when the electric displacements are perpendicular to this direction.

Case 1.—From (12), with $\mathrm{C}=0$, $\mu=$ constant,

$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}\right)\frac{h}{\mathrm{K}} + n^2\mu\mathrm{K}\,\frac{h}{\mathrm{K}} = 0;$$

or if, as before, $k=2\pi/\lambda$,

$$\left(\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+k^2\right)\frac{h}{\mathrm{K}} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (63)$$

in which k is constant in each medium, but changes as we pass from one medium to another. From (63) we see that the problem now before us is analytically identical with that treated in my book on Sound, § 343, to which I must refer for more detailed explanations. The incident plane waves are represented by

$$e^{int}e^{ikx} = e^{int}e^{ikr\cos\theta}$$

$$= e^{int}\{\mathrm{J}_0(kr) + 2i\mathrm{J}_1(kr)\cos\theta + \ldots + 2i^m\mathrm{J}_m(kr)\cos m\theta + \ldots\}; \quad (64)$$

and we have to find for each value of m an internal motion finite at the centre, and an external motion representing a divergent wave, which shall in conjunction with (64) satisfy at the surface of the cylinder ($r=c$) the condition that the function and its differential coefficient with respect to r shall be continuous. The divergent wave is expressed by

$$\mathrm{B}_0\psi_0+\mathrm{B}_1\psi_1\cos\theta+\mathrm{B}_2\psi_2\cos 2\theta+\ldots,$$

where ψ_0, ψ_1, &c. are the functions of kr defined in § 341. The coefficients B are determined in accordance with

$$\mathrm{B}_m\left\{kc\,\frac{d\psi_m}{d\,.\,kc}\mathrm{J}_m(k'c)-k'c\,\psi_m\frac{d}{d\,.\,k'c}\mathrm{J}_m(k'c)\right\}$$
$$=2i^m\{k'c\,\mathrm{J}_m(kc)\,\mathrm{J}_m{}'(k'c)-kc\,\mathrm{J}_m(k'c)\,\mathrm{J}_m{}'(kc)\},$$

except in the case of $m=0$, when $2i^m$ on the right-hand side is to be replaced by i^m. In working out the result we suppose kc and $k'c$ to be small; and we find approximately for the secondary disturbance corresponding to (64)

$$\psi=\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}}e^{i(nt-kr)}\left[\frac{k'^2c^2-k^2c^2}{2}-\frac{k^2c^2(k'^2c^2-k^2c^2)}{8}\cos\theta\right];\quad(65)$$

showing, as was to be expected, that the leading term is independent of θ.

For case 2, which is of greater interest, we have from (15),

$$\left(\frac{d}{dx}\frac{1}{k^2}\frac{d}{dx}+\frac{d}{dy}\frac{1}{k^2}\frac{d}{dy}+1\right)c=0.\quad .\quad .\quad(66)^*$$

This is of the same form as (63) within a uniform medium, but gives a different boundary condition at a surface of transition. In both cases the function itself is to be continuous; but in that with which we are now concerned the second condition requires the continuity of the differential coefficient *after division by* k^2. The equation for B_m is therefore

$$\mathrm{B}_m\left\{k'c\frac{d\psi_m}{d\,.\,kc}\mathrm{J}_m(k'c)-kc\,\psi_m\frac{d\mathrm{J}_m(k'c)}{d\,.\,k'c}\right\}$$
$$=2i_m\{kc\,\mathrm{J}_m(kc)\,\mathrm{J}_m{}'(k'c)-k'c\,\mathrm{J}_m(k'c)\,\mathrm{J}_m{}'(kc)\},$$

with the understanding that the 2 is to be omitted when $m=0$. Corresponding to the primary wave $e^{i(nt+kx)}$, we find as the expression of the secondary at a great distance from the cylinder,

$$\psi=\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}}e^{i(nt-kr)}\left[-\frac{k^2c^2}{16}(k^2c^2-k'^2c^2)\right.$$
$$\left.-k^2c^2\frac{k'^2-k^2}{k'^2+k^2}\cos\theta-\frac{1}{8}k^4c^4\frac{k^2-k'^2}{k^2+k'^2}\cos 2\theta\right].\quad .\quad(67)$$

The term in $\cos\theta$ is now the leading term; so that the second-

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* In (66) c is the magnetic component, and not the radius of the cylinder. So many letters are employed in the electromagnetic theory, that it is difficult to hit upon a satisfactory notation.

ary disturbance approximately vanishes in the direction of the primary electrical displacements, agreeably with what has been proved before. It should be stated here that (67) is not complete to the order k^4c^4 in the term containing $\cos\theta$. The calculation of the part omitted is somewhat tedious in general; but if we introduce the supposition that the difference between k'^2 and k^2 is small, its effect is to bring in the factor $(1-\frac{1}{4}k^2c^2)$.

Extracting the factor (k'^2-k^2), we may conveniently write (67)

$$\psi = -k^2c\,\frac{k'^2-k^2}{k'^2+k^2}\left(\frac{\pi}{2ikr}\right)^{\frac{1}{2}} e^{i(nt-kr)}\left[\cos\theta - \frac{k'^2c^2+k^2c^2}{16} - \frac{k^2c^2}{8}\cos 2\theta\right], \quad . \quad (68)$$

in which

$$\cos\theta - \frac{k'^2c^2+k^2c^2}{16} - \frac{k^2c^2}{8}\cos 2\theta$$
$$= \cos\theta - \frac{k'^2c^2-k^2c^2}{16} - \frac{k^2c^2}{4}\cos^2\theta. \quad . \quad . \quad (69)$$

In the directions $\cos\theta=0$, the secondary light is thus not only of high order in kc, but is also of the second order in $(k'-k)$. For the direction in which the secondary light vanishes to the next approximation, we have

$$\tfrac{1}{2}\pi-\theta = \tfrac{1}{16}(k'^2c^2-k^2c^2) = \frac{k^2c^2}{16}\,\frac{\mathrm{K}'-\mathrm{K}}{\mathrm{K}}. \quad . \quad . \quad (70)$$

This corresponds to (61) for the sphere; and is true if kc, $k'c$ be small enough, whatever may be the relation of k' and k. For the cylinder, as for the sphere, the direction is such that the primary light would be bent through an angle *greater* than a right angle.

If we neglect the square of (k'^2-k^2), the complete expression corresponding to (69) is

$$\cos\theta(1-\tfrac{1}{4}k^2c^2)-\tfrac{1}{4}k^2c^2\cos^2\theta = \cos\theta[1-\tfrac{1}{4}k^2c^2-\tfrac{1}{4}k^2c^2\cos\theta].$$

This may be compared with the value obtained by the former method, viz. $\cos\theta\,\mathrm{J}_1(2kc\cos\frac{1}{2}\theta)\div kc\cos\frac{1}{2}\theta$, and will be found to agree with it as far as the square of kc.

If we suppose the cylinder to be extremely small, we may confine ourselves to the leading terms in (65) and (67). Let us compare the intensities of the secondary lights emitted in the two cases along $\theta=0$, *i. e.* directly backwards. From (65)

$$\psi \propto \frac{k'^2c^2-k^2c^2}{2},$$

while from (67)

$$\psi \propto -k^2c^2\,\frac{k'^2-k^2}{k'^2+k^2}.$$

The opposition of sign is apparent only, and relates to the different methods of measurement adopted in the two cases. In (65) the primary and secondary disturbances are represented by h/K, but in (67) by the magnetic function c. If we express the solution in the second case in terms of the electric function g, we shall find (see 13) that the ratio of c to g changes sign when we pass from the primary light propagated along $-x$ to the secondary light propagated along $+x$. The actual ratio of amplitudes in the two cases is thus $(k'^2+k^2)/2k^2$, or $(\mathrm{K}'+\mathrm{K})/2\mathrm{K}$. Unless the difference between K′ and K be neglected, the two components of unpolarized light are scattered along this direction in different proportions, that component preponderating in which the electric displacement is parallel to the axis of the cylinder. The secondary light is therefore partially polarized in the plane perpendicular to the axis.

June 1881.

XI. *An Abstract of the Results obtained in a Recalculation of the Atomic Weights.* *By* FRANK WIGGLESWORTH CLARKE, *S.B., Professor of Chemistry in the University of Cincinnati*[*].

DURING the past three years I have been engaged upon a recalculation of all the atomic-weight determinations which have been published from the time of Berzelius's earlier investigations down to the present date. My purpose has been to reduce all similar series of experiments to common standards, to calculate the probable error of each series, to combine the results into general means, and then to deduce the atomic weights in such a way that each value should represent a fair average of all the trustworthy estimations. In other words, I have sought to bring together all the vast number of scattered details, and to derive from them a more consistent table of atomic weights than has hitherto been found in chemical literature. My complete work will appear in due time as a separate volume; my present intention is to give merely a summary of my methods, and my conclusions.

Taking hydrogen as unity, I necessarily began with the ratio between it and oxygen. This ratio has been determined accurately in only two ways:—first, by the synthesis of water over copper oxide; and secondly, from the relative density of the two gases. Ignoring earlier inexact experiments, we may consider only the data furnished by Dumas, by Erdmann and Marchand, and by Regnault. From Dumas's nineteen syntheses of water we get for oxygen values ranging

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[*] Communicated by the Author.

from O=15·892 to O=16·024. The mean of all is 15·9607, with a probable error of ±·007. Erdmann and Marchand give eight results, which average O=15·9733±·0113. The general mean from both investigations is

O=15·9642±·006.

The density of hydrogen, referred to air as unity, was determined by Regnault in three experiments. The mean was ·069263; ±·000019. For oxygen four determinations were made by Regnault, and one of them was rejected. The three remaining figures give a density of 1·105633; ±·000008. The ratio between these two density-estimations gives an atomic weight of O=15·9628; ±·0044. Combining this with the value found from the synthesis of water, we get a general mean of

O=15·9633; ±·0035.

This is the most probable value which can be deduced from the published data, and it forms the corner-stone upon which our entire system of atomic weights must rest. I need not discuss here the methods employed in the calculation of probable errors; for they are given in all treatises upon least squares. Suffice it to say, that I assign no arbitrary weights to the values under examination; each series of experiments receives the weight to which the probable error of its mean entitles it.

Having found a value for oxygen, I next discussed in a group the atomic weights of the elements chlorine, bromine, iodine, silver, potassium, sodium, and sulphur. The data to be considered were determined by Berzelius, Penny, Pelouze, Marignac, Maumené, Gerhardt, Millon, Struve, Svanberg and Struve, Turner, Dumas, Cooke, and Stas, and represented twenty distinct ratios. For example, one of the most important ratios was that between potassium-chloride and potassium-chlorate, for which there were nine series of determinations. The mean of each series was calculated, together with its probable error; and then all nine means were combined into one general mean. Thus all the available data were reduced to the twenty compact ratios above referred to. Two of these may be cited, to illustrate the methods of calculation.

1. Percentage of O in $KClO_3$, 39·154; ±·0004.
2. Ag : KCl : : 100 : 69·1032; ±·0002.

From the first of these ratios the molecular weight of KCl is easily calculated by the usual proportion 39·154 : 60·846 : : $O_3 : \chi$.

Using the value for oxygen previously found, χ, or KCl, became 74·4217; and as the probable errors are known for the three known terms of the proportion, we easily deduce that of the fourth term, and write

KCl=74·4217 ; ±·0164.

Here the probable error is a function of the probable errors of the experiments upon potassium-chlorate, and the probable error of the atomic weight of oxygen. We may now use this value KCl in connexion with the second of the ratios above cited, and deduce for the atomic weight of silver the figure Ag=107·696 ; ±·024. In every proportion used there are known probable errors for three terms, and they are involved in the probable error of the fourth term. From the twenty ratios above referred to, eight entirely independent values for the atomic weight of silver can be found. Each value has a definitely ascertained probable error, and each receives the weight which that error indicates. The general mean of all is

Ag=107·675±·0096.

This is the final result from the discussion of over two hundred experiments; and it gives us the key to the atomic weights of the other elements in the group under consideration. These are as follows, when O=15·9633±·0035 :—

Cl = 35·370 ; ±·014.
Br = 79·768 ; ±·019.
I = 126·557 ; ±·022.
K = 39·019 ; ±·012.
Na = 22·998 ; ±·011.
S = 31·984 ; ±·012.

The values which Stas assigns to these elements are all based upon the standard of O=16. If we adapt the above figures to the same standard, we may compare them side by side with those of Stas. As the latter chemist also determined the atomic weights of nitrogen, lithium, and lead, I will include the values found in this recalculation for these substances also.

Atomic Weights with O=16.

	New values.	Stas's values.	Difference.
Ag.........	107·923	107·930	—·007
Cl	35·451	35·457	—·006
Br	79·951	79·952	—·001
I	126·848	126·850	—·002
Na.........	23·051	23·043	+·008
K	39·109	39·137	—·028
S	32·058	32·074	—·016
N	14·029	14·044	—·015
Li	7·0235	7·022	+·0015
Pb	206·946	206·926	+·020

The magnificent accuracy of Stas's manipulations could hardly receive more striking confirmation than is afforded by these figures.

The atomic weights of the remaining elements I give below in the order of their calculation. I also give the authorities whose experiments are combined in the values presented. Elaborate details there is, of course, no room for. They must be sought in the complete monograph when the latter is published.

Nitrogen.—Data by Regnault, Pelouze, Marignac, Turner, Penny, and Stas. General mean of all, N=14·0210; ±·0035. This value involved the values previously found for O, K, Na, Ag, and Cl.

Carbon.—Data by Liebig and Redtenbacher, Maumené, Dumas and Stas, Erdmann and Marchand, and Stas. General mean of all, C=12·0021; ±·0019. Rejecting the series by Liebig and Redtenbacher, and by Maumené, which involve constant errors, we get from the remaining series a value of C=11·9736; ±·0028. If O=16, this becomes C=12·0011. The ratio between oxygen and carbon, therefore, is a ratio between two whole numbers, 16 and 12.

Barium.—Determined by Berzelius, Turner, Struve, Pelouze, Marignac, Dumas, and Stas. General mean, Ba=136·763; ±·031. If O=16, Ba=137·007.

Strontium.—Data by Pelouze, Dumas, and Marignac. General mean, Sr=87·374; ±·032.

Calcium.—Experiments by Berzelius, Erdmann and Marchand, and Dumas. Ca=39·990; ±·010. The best single result by Erdmann and Marchand gives Ca=39·905; or, if O=16, Ca=39·997.

Lead.—Berzelius, Turner, Marignac, Dumas, Anderson, and Stas. Some data were rejected. The final, most probable value is Pb=206·471; ±·021.

Fluorine.—Berzelius, Louyet, Dumas, and De Luca. General mean, F=18·984; ±·007.

Phosphorus.—The only good determinations for this element are those by Schrötter and by Dumas. General mean of both series, P=30·958; ±·007.

Boron.—Berzelius, Laurent, Dumas, and Deville. In mean, B=10·941; ±·023. Not well determined.

Silicon.—Pelouze, Dumas, and Schiel. Si=28·195; ±·031. This constant needs thorough experimental revision.

Lithium.—Mellet, Diehl, Troost, and Stas. General mean, Li=7·0073; ±·007.

Rubidium.—Bunsen, Piccard, Godeffroy. Rb=85·251; ±·0045.

Cæsium.—Johnson and Allen, Bunsen, and Godeffroy. Cs=132·583 ; ±·024.

Thallium.—Although I have recalculated the results obtained by Lamy, Hebberling, and Werther, I need consider here only the experiments of Crookes. His weighings, calculated with the values for O and N given above, make Tl=203·715 ; ±·037. Crookes himself, using the value NO_3=61·889, found Tl=203·642 and regards his results as evidence against Prout's hypothesis. His experiments, however, really fix only the ratio between NO_3 and Tl. If NO_3=62, then Tl=204·008 ; that is, the ratio which Crookes has rigorously established is a ratio between two whole numbers, and is confirmatory of Prout's idea.

Glucinum.—The results of Awdejew, Klatzo, Weeren, and Debray have high probable errors, and practically vanish from the mean when combined with those of Nilson and Pettersson, The weighings published by the latter chemists give me Gl=9·085±·0055, or Gl=13·628±·008, according to whether the oxide is GlO or Gl_2O_3.

Magnesium.—Scheerer, Svanberg, and Nordenfeldt, Bahr, Jacquelain, Marchand and Scheerer, and Dumas. General mean of all, Mg=24·103 ; ±·004. Some of the series are vitiated by constant errors. The best value is to be deduced from Marchand and Scheerer's work alone, and is Mg=23·959 ; ±·005.

Zinc.—Jacquelain, Favre, and Axel Erdmann. General mean, Zn=65·557 ; ±·011. Erdmann's results alone, however, give a more probable value (considered chemically) of Zn=64·9045 ; ±·019.

Cadmium.—Von Hauer, Lenssen, and Dumas. General mean, Cd=111·770 ; ±·030.

Mercury.—Part of Turner's data are to be considered, together with results by Erdmann and Marchand, Millon, and Svanberg. General mean, Hg=199·712 ; ±·042. New determinations are much needed.

Chromium.—Peligot's work was not available for discussion. The data studied were by Berlin, Moberg, Lefort, Wildenstein, Kessler, and Siewert. General mean from all, Cr=52·453 ; ±·015. Berlin's work, considered by itself, gives Cr=52·389 ; ±·019. Siewert's results give Cr=52·009 ; ±·025. I regard the last value as freest from constant errors, and use it in subsequent calculations.

Manganese.—Turner, Berzelius, Dumas, v. Hauer, Schneider, Rawack. General mean, Mn=54·128 ; ±·011. Schneider's and Rawack's results give a better value of Mn=53·906 ; ±·012.

Iron.—Berzelius, Svanberg, and Norlin, Erdmann and Marchand, Maumené, Rivot, and Dumas. General mean, Fe=55·913 ; ±·012.

Copper.—Berzelius, Erdmann and Marchand, Millon and Commaille, and Hampe. Cu=63·173 ; ±·011.

Molybdenum.—Debray, Dumas, Rammelsberg, and analyses by Liechti and Kemp. Mo=95·527 ; ±·051.

Tungsten.—Berzelius, Schneider, Marchand, v. Borch, Dumas, Bernoulli, Persoz, Roscoe, Scheibler, and Zettnow. General mean of all, W=183·610 ; ±·032.

Uranium.—Peligot, Ebelmen, and Wertheim. General mean, U=238·481 ; ±·082.

Aluminum.—Berzelius, Tissier, Isnard, Dumas, Terreil, and Mallet. General mean, Al=27·0092 ; ±·0028. All the data except Mallet's might be rejected without essentially affecting this value.

Gold.—Berzelius and Levol. Au=196·155 ; ±·095. A thorough redetermination is much needed.

Nickel.—Several early series of nickel and cobalt were rejected. The data taken were by Schneider, Dumas, Russell, Sommaruga, Winkler, and Lee. General mean, Ni=58·547 ; ±·009. I attach more importance to the concordant results of Schneider, Sommaruga, and Lee, whose figures give a general mean of Ni=57·928 ; ±·022. Lee's investigation was of all the least susceptible to constant errors.

Cobalt.—Schneider, Dumas, Russell, Sommaruga, Winkler, Weselsky, and Lee. Co=58·887 ; ±·008.

Selenium.—Berzelius, Sacc, Erdmann, and Marchand, Dumas, and Ekman and Pettersson. Se=78·797 ; ±·011.

Tellurium.—Berzelius, v. Hauer, and Wills. General mean, Te=127·960 ; ±·034. The results of v. Hauer and Wills upon K_2TeBr_6 give Te=127·170 ; ±·173. Wills's minimum figures give me Te=126·07. In all of these results certain constant errors are possible ; so that the question raised by Mendelejeff as to whether tellurium is above or below iodine, cannot be regarded as settled.

Vanadium.—Roscoe's weighings, recalculated with the new values for O, Ag, and Cl, give V=51·256 ; ±·024.

Arsenic.—Pelouze, Dumas, Wallace, and Kessler. General mean, As=74·918 ; ±·016. Wallace's analyses of $AsBr_3$ were made to establish the atomic weight of bromine ; but they serve a better purpose here.

Antimony.—For this element there have been the two rival figures 120 and 122. The general mean from data by Kessler, Dumas, and Dexter is Sb=122·092 ; ±·035. The general mean from the experiments of Schneider and of Cooke is

Sb=119·955 ; ±·036. In view of the recent discussions upon the subject, we may regard the lower figure as established.

Bismuth.—General mean from Schneider's experiments Bi=207·523 ; ±·082. If O=16, this becomes 208·001. All of Dumas's results on bismuth chloride give a mean of Bi=210·464 ; the figures which he considers best give Bi=209·78. Schneider's work is probably nearest correct, his method being less liable to constant errors than that of Dumas.

Tin.—Berzelius, Mulder and Vlaanderen, Dumas, and Vlaanderen. General mean, Sn=117·698 ; ±·040.

Titanium.—Rose's weighings give Ti = 48·710 ± ·105. Mosander's figures give Ti=47·045. Pierre's give Ti=49·889 ±·096 ; and Demoly's, Ti=52·191±153. A general mean of the results of Pierre, Rose, and Demoly is 49·846±·064. Mosander's work must be rejected for want of sufficient details. An experimental revision is wanted.

Zirconium.—From Berzelius's figures, Zr=89·255 ; ±·039. From Marignac's data Zr=90·328 ; ±·113. The general mean of both sets is Zr=89·367 ; ±·037. New determinations are evidently needed.

Thorium.—Berzelius, Chydenius, Berlin, Delafontaine, Hermann, and Cleve. General mean, Th=233·414 ; ±·073.

Gallium.—From Boisbaudran's results, in mean, Ga= 68·854.

Indium.—Reich and Richter, Winkler, and Bunsen. General mean, In=113·398 ; ±·047.

Cerium.—Beringer, Hermann, Marignac, Bunsen and Jegel, Rammelsberg, Wolf, Wing, and Buehrig. General mean of all, Ce=140·424 ; ±·017. Buehrig's analyses of the oxalate give Ce=141·198 ; ±·020. The figures by Wolf and by Wing give Ce=137·724. Wolf and Wing had a *white* ceroso-ceric oxide ; and Wolf suspects the ordinary yellowish compound to contain a fourth metal of the cerium group. Buehrig's work is the best ; but the possible presence of a fourth metal is not considered by this chemist. Therefore new experiments are needed.

Lanthanum.—Hermann, Rammelsberg, Marignac, Czudnowicz, Holzmann, Zschiesche, Erk, and Cleve. General mean, La=138·526 ; ±·030.

Didymium.—Marignac, Hermann, Zschiesche, Erk, and Cleve. General mean, Di=144·573 ; ±·031. Cleve's work alone, which is doubtless the best, gives Di=146·804.

Scandium.—Nilson's results, recalculated, give Sc = 43·980 ; ±·015.

Yttrium.—Popp, Delafontaine, Bahr and Bunsen, and Cleve.

General mean, Yt=97·616. Excluding Popp's work as worthless, the general mean becomes Yt=89·816 ; ±·067.

Ytterbium.—Nilson. Yb=172·761 ; ±·038.

Erbium.—For this metal the most probable results are those of Cleve, which give, recalculated, Er=165·891. Previous workers undoubtedly studied material rich in ytterbium and other metals of the group.

For terbium, samarium, phillipium, decipium, thulium, &c. there are no satisfactory data. Cleve gives 170·7 for thulium, while Delafontaine puts Ph=123 to 125, and Dp=171. These figures assume that all the earths of the group are sesquioxides.

*Columbium**.—Marignac's results give approximately Cb=94 ; Blomstrand regards 95 as the most probable value. New estimations are needed.

Tantalum.—All of Marignac's figures give a general mean of Ta=182·144 ; ±·166. Probably 182 may be safely used as the true value.

Platinum.—By a recalculation of the results lately published by Seubert, I get Pt=194·415 ; ±·049.

Osmium.—Berzelius's figures give me Os=198·494.

Iridium.—The general mean calculated from Seubert's weighings is Ir=192·651 ; ±·033.

Palladium.—Berzelius's last results are the only ones worth considering. They give Pd=105·737.

Rhodium.—Data by Berzelius. Rh=104·055.

Ruthenium.—A single analysis of potassium ruthenio-chloride by Claus gives Ru=104·217. Plainly the values for Ru, Rh, Pd, and Os need scrupulous redetermination.

Conclusions.

A careful scrutiny of all the data upon which the foregoing atomic-weight calculations depend will reveal various sources of error. Of course, each series of results must be considered by itself and weighed on its own merits ; but a few general errors are important enough to warrant mention here.

First, every value after oxygen, with one or two partial exceptions, involves whatever error may attach to the atomic weight of oxygen. If the latter is 16 instead of 15·9633, this error in some instances becomes multiplied to a serious extent, as a glance at the tabulated results will show. Other similar errors are repeated continually. The value assigned to any element is necessarily affected by whatever errors may attach to the atomic weights of those other elements through whose medium it is referred to the standard, hydrogen.

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* This name has priority over the generally accepted "niobium," and therefore is entitled to preference.

Secondly, confusion arises from the fact that many of the weighings under discussion were not reduced to the standard of a vacuum; while others had been subjected to such a correction. The errors thus introduced into the calculations are small, but still they cannot be lost sight of.

Another set of errors of unknown magnitude are produced as follows:—Many series of experiments, notably in the work of Stas, Marignac, and other eminent investigators, involve the titration of chlorides, bromides, or iodides with solutions containing known weights of metallic silver. But Dumas has lately shown that silver, purified after the usual methods, occludes weighable quantities of oxygen. In other words, the silver hitherto employed in atomic-weight investigations has not been pure silver. One exception is found in Mallet's research upon aluminum. Since the atomic weights of nearly all the elements depend directly or indirectly upon silver, this source of error becomes of the gravest importance. Analogous errors may possibly occur with metals other than silver, and should be carefully looked for. For example, the atomic weights of both copper and oxygen depend upon the reduction of copper-oxide in hydrogen. If the residual copper, which is weighed, occludes any hydrogen, then the atomic weight of copper will come out too high, and that of oxygen a trifle too low. Such an error might account for the difference of between 15·9633 and 16 in the case of oxygen.

In connexion with the data discussed in this investigation, it is perhaps worth while to consider the bearing of the results upon Prout's famous hypothesis. In order to simplify matters I tabulate the new atomic-weight values in two columns, one containing numbers referred to hydrogen as unity, the other with figures comparable with oxygen as equal to 16.

Table of Atomic Weights.

	H=1.	O=16.		H=1.	O=16.
Hydrogen	1·0000	1·0023	Cæsium	132·583	132·918
Fluorine	18·984	19·027	Silver	197·675	107·023
Chlorine	35·370	35·451	Thallium	203·715	204·183
Bromine	79·768	79·951	Phosphorus	30·958	31·029
Iodine	126·557	126·848	Vanadium	51·256	51·373
Lithium	7·0073	7·0235	Arsenic	74·918	75·090
Glucinum	9·085	9·106	Cadmium	111·770	112·027
Magnesium	23·959	24.014	Mercury	199·712	200·171
Zinc	64·905	65·054	Calcium	39·990	40·082
Sodium	22·998	23·051	Strontium	87·374	87·575
Potassium	39·019	39·109	Barium	136·763	137·007
Rubidium	85·251	85·529	Lead	206·471	206·946

Table of Atomic Weights (*continued*).

	H=1.	O=16.		H=1.	O=16.
Oxygen	15·9633	16·0000	Tantalum	182·144	182·562
Sulphur	31·984	32·074	Scandium	43·980	44·081
Selenium	78·797	78·978	Yttrium	89·816	90·023
Tellurium	127·960	128·254	Erbium	165·891	166·273
Chromium	52·009	52·129	Ytterbium	172·761	173·158
Molybdenum	95·527	95·747	Cerium	140·424	140·747
Tungsten	183·610	184·032	Lanthanum	138·526	138·844
Uranium	238·482	239·030	Didymium	144·573	144·906
Manganese	53·906	54·029	Carbon	11·9736	12·0011
Iron	55·913	56·042	Silicon	28·195	28·260
Nickel	57·928	58·062	Titanium	49·846	49·961
Cobalt	58·887	59·023	Zirconium	89·367	89·573
Copper	63·173	63·318	Tin	117·698	117·968
Boron	10·941	10·966	Thorium	233·414	233·951
Aluminum	27·009	27·075	Platinum	194·415	194·867
Gallium	68·854	68·963	Iridium	192·651	193·094
Indium	113·398	113·659	Osmium	198·494	198 951
Nitrogen	14·021	14·029	Palladium	105·737	105·981
Antimony	119·955	120·231	Rhodium	104·055	104·285
Bismuth	207·523	208·001	Ruthenium	104·217	104·457
Columbium	about 94	about 94	Gold	196·155	196·606

Here we have sixty-six elements, or, rejecting columbium as too vaguely determined, sixty-five. Such elements as phillipium, decipium, thulium, samarium, &c. are not yet sufficiently well known to be considered in this connexion.

In his recent superb investigation of the atomic weight of aluminum, Mallet makes substantially the following argument in favour of Prout's hypothesis. Considering the atomic weights of eighteen elements only as well determined, he finds that ten of them have values varying less that 0·1 from whole numbers. In other words, these ten elements have atomic weights varying from even multiples of that of hydrogen by insignificant amounts. What is the probability that this agreement with Prout's hypothesis in ten cases out of eighteen is purely accidental, as those hold who agree with the views of Stas? Working this problem out, he finds the probability of mere coincidence to be 1 : 1097·8; and he concludes that Prout's hypothesis is still worthy of careful consideration.

Applying Mallet's reasoning to the Table of atomic weights now before us, we find that in the first column, when H=1, twenty-five out of sixty-five elements have atomic weights falling within one tenth of a unit of whole numbers; but many of the figures which fall outside this limit of variation involve the variation of oxygen multiplied many times over. We must therefore study the second column, which assumes O=16. Here we have thirty-nine elements falling within the limit of variation assigned byMallet and twenty-six falling without. The latter are chlorine, iodine, potassium, thallium, glucinum, mercury, strontium, tellurium, chromium, molybdenum, rubidium, copper, indium, vanadium, antimony, tantalum, erbium, ytterbium, cerium, lanthanum, silicon, zirconium, platinum, rhodium, ruthenium, and gold. Of these, chlorine, rubidium, and strontium agree closely with half multiples of hydrogen; while tellurium, molybdenum, iridium, tantalum, erbium, cerium, silicon, zirconium, lanthanum, rhodium, and ruthenium may be dismissed from consideration as not sufficiently well determined to bear upon the problem before us. The exceptions in the cases of potassium, iodine, thallium, glucinum, mercury, chromium, vanadium, antimony, ytterbium, platinum, and gold remain to be considered.

For potassium and iodine, we must remember that both of these elements involve the constant error due to the occlusion of oxygen by silver. This error is probably great enough to throw the values for both elements outside the limit of variation above assigned. For thallium it has already been shown that when its atomic weight is calculated with $NO_3=62$, Crookes's data give $Tl=204{\cdot}008$. The atomic weights of glucinum and ytterbium, as given in the Tables, are calculated from analyses of the sulphates. If $SO_3=80$, then $Gl=9{\cdot}096$; and $Yb=173{\cdot}016$. Both fall within, and one *narrowly* within the limit of one tenth of a unit variation. In the case of platinum, I need only say that Seubert's figures give values ranging both above and below the even number 195; while as for antimony, although the general mean is $Sb=120{\cdot}231$, Crookes's analyses of the bromide, reckoned with $Br=80$, give almost exactly 120 for the value. For mercury, chromium, vanadium, and gold, new determinations are desirable.

Enough has been said to show that none of the apparent exceptions to Prout's hypothesis are absolutely inexplicable. As the figures actually stand, thirty-nine out of sixty-five elements vary less than a tenth of a unit each from even multiples of the atomic weights of hydrogen. Of the remaining twenty-six, three conform to half multiples, three more are legitimately recalculable so as to fall within bounds, and eleven have been so defectively determined that the assigned values can carry scarcely any weight. The remaining nine are still subject to slight revision. In short, the many agreements, which include three fourths of the *well*-determined atomic weights, renders Prout's hypothesis very highly probable. It is more likely that the seeming exceptions are due to undetected constant errors, than that the great number of coincidences should be accidental. The mathematical probability in favour of Prout's hypothesis I have not yet calculated;

but a glance at Mallet's figures will show that it must be enormous.

As I said at the beginning, this paper is but a summary of methods and conclusions. Within its scope elaborate discussions would not be admissible. I hope that my complete memoir will soon be published; and it is my intention to discuss, in an appendix to it, both the bearing of the results upon Prout's hypothesis, and the distribution of the variations therefrom. I ought to say, that at the beginning of my investigation I was strongly prejudiced against Prout's hypothesis, and fully believed that it had been for ever overthrown. My results have forced me to give it very respectful consideration.

XII. *On the Opacity of Tourmaline Crystals.*

By Professor SILVANUS P. THOMPSON, *B.A., D.Sc.*[*]

Introduction.

1. TOURMALINE is distinguished amongst crystals for its remarkable optical properties, particularly its power of polarizing light. It is distinguished moreover by possessing characteristic electric properties. It possesses also a crystallographic interest as furnishing an eminent example of non-superposable hemihedry. There can be little doubt that these remarkable and characteristic qualities are closely related to one another, though as yet very little is known of the nature of this probable connexion. In the present paper an attempt is made to connect the optical and electrical properties of the crystal, by showing that the opacity of the crystal to light polarized in a principal plane of section can be deduced from its electric conductivity.

2. In a paper read before Section A of the British Association at Dublin in 1878, by Dr. O. J. Lodge and myself[†], we suggested, as a possible explanation of the phenomena of pyroelectricity in tourmaline, that it might be found to possess unilateral conductivity for electricity—and if for electricity, for heat also. Our experiments on the electric conductivity, however, led to negative results; and in the case of heat-conductivity, the only differences observed of a unilateral kind were such as occurred while the temperature was either rising or falling—not whilst it was constant. Our original suggestion, therefore, was not confirmed, though I obtained instead

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[*] Communicated by the Physical Society, having been read at the Meeting on June 11.

[†] Report Brit. Assoc. (Dublin) 1878, p. 495; and Phil. Mag. July 1879.

a result which amounted to the discovery of a new phenomenon, "*the Convection of Heat by Electricity*"—namely, that in a pyroelectric crystal whose temperature is rising, heat flows more easily *with* the electricity than it does *against* the electricity. We were later indebted to Mr. G. F. Fitzgerald, of Trinity College, Dublin, for the suggestion that an analogous phenomenon might occur, namely that there might be a unilateral electric convection whilst the electromotive force producing the flow was rising or falling*.

3. Whilst we were pursuing these investigations, and before we abandoned our original hypothesis, the suggestion was made to me by Dr. Lodge that if tourmaline possessed unilateral conductivity *along* the axis, this might afford a possible explanation of the greater opacity observed in coloured tourmalines for one set of rays. For if, as on our original hypothesis, the elasticity in one sense along the axis were different from the elasticity in the opposite sense along the axis, vibrations taking place along the axis would be stopped, and the only rays *transmitted* would be those vibrating at right angles to the axis. Unfortunately for this suggestion, the ray whose vibrations are executed in a plane at right angles to the axis is the ordinary ray, which is the one to which the tourmaline is opaque; whilst the ray which it transmits is the extraordinary ray, which, being polarized in a plane at right angles to the axis, is propagated by vibrations (according to Fresnel and Stokes) executed in a principal plane of section. I could not, therefore, agree with this suggestion as to the cause of opacity in tourmaline crystals, though the suggestion that the opacity was involved in the electrical properties of the crystal appeared an extremely likely one.

4. A theory of the opacity of tourmaline crystals, however, has recently occurred to me whilst considering the general relations of electricity and light; and I now beg to offer the following explanation, based upon Clerk Maxwell's electromagnetic theory of light.

According to Maxwell's theory, light is an electromagnetic disturbance propagated in the same medium which transmits other electromagnetic actions, the periodic vibrations being propagated in a wave through media possessing "electric elas-

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* We have not yet put this suggestion to experimental proof. In any case the experiment would be difficult, for the reasons alleged in our paper of 1878. Moreover the question would still be complicated by phenomena of heating; for, as we pointed out in 1878, the phenomena of pyroelectricity must be reversible, and the application of an external electromotive force to a crystal must produce in it a thermal change just the reverse of that which would itself give rise to the electromotive force in question.

ticity" (*i. e.* dielectric properties), but being frittered down into electric currents (and ultimately into heat) when these vibrations pass into conducting media. Good insulators should therefore be transparent, and good conductors opaque, to light. *If it can be shown that tourmaline crystals are better conductors of electricity in one direction than another, it can be deduced as a consequence of Maxwell's theory that they will be better transmitters of light in one direction than in another, and that they will absorb more of those rays of light whose vibrations, consisting of electric displacements, lie in the direction of best electric conductivity.*

The present paper consists of three parts:—

I. A *résumé* of the Optical, Electrical, and Magnetic properties of Tourmaline Crystals.

II. A Mathematical Theory of the Opacity of Crystals which are electrically or magnetically æolotropic.

III. A discussion of the Electric Conductivity of Tourmaline Crystals, and of the Experimental methods of observing it.

I. *Physical Properties of Tourmaline Crystals.*

5. *Optical Properties.*—Tourmaline belongs to the rhombohedral system of crystals, and is therefore optically a uniaxal crystal. Its refractive indices are (approximately)

Ordinary ray	1·6406,
Extraordinary ray	1·6212;

and it is therefore a negative crystal. Tourmalines are of various colours, occasionally colourless, still more rarely pink (rubellite), but frequently bluish-green, green, olive, or brown. The commonest of all kinds is of a brownish tint when cut into very thin slices, but appears exteriorly jet-black. Those of bluish-green or green hue are, according to M. Mascart, the most strongly pyroelectric; and of these the most transparent are more highly pyroelectric than the opaque varieties. *In ordinary light a coloured tourmaline is always found to be much more opaque to rays traversing it in a direction parallel to its axis than to rays traversing it at right angles to its axis, equal thicknesses being considered.* Tourmaline possesses the property of polarizing partially or wholly the light which it transmits. Its action on light is usually explained by saying that a ray of ordinary light incident upon the crystal is divided into two portions, the ordinary and the extraordinary ray, which in passing through the crystal are unequally absorbed. If a slice of the crystal be taken whose parallel faces are principal planes of section, and which is of a suitable thickness, the ordinary ray will be virtually suppressed, whilst the extraordinary ray is transmitted, though with some loss by absorption. The transmitted ray is polarized in a plane at right angles to the axis of the crystal; and its vibrations are therefore executed (if we assume with Fresnel and Stokes that the vibrations are at right angles to the plane of polarization) in a direction parallel with that of the axis of the crystal.

We may remark parenthetically, that the almost complete opacity of coloured tourmalines to rays travelling along the axis affords a strong confirmation of the views of Fresnel and Stokes. For let this axis be the x-axis of a system of rectangular coordinates, the transverse vibrations to a ray travelling along x must be in planes parallel to the plane (yz); and they are suppressed by absorption whether parallel to y or z. Now for a ray travelling along y the vibrations are in planes parallel to the plane (xz), and the z-component is suppressed by absorption, the x-component only being transmitted; while for a ray travelling along the axis of z the vibrations are in planes parallel to (yx), and, the y-component being suppressed by absorption, the x-component is alone transmitted.

6. *Electrical Properties.*—With the characteristic pyroelectric properties of tourmaline we have not much concern here; for the tourmalines which are most highly pyroelectric appear to be least opaque.

The electric conductivity of tourmaline is in general very small. The resistance of a centimetre cube of the crystal at ordinary temperatures is certainly many thousand ohms. Gaugain states that the resistance decreases rapidly if the temperature is raised to 400° or 500°, and he has observed* some crystals, on being cooled from this temperature, to retain a high surface-conductivity, which he attributed to their *having become* hygroscopic—an explanation which seems somewhat doubtful, since he states that washing them in ordinary water rendered them non-conducting as before. Becquerel sought to explain the disappearance of pyroelectric phenomena at temperatures above 150° C. by supposing an increase of conductivity to be assumed at that temperature. In the researches of Dr. Lodge and myself in 1878–9 we found no such great decrease in resistance, which at 300° C. was still enormous. We were not able to compare the conductivity in a direction at right angles to the axis with that along the axis.

The only experiment directed toward the comparison of the electric conductivities in different directions with respect to the axis of the crystal, are those of de Senarmont† and of G.

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* *Annales de Chimie et de Physique*, 3e série, t. vii. p. 8.

† Ibid. t. xxviii. (1850), p. 257.

Wiedemann*, both of whom examined the conducting-power of the surfaces of crystals. Their methods and results are examined in detail in the concluding part of this paper. They each found the conductivity of tourmaline to be greater along the axis than across the axis: I shall have occasion for showing that this conclusion is fallacious.

7. *Magnetic Properties.*—Plücker examined the magnetic deportment of tourmaline. The following account of what was observed is given by Tyndall†:—"A plate of the crystal which had been prepared for the purposes of polarization, 12 millimetres long, 9 wide, and 3 thick, was suspended by a silk fibre between the poles of an electromagnet. On sending a current round the latter, the plate, which was magnetic, set itself as an ordinary magnetic substance would do, with its longest dimension from pole to pole. The optic axis of the crystal, thus suspended, was vertical. . . . . . On hanging the crystal, however, with its optic axis horizontal, when the magnet was excited, the plate stood no longer as a magnetic substance, but as a diamagnetic; its longest dimension being at right angles to the line joining the poles. The optic axis of the crystal was found to coincide with its length; and the peculiar deportment was considered as a proof that the optic axis was repelled." From all which the logical inference is, that the coefficient of magnetic induction is less along the axis than in a direction at right angles to it. It is curious that Professor Tyndall states‡ just the opposite to this—that the maximum magnetic induction is, in coloured crystals at all events, in a direction parallel to the axis.

8. *Thermal Conductivity.*—De Senarmont examined the thermal conductivity of tourmaline by his well-known method. The conductivity is greater across the axis than along it, the ratio between the major and minor axes of the isothermal curves being (according to my own observations) about 3 : 2; and these being proportional to the square roots of the conductivities, we have

$$3 : 2 = \sqrt{k_2} : \sqrt{k_1},$$

or

$$\textit{conductivity along axis} : \textit{conductivity across axis} = 4 : 9.$$

The unilateral conductivity observed in my experiments during rise or fall of temperature need not be further noticed here.

9. *Radiating-power.*—Kirchhoff§ and Stewart‖ have shown

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* *Ann. de Chim. et de Phys.* t. xxix. (1850), p. 229; and Pogg. *Ann.* lxxvi. p. 404 and lxxvii. p. 534.

† 'Diamagnetism,' p. 2.

‡ *Op. cit.* p. 71.

§ Pogg. *Ann.* cix. 1860, p. 275.

‖ Proc. Roy. Soc. x. p. 503.

that the law of equality of power to absorb and emit radiations extends to the case of the light absorbed by the tourmaline, which, when heated to redness, emits rays partially polarized in a principal plane of section—that is to say, emits those rays most freely which it most freely absorbs.

II. *Mathematical Theory.*

10. Let K, μ, and C stand respectively for the dielectric inductive capacity, the magnetic inductive capacity, and the conductivity per unit of volume of a medium. In crystalline media these quantities will, in general, have different values in three principal rectangular directions, being no longer mere scalar quantities, but linear (and vector) operators upon their several functions. If these values be specified for any crystal, then the optical properties of that crystal may be deduced from the general equations of electromagnetic disturbances. These equations are found in Maxwell's 'Electricity and Magnetism,' art. 783 (7), as follows:—

$$\left.\begin{aligned}\mu\left(4\pi C+K\frac{d}{dt}\right)\left(\frac{dF}{dt}+\frac{d\psi}{dx}\right)+\nabla^2F+\frac{dJ}{dx}&=0,\\ \mu\left(4\pi C+K\frac{d}{dt}\right)\left(\frac{dG}{dt}+\frac{d\psi}{dy}\right)+\nabla^2G+\frac{dJ}{dy}&=0,\\ \mu\left(4\pi C+K\frac{d}{dt}\right)\left(\frac{dH}{dt}+\frac{d\psi}{dz}\right)+\nabla^2H+\frac{dJ}{dz}&=0,\end{aligned}\right\}\quad . \; . \; (1)$$

where

$$J=\frac{dF}{dx}+\frac{dG}{dy}+\frac{dH}{dz},$$

and where F, G, H are the components of vector-potential at the point (x, y, z) in three rectangular directions parallel to the x, y, and z axes respectively, and where ψ represents the electric (scalar) potential of the possible free electricity. These general equations apply to the case of a medium in which both electric displacements and true conduction currents are simultaneously produced under the action of an impressed electromotive force. They cease to apply in strictness to the case where the medium possesses movements of its own, which we are not here considering. If the medium be homogeneous, whether æolotropic or isotropic, and if its conductivity undergoes no discontinuity in its values at any point (that is to say, has the same continuous values in the same direction), there will be no reason to expect the phenomenon of electric absorption to occur, or free electrification to appear at any internal point of the medium. Hence, in considering steady periodic disturbances such as light-waves, we may neglect terms involving only functions of the free electricity, or its volume-density, such as $\nabla^2\psi$, since these will be independent of t.

11. In order further to simplify the equations, we will narrow down the case to that of a plane wave of unpolarized light propagated along the axis of z, and therefore lying in planes parallel to the plane (xy), in which planes also the electric and magnetic disturbances are executed. We will further suppose the crystal under consideration to be, as tourmaline is, a uniaxal crystal, and to have its optic axis coincident with the axis of x, so that the x and y components of the disturbances will be respectively parallel and at right angles to the optic axis. We shall also make the *assumption* that in tourmaline the vectors, or linear vector operators, which should stand for K, μ, and C, may be represented with sufficient accuracy by assigning to these quantities the values

$$\begin{matrix} K_1, & K_2, & K_3, \\ \mu_1, & \mu_2, & \mu_3, \\ C_1, & C_2, & C_3, \end{matrix}$$

for their respective values as measured in the directions of x, y, and z.

We may then write, neglecting functions of J, which in the case of periodic disturbances can at most be a linear function of the time,

$$\left.\begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} + \mu_1 K_1 \frac{d^2F}{dt^2} + \nabla^2 F = 0, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} + \mu_2 K_2 \frac{d^2G}{dt^2} + \nabla^2 G = 0, \\ 4\pi\mu_3 C_3 \frac{dH}{dt} + \mu_3 K_3 \frac{d^2H}{dt^2} + \nabla^2 H = 0. \end{aligned}\right\} \quad . \quad . \quad (2)$$

Since, however, we are going to deal only with disturbances in the plane (xy) propagated along the axis of z, we may simplify the above to the following form:—

$$\left.\begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} + \mu_1 K_1 \frac{d^2F}{dt^2} - \frac{d^2F}{dz^2} = 0, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} + \mu_2 K_2 \frac{d^2G}{dt^2} - \frac{d^2G}{dz^2} = 0, \\ 4\pi\mu_3 C_3 \frac{dH}{dt} + \mu_3 K_3 \frac{d^2H}{dt^2} \qquad\quad = 0. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (3)$$

Or, in words, the rate of change in the transverse components of electromotive force as the wave advances along the axis of z is expressed as the sum of two time-functions of the corresponding components of vector-potential—one of these functions varying with the electric conductivity of the medium, the other with its dielectric capacity, and both being proportional also to the magnetic inductive capacity.

12. We have now four possible cases to consider:—

(i) where K is very great and C relatively negligible, as in non-conducting media;
(ii) where C is great and K relatively small, as in true conductors of electricity;
(iii) where C and K are magnitudes of comparable order, but having different values in different directions.
(iv) where μ has values differing appreciably in different directions.

Although from Plücker's experiments (see § 7 *antè*) it appears that μ_1 and μ_2 cannot be regarded as equal, yet their values are both approximately so nearly equal to unity that we are not in a position to discuss case (iv).

13. Case i. (*Non-conducting Substance*), C=0.

Equations (2) become

$$\left.\begin{aligned} \mu_1 K_1 \frac{d^2F}{dt^2} - \frac{d^2F}{dz^2} &= 0, \\ \mu_2 K_2 \frac{d^2G}{dt^2} - \frac{d^2G}{dz^2} &= 0, \\ \mu_3 K_3 \frac{d^2H}{dt^2} \qquad\quad &= 0; \end{aligned}\right\} \quad \ldots\ldots \quad (4)$$

whence

$$\left.\begin{aligned} \frac{1}{\mu_1 K_1} \cdot \frac{d^2F}{dz^2} &= \frac{d^2F}{dt^2}, \\ \frac{1}{\mu_2 K_2} \cdot \frac{d^2G}{dz^2} &= \frac{d^2G}{dt^2}; \end{aligned}\right\} \quad \ldots\ldots \quad (5)$$

whence it follows that a plane wave in the plane (xy), whose electric displacements are parallel to x, is propagated with velocity V_1; whilst displacements parallel to y are propagated in a wave whose velocity is V_2 with the following values:—

$$\left.\begin{aligned} V_1 &= \frac{1}{\sqrt{\mu_1 K_1}}, \\ V_2 &= \frac{1}{\sqrt{\mu_2 K_2}}. \end{aligned}\right\} \quad \ldots\ldots \quad (6)$$

This agrees with the known fact that a wave of light is split on passing through the crystal into two portions whose vibrations are respectively at right angles to one another, and which move with different velocities.

Now assuming, as above, $\mu_1=\mu_2=1$, we have

$$\frac{V_1}{V_2}=\frac{\sqrt{K_2}}{\sqrt{K_1}}=\frac{\rho_2}{\rho_1}; \quad \ldots\ldots \quad (7)$$

where ρ_2, ρ_1 are respectively the indices of refraction for ordinary and extraordinary rays.

Billet gives (*Optique Physique*, p. 619) the following values for the refractive indices of different specimens of tourmaline:—

Colour of ray.	Ray used.	Ordinary index (ρ_2).	Extraordinary index (ρ_1).	$\frac{\rho_2}{\rho_1}$.
Colourless ...	Ray D	1·6366	1·6193	1·0107
Green	Ray D	1·6408	1·6203	1·01265
Green-blue...	Red ray	1·6415	1·623	1·01311
Blue	Red ray	1·6435	1·6222	1·01313
Mean		1·6406	1·6212	1·0120

14. From these we deduce for the two velocities of light in tourmaline:—

Ordinary Ray.

$$V_2=\frac{3\times10^{10}\text{ cm. per sec.}}{1\cdot6406}=1\cdot8286\times10^{10}\text{ cm. per sec.};$$

and

Extraordinary Ray.

$$V_1=\frac{3\times10^{10}\text{ cm. per sec.}}{1\cdot6212}=1\cdot8504\times10^{10}\text{ cm. per sec.}$$

15. The two corresponding dielectric capacities should therefore be

$K_1=\rho_1^2=2\cdot6283$ in direction parallel to crystallographic axis,

$K_2=\rho_2^2=2\cdot6792$ in direction perpendicular to crystallographic axis.

I am not aware of any experimental determination of these quantities having been made.

16. Case ii. (*Conducting Medium*).

Here $\psi=0$, and we may neglect the terms containing K; for, though $K>1$, C is by hypothesis much greater, and the terms containing C are therefore those by which the mode of propagation of electric displacements in the medium are represented. So the equations (3) become

$$\left.\begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} - \frac{d^2F}{dz^2} &= 0, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} - \frac{d^2G}{dz^2} &= 0, \\ 4\pi\mu_2 C_3 \frac{dH}{dt} \qquad\quad &= 0, \end{aligned}\right\} \quad \cdots\cdots \quad (8)$$

which are not the equations of any kind of wave-propagation, but represent as going on in the plane (xy) the diffusion of electricity by conduction through the medium at logarithmic rates whose values along the x and y axes are proportional to $-\mu_1 C_1$ and $-\mu_2 C_2$ respectively; that is to say, the disturbances will be propagated just in the same fashion as heat would be in a medium whose thermal conductivities and specific heats had different values in different directions. The spread of electricity in this case resembles therefore that of the spread of heat in a thin film when the heat starts at a point and diffuses around. In its generality the case is therefore comparable, as Maxwell points out, to that of which Fourier gave the complete solution (art. 384, 'Analytical Theory of Heat,' p. 382, Freeman's translation), for the diffusion of heat in all directions, where the temperature is represented by the triple integral

$$\iiint \frac{d\alpha\, d\beta\, d\gamma}{2^3\sqrt{\pi^3 k^3 t^3}} \cdot \epsilon^{-\frac{(\alpha-x)^2+(\beta-y)^2+(\gamma-z)^2}{4kt}} \cdot f(\alpha, \beta, \gamma),$$

in which, if we write $r=\sqrt{(\alpha-x)^2+(\beta-y)^2+(\gamma-z)^2}$, the exponential $\epsilon^{-r^2/4kt}$ will represent the value contributed to the mean temperature at the point (x, y, z) whose distance from the origin is r. In the present case of diffusion of electric currents in the plane (xy), a similarly constructed double integral may be employed to represent the distribution of vector-potential, the quantity symbolized by k (the coefficient of the intrinsic rate of diffusion) being replaced by $\frac{1}{4\pi\mu_1 C_1}$ and $\frac{1}{4\pi\mu_2 C_2}$ in the exponential.

Now, where the whole energy of the electromagnetic disturbances is thus converted in the conducting medium into the energy of currents diffused through its substance, no wave will be propagated; or, as Maxwell has shown, the substance will be opaque to light, the energy of the diffused currents being frittered away into heat within the conducting medium by reason of the resistance encountered.

17. Case iii. (*Medium possessing both Dielectric Inductive Capacity and Conductivity, the values of* K *and* C *being of comparable order of magnitude, but having different values in different directions*).

We have (from § 11) the equations

$$\left.\begin{aligned} 4\pi\mu_1 C_1 \frac{dF}{dt} + \mu_1 K_1 \frac{d^2F}{dt^2} &= \frac{d^2F}{dz^2}, \\ 4\pi\mu_2 C_2 \frac{dG}{dt} + \mu_2 K_2 \frac{d^2G}{dt^2} &= \frac{d^2G}{dz^2}, \\ 4\pi\mu_3 C_3 \frac{dH}{dt} + \mu_3 K_3 \frac{d^2H}{dt^2} &= 0. \end{aligned}\right\} \quad . \quad . \quad (3\,bis)$$

F and G being the components of vector-potential in the x and y directions respectively, the problem is to determine at what rate these will be diminished by absorption in passing through a given thickness z of the medium when the conductivities per unit volume in those directions are respectively C_1 and C_2. The proportion borne by the transmitted electromagnetic displacements of the luminous wave to the original displacements, after traversing a thickness z of the crystal, will (neglecting the small proportion lost by surface reflexion) be expressed by an exponential of the form

$$\epsilon^{-pz}.$$

where p is the coefficient of absorption. This coefficient Maxwell has calculated (art. 798) for the case of isotropic conducting media, from the general fundamental equation, on the assumption that the disturbance may be expressed as a circular function of the form cos $(nt-qz)$. Following the lines thus laid down, we will write:—

$$\left.\begin{aligned} F &= \epsilon^{-p_1 z} \cos \frac{2\pi}{\lambda_1}(V_1 t - z), \\ G &= \epsilon^{-p_2 z} \cos \frac{2\pi}{\lambda_2}(V_2 t - z), \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where p_1 and p_2 are coefficients of absorption in the x and y directions respectively, and where V_1 and V_2 are the two velocities of propagation (see § 13), and where λ_1 and λ_2 are the corresponding lengths of waves. Now, putting

$$\left.\begin{aligned} q_1 &= \frac{2\pi}{\lambda_1}, \\ q_2 &= \frac{2\pi}{\lambda_2}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and

$$\left.\begin{aligned} n_1&=\frac{2\pi V_1}{\lambda_1}, \\ n_2&=\frac{2\pi V_2}{\lambda_2}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

whence

$$\left.\begin{aligned} V_1&=\frac{n_1}{q_1}, \\ V_2&=\frac{n_2}{q_2}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

we may write equations (9) in the form

$$\left.\begin{aligned} F&=\epsilon^{-p_1 z}\cos(n_1 t-q_1 z), \\ G&=\epsilon^{-p_2 z}\cos(n_2 t-q_2 z); \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (13)$$

in which case the expressions (9) will satisfy the differential equations (3), provided that

$$\left.\begin{aligned} 2p_1 q_1&=4\pi\mu_1 C_1 n_1, \\ 2p_2 q_2&=4\pi\mu_2 C_2 n_2. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

It follows at once that the coefficients of absorption are

$$\left.\begin{aligned} p_1&=2\pi\mu_1 C_1 V_1, \\ p_2&=2\pi\mu_2 C_2 V_2. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Here C_1 and C_2 are volume-coefficients of conductivity, and would require to be replaced by their values l/bzR_1 and b/lzR_2 if a given rectangular plate of the substance, of length l (parallel to x), breadth b, and thickness z, were measured, and found to have resistances of R_1 and R_2 respectively in directions parallel to x and y. In the actual case of the tourmaline, however, in which no measurements of resistance have yet been made, the volume-conductivities C_1 and C_2 may stand for conductivities in general. And the proportions of the incident (unpolarized) light transmitted by the crystal will be as follow:—

Ordinary ray (polarized in a plane (xz) of principal section; electric displacements parallel to y, magnetic displacements parallel to x),

$$\text{proportion transmitted } =\epsilon^{-2p_2 z}=\epsilon^{-4\pi\mu_2 C_2 V_2 z}.$$

Extraordinary ray (polarized in a plane (yz) perpendicular to the optic axis; electric displacements parallel to x, magnetic displacements parallel to y),

$$\text{proportion transmitted } =\epsilon^{-2p_1 z}=\epsilon^{-4\pi\mu_1 C_1 V_1 z}.$$

If O represent the intensity of the transmitted ordinary ray and E that of the transmitted extraordinary ray, we have

$$\frac{\mathrm{O}}{\mathrm{E}}=\frac{\epsilon^{-\mu_2 \mathrm{C}_2 \mathrm{V}_2}}{\epsilon^{-\mu_1 \mathrm{C}_1 \mathrm{V}_1}}, \quad \ldots\ldots\ldots \quad (16)$$

whence

$$\frac{\log \mathrm{O}}{\log \mathrm{E}}=\frac{\mu_1 \mathrm{C}\, \mathrm{V}_1}{\mu_2 \mathrm{C}_2 \mathrm{V}_2}. \quad \ldots\ldots \quad (17)$$

Now the ratio of V_1 to V_2 is known (§ 14), the velocity of the ordinary ray V_2 being to that of the extraordinary ray V_1 in the ratio 1·8286 to 1·8504. If, therefore, the magnetic permeability were equal both along and across the axis of the crystal, and if the electric conductivity were equal in both directions, the relative intensities of the two transmitted rays would depend only on the relative velocities; and as V_1 is greater than V_2, the ordinary ray should be more freely transmitted than the extraordinary ray, which is contrary to observation. The conductivities therefore cannot be equal in fact.

Billet's values (§ 13) for the refractive indices of tourmalines of different colours show slight differences, the colourless tourmaline being apparently a little less refringent than the coloured crystals. As in this case there was no opacity, the presumption is that both C_1 and C_2 were $= 0$. If the fact were confirmed by observation generally that the index of refraction is less in crystals exhibiting no coloration, we should be obliged to consider K as no longer independent of C in the general equations of electromagnetic disturbances; and V, which is taken $=(\mu K)^{-\frac{1}{2}}$, would require a more complicated formula for its complete expression.

18. If, again, either the coefficient of magnetic induction or the conductivity be greater along the axis of the crystal than across it, then the ordinary ray should be transmitted more freely than the extraordinary. This is not the fact with *any* tourmalines; it is always the ordinary ray which suffers the greater absorption.

As I understand Plücker's experiment on the diamagnetism of tourmaline (§ 7 *antè*), the crystal examined by that distinguished physicist possessed a greater magnetic inductive capacity in a direction *across* the crystallographic axis than in the direction of that axis. If this observation were also confirmed for tourmalines in general, we should have some reason to expect that crystals possessing electric conductivity, even if the conductivity were equal in all directions, should show greater absorption for the ordinary ray; for if $C_1=C_2$, equation (15) becomes

$$\frac{\log \mathrm{O}}{\log \mathrm{E}}=\frac{\mu_1 \mathrm{V}_1}{\mu_2 \mathrm{V}_2}. \qquad (18)$$

But μ_1 and μ_2 are so nearly equal to 1 in all cases hitherto observed, that the ratio of μ_1 to μ_2 is far less than that of V_2 to V_1; and therefore the greater opacity to the ray cannot be accounted for on the supposition that it depends on differences in the magnetic inductive capacity.

19. There appears, then, to be no other explanation of the observed opacity of coloured tourmaline crystals for rays polarized in a principal plane of section than that afforded by the hypothesis that the electric conductivity is greater in directions at right angles to the crystallographic axis than along it. One fact of some importance bearing upon this point is an observation which I have made, that the more opaque the crystal in general the greater is the inequality of absorption of the ordinary and extraordinary rays. The green tourmaline "C" in my possession, which is about 2 millim. thick, transmits both rays partially; but cuts off about three fourths of their intensity from ordinary rays, and above half their intensity from extraordinary rays. It is about 30 millim. in length, and appears to be perfectly opaque in that direction. On the other hand, a slice of a jet-black tourmaline, the property of University College, Bristol, not more than 0·2 millim. in thickness, is perfectly opaque to the ordinary ray; but it transmits extraordinary rays at about half of their intensity. Not only must the conductivity as a whole be higher in the latter case, but the ratio of the two conductivities must be higher in the latter than in the former; and the transverse conductivity must be greater than the longitudinal. The latter crystal was too small to permit of verification of these inferences by direct experiment.

III. *Electric Conductivity of Tourmaline Crystals.*

20. The conducting-power of the tourmaline for electricity is very feeble; and the only determinations that have been made of its relative conducting-powers in longitudinal and transverse directions are determinations, not of the flux of electricity through a slab or film of the substance, but of the superficial conductivity along a face of the crystal. This superficial conductivity has been examined by two slightly different processes, by de Senarmont* and by Wiedemann†.

21. De Senarmont's process consisted in covering the face

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* *Ann. de Chim. et de Phys.* 3e série, t. xxviii. p. 257.

† Ibid. t. xxix. p. 229; and Pogg. *Ann.* lxxvi. p. 404, and lxxvii. p. 534.

of the crystal with tinfoil through which a truly circular hole had been punched. The foil was fixed over the crystal with gum or varnish, and was connected to earth. At the centre of the exposed circle a metallic point was fixed in an insulating support; and by this means the electricity stored in a Leyden jar could be made to flow across the circular space from the centre to the circumference. With powerful discharges from a Leyden battery sparks leapt along the crystal face, leaving a permanent trace along the path of least resistance. Later experiments were conducted in a partial vacuum, when the discharge took place quietly in luminous streaks of a pale-violet colour along that diameter of the circle which corresponded to best conductivity along the surface.

The crystals operated upon by de Senarmont were three black tourmalines from Greenland, in the collection of the École des Mines, having plane mirror-like faces tangential to the lateral edges of the primitive rhombohedron and slightly striated ("stries à peine sensibles"). With circles of three different sizes, de Senarmont found the maximum conductivity to lie in a direction parallel to the axis.

22. Wiedemann's process consisted in producing an electric "figure" upon the surface of the crystal, which was dusted over with powdered sulphur, or lycopodium, or red-lead, and then exposed to the discharge of electricity from a finely-pointed conductor close to the surface. The powder was repelled from the point, and heaped itself all round a clear space. In the case of equal conductivities this space was a circle; in the case of unequal conductivities an ellipse whose major axis indicated the greater conductivity. Wiedemann only examined one specimen of tourmaline. He states that "on the faintly striated prismatic faces of the crystal employed, the larger diameter of the electric figure shows itself parallel to the principal axis." He adds, that in the case of other optically negative crystals (including calc-spar) the electric conductivity is greater along than across the axis. (He notes *rutile* as an exception.) But he appends, as a general conclusion, that the electricity is propagated in crystals more rapidly in that direction in which the propagation of light is the more rapid; which dictum exactly contradicts his own observations on calc-spar and on tourmaline, in which the velocity of the extraordinary ray is greater than that of the ordinary ray

23. Now, if we are to accept these observations on the superficial conduction of natural tourmaline crystals with striated faces as establishing the electric conductivity of tourmaline to be a maximum along the axis and a minimum across

it, we shall have to admit that, of all substances examined, tourmaline forms the solitary exception to the rule that the thermal conductivity and the electric conductivity of substances go together *pari passu**. There is no uncertainty as to the thermal conductivity of tourmaline, its transverse conductivity being about twice as great (see § 8 *antè*) as its longitudinal conductivity.

24. Both observers stated that the faces of the crystals were *striated*. De Senarmont appeared, indeed, to think that natural striation interfered less with the regularity of conduction than the invisible striations of an artificially polished face†. I am inclined to think from my own observations that, on the contrary, the natural striations of the crystal greatly affect the conditions of surface-conductivity. I have repeated both methods of experiment upon tourmalines with natural and with artificially-cut faces. In the former case, the crystals tried were black striated crystals from Norway. They showed, as did the crystals used by de Senarmont and by Wiedemann, a longitudinal conductivity higher than the transverse conductivity. I cleaned one of these crystals, first with hydrochloric acid, and then by rubbing its surface in boiling paraffin. When cold it was carefully cleaned with a piece of dry leather, and the experiments were repeated. De Senarmont's method now showed a transverse conductivity, while Wiedemann's method gave an indecisive result.

25. I therefore tried another method, as follows :—A small circular drop of pure olive-oil was deposited upon the middle of the face of the crystal. A pointed needle held in a non-conducting support was fixed centrally above it, and then the knob of a charged Leyden jar was cautiously approached. The oil-drop spread out somewhat irregularly across the surface; and at its centre appeared a clear space from which the oil was repelled. This clear space was distinctly elliptical, the major axis of the ellipse being transversely placed across the axis of the crystal. The natural striations, however, rendered the form irregular. When the artificially-cut green tourmaline ("C") mentioned in § 19 was employed, the elliptical space in the centre of the oil-drop was much more satis-

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* See Tait, Trans. Roy. Soc. Edinb. 1878, where, however, it appears that German silver, while superior in thermal conductivity to lead, is inferior to that metal in electric conductivity, being the single known exception to Forbes's rule.

† "Quant aux stries et aux aspérités naturelles, elles ne semblent pas avoir une influence appréciable tant qu'elles ne sont pas extrêmement prononcées. J'ai donc autant que possible, pour être sûr des résultats, cherché à opérer sur des faces naturelles ou obtenus par le clivage." *Loc. cit.* p. 264.

factorily observed. The ratio of the minor axis to the major axis was not greater than 1 : 2.

It is greatly to be wished that some more exact and more reliable measurements were made of the conductivity of tourmaline in the two principal directions along the axis and across it. Although these more reliable measurements are still wanting, I am disposed to think that my own experiments, backed by the analogy of the thermal conductivity (about which there is no uncertainty whatever), are sufficient to establish qualitatively that the electric conductivity of tourmaline is greater in directions at right angles to the axis than in the direction of the axis. If this be established, the theory of opacity advanced above is proved.

26. There is, indeed, one other possible way of accounting for opacity in a homogeneous medium besides the supposition made above, that the medium should possess electric conductivity of one kind or another. This alternative way of explaining opacity supposes that a luminous oscillation may pass into a calorific one of the same period. The vibrations of light being regarded as due to rapid alternations in vector-potential, the particles of a medium transmitting the luminous waves must be subjected to alternately directed electromotive forces. Now let there be in the medium a particle of matter whose natural period of oscillation coincides with that of the wave. This particle will be subjected to alternate inductions which will displace electricity in it from side to side, the (electro-) kinetic energy of the movement changing to (electro-) potential energy and back again to the kinetic form at each half swing, as in the alternations of the swinging pendulum. There is no difficulty in understanding that, under this action, the mass of the molecule will be set swinging. It is easy to set a pendulum, consisting of a metal ball hanging by a silk thread, into vibration by subjecting it to periodic electric inductions whose period agrees with that of the pendulum. Thus the (electro-)kinetic energy of the wave passes over into the (pondero-)kinetic energy of the oscillating particle; which is merely another way of saying that the light is absorbed and changed into heat. This action, however, can only account for absorption on the assumption that the medium is homogeneous, and its individual particles themselves conductors. But if the medium be homogeneous, all its particles will have similar periods of oscillation. This hypothesis may therefore account for selective absorption in media, though it is not adequate to explain general absorption. If the medium be not homogeneous, a *third* explanation of opacity is possible. In a *non-homogeneous* medium there is no need to suppose conductivity to exist; for heterogeneity of structure affords in

itself a possible explanation of electric absorption; and if electric absorption were possible in the medium, the variations of vector-potential would no longer be propagated without loss through the medium, any more than they can be through a medium possessing true conductivity. And if, by reason of structural differences in different directions (differences of fibre, lamination, cleavage, and the like), the degree of electric absorption were different in different directions, then such a medium might possess also a directed opacity. But such an opacity would be limited by the limitations of electric absorption in general, and would probably be quite distinguishable from the opacity arising from conduction in the medium. Were electric absorption possible, we could no longer omit functions of ψ, the (scalar) electric potential, from the general equations of electromagnetic disturbances assumed in § 10 and in the succeeding development of the theory.

27. The main argument for rejecting the supposition that electric absorption can account for opacity is derived from the consideration that such a medium, after having absorbed charges of opposed signs at different surfaces of its heterogeneous structure, would, when left to itself, slowly recover; and would give rise to phenomena of recovery, variations of vector-potential taking place in the medium in inverse order after the lapse of time. Whether this action may furnish a possible explanation of the phenomena of phosphorescence (*Nachleuchtung*), is a suggestion which may be worth future consideration. Whether the optical properties of tourmaline are to be regarded as dependent on its conductivity or on any other physical property, the physical property on which they depend is certainly a directed property; and its action is such that the crystal possesses what Maxwell called "electric elasticity" in a longitudinal and not in a transverse direction. Now, as those bodies which possess "electric elasticity" are observed for the most part to phosphoresce under the impact of electrified molecules in the negative glow produced in very attenuated vacua such as Crookes has obtained, whilst those bodies which do not possess this property do not phosphoresce under the same circumstances, I venture to predict that, if a slice of tourmaline cut parallel to the axis be found to phosphoresce at all when exposed to the negative discharge, its phosphorescence will be found to consist chiefly of rays polarized in a plane at right angles to the axis—or, in other words, that it will emit the same kind of rays as it transmits. Whether this prediction be verified or not, however, does not affect the validity of the theory of opacity of tourmaline crystals here advanced.

June 4, 1881.

XIII. *Electrostatic Investigations, especially relating to the Division of Induction in the Differential Inductometer and in the Electrophorus.* *By* Dr. JAMES MOSER*.

The Differential Inductometer.

IN the year 1838 Faraday completed his experiments on Specific Inductive Capacity, thus virtually repeating the investigations of Cavendish, published some sixty years before. At the end of his communication Faraday gave a description of an apparatus, the "Differential Inductometer," for the determination of such inductive capacity.

This serves as the point of departure for the following investigation; but I shall first, and chiefly, consider the electrostatic induction in a single medium, namely the atmospheric air; so that the question of "specific induction," a question involving more than one medium, will be considered later.

Faraday, in his 'Experimental Researches' (§ 1307), describes the above-mentioned apparatus as follows:—"Three circular brass plates about five inches in diameter were mounted side by side upon insulating pillars. The middle one 'A' was a fixture; but the outer plates 'B' and 'C' were moveable on slides, so that all three could be brought with their sides almost into contact or separated to any required distance. Two gold leaves were suspended in a glass jar from insulated wires. One of the outer plates, 'B,' was connected with one of the gold leaves, and the other outer plate with the other gold leaf. The outer plates B and C were adjusted at the distance of an inch and a quarter from the middle plate 'A;' and the gold leaves were fixed at two inches apart. 'A' was then slightly charged with electricity, and the plates 'B' and 'C' with their gold leaves thrown out of insulation *at the same time*, and then left insulated. In this state of things 'A' was charged positive inductrically, and B and C negative inducteously; the same dielectric (air) being in the two intervals, and the gold leaves hanging, of course, parallel to each other in a relatively unelectrified state."

Faraday affected the relation of the induction from A to B to the induction from A to C by the introduction of various dielectrics. I vary such relationship by altering the distance between B and C from A.

Faraday brought a plate of shellac between the middle plate A and one of the outer plates, B. Thereupon the gold

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* Communicated by the Physical Society.

leaves approached one another. The leaves again separated when the shellac was taken away. According to Faraday, the attraction is due to the increased induction where the shellac is, causing A to induce more negative electricity on B. The side therefore of B (and its gold leaf) furthest from A becomes positive. On the other outer plate C there is less negative electricity induced; and the outer side of this plate, together with the gold leaf connected with it, becomes therefore negative. Hence the gold leaves attract one another.

In my experiments I employed three zinc plates of 30 millim. diameter, and made use of, on some occasions, a glass jar (described above) having the two insulated gold leaves. Sometimes, however, I used two electroscopes, one of which was connected with the one and the other with the other outer plate. Under these conditions the kind of electricity which was to be found on the plate was easily determined.

I should like to be allowed to mention a few details which I think are of interest in electrostatic investigations. The insulation of the wire to which the gold leaves were hung was effected simply by passing the wire through a tube of ebonite having an external diameter of 5 millim. and 1 millim. opening.

In order to avoid binding-screws, the numerous edges of which are troublesome in electrostatic experiments, connexions were made by boring conical holes of diameters from $\frac{3}{4}$ tapering to $\frac{1}{2}$ millim., and simply thrusting the wires into the holes. No electricity must be allowed to settle on the glass of the electroscope. I therefore employed an ordinary cylindrical glass 15 centim. high and 10 centim. wide, but led round the glass, inside and outside, a continuous strip of tinfoil 3 centim. wide, parallel to the gold leaves and connected with the earth. By this means the electricity of the leaves induced electricity of the opposite kind in the tinfoil; and if this charge became too strong, the leaves touched the tinfoil and discharged themselves. The glass was provided with a wooden cover, through which passed the little ebonite tube enclosing the wire. Each zinc plate was fastened to a horizontal Z made of shellac. The three Z's overlapped one another, so that the plates could be brought to within 1·5 centim. of one another.

Division of Induction.

The three plates of the differential inductometer are to be looked on as two condensers which have one plate, namely the collecting-plate, in common. From this plate, as "inductric," the induction begins, and ends on the two side plates as "inducteous." In this arrangement the division of the induction corresponds to the division of the current in flowing electricity.

But before I proceed further with the physical considerations, which would appear hypothetical, I shall deduce the mathematical from the theory of potential.

If we look upon the differential inductometer as formed of two condensers, the total electricity, E, on the middle plate consists of two parts, e_1 and e_2;

$$\mathrm{E}=e_1+e_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

so that e_1 is the charge on one plate, and e_2 on the other. The value of each of these is found in the following manner. According to Green*, if e be the charge on a plane condenser whose plates have the surface S and the distance r, and if the potential-function on the one is V, while the other is connected with the earth, then

$$e=\frac{\mathrm{VS}}{4\pi r}.$$

In the derivation of this equation it is assumed that r is small in comparison with the diameter of the plates, an assumption to which I will return afterwards.

We will apply this formula to the two condensers of the differential inductometer. All three plates have the same surface S. The middle one has the potential V; the two outer ones are connected with the earth. Let the distance of the plate of which the charge is e_1 be r_1, that of the one with charge e_2 be r_2. Then

$$e_1=\frac{\mathrm{VS}}{4\pi r_1},$$

and

$$e_2=\frac{\mathrm{VS}}{4\pi r_2},$$

and, accordingly,

$$e_1 : e_2=\frac{1}{r_1} : \frac{1}{r_2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In other words, the charges are inversely proportional to the distances. The values of the two charges are derived immediately from equations (1) and (2). According to these,

$$e_1=\frac{r_2}{r_1+r_2}\mathrm{E} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$e_2=\frac{r_1}{r_1+r_2}\mathrm{E}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

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* Green, Essay, § 8; Thomson, Papers, § 53.

These formulæ, which are the same as those for the divided current, are only absolutely true for plates of an infinite size. They signify that the inductive capacities of the two condensers which form the differential inductometer are inversely proportional to the distances of the plates. If we use the term "inductive resistance" to denote the reciprocal of the inductive capacity in the same way as conductivity is the reciprocal of resistance, then, in the case of infinite plates, the inductive resistance is directly proportional to the distance. In such a case of infinite plates the equipotential surfaces are planes parallel to the plates, and the tubes of force are cylinders at right angles to them.

This electrostatic condition is converted into the corresponding electrodynamic one if a conducting liquid is placed between the plates. If, then, the plates are kept at the same electrical level by (say) a battery, then a divided current would pass from the middle plate through the liquid to the two side plates; so that the middle plate would be an anode, and the side plates kathodes; and the division would then take place in such a manner that the strength of the two branch currents would be inversely proportional to the respective resistances; or, since the latter are proportional to the distances, the currents would be inversely as the distances*. Just as we incur an error when we pass from the law that the divisions of the current are inversely proportional to the distances in the case of infinitely large plates, to the case where the plates are simply very large, so we incur a similar error in electrostatics when we apply the formula for infinitely great plates to the inductometer.

The case in electrodynamics which corresponds to the inductometer (which we regard as an apparatus for the division of induction) would be the branching of the current between three round plates. If we imagine the differential inductometer to be plunged into a conducting liquid and the potentials to be constantly maintained, then a current would pass from the middle plate as anode to the external plates as kathodes. Further, if in both cases we construct the system of the surfaces of equal potential, then, if their equation be

$$V = \text{constant},$$

V in both cases (that is, by the branching of the current and in the inductometer) would satisfy the same differential equation, and the same limiting conditions must be fulfilled. We should get in both cases the same system of surfaces of electrical level; and the perpendiculars to these surfaces would

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* See Currents between Plates, Wied. *Galv.* § 116.

have in the one case to be regarded as lines of force, and in the other as lines of flow. The greater the plates are, the more nearly flat are the surfaces of electric level, and the straighter are the lines of force.

Induction and conduction act, therefore, in the same lines. If, in order to fix our ideas, we take spermaceti as an intervening plate, as Faraday did, we find, with him, "spermaceti to be a dielectric, through which induction can take place" (Exp. Res. § 1322). But "spermaceti is also a conductor, though in so low a degree that we can trace the process of conduction, as it were, step by step through the mass. . . . . Here induction appears to be a necessary preliminary to conduction" (§ 1323). And although Faraday could not, as we have done, formularize the division of the induction in the same way as the branching of the current is formularized, still he had this agreement in his mind. That he could not formulate this idea was the reason why he was misunderstood; and this is the reason which has induced me to examine the point more carefully.

In one case of electrostatic induction through the air Faraday replaced a part of the air by sulphur, and found that the induction was now greater than when the air was there which the sulphur had replaced.

He now compares this case with that of the branching of a current in bad conductors, one of which he replaced by a good conducting wire. Then a stronger current would flow through the wire than flowed through the bad conductor which it had replaced. And as much less electricity would flow through the remaining bad conductors as more through the wire.

This is the meaning of Faraday's words, "Amongst insulating dielectrics some lead away the lines of force from others as the wire will do from worse conductors" (§ 1331).

I shall, in the following, consider some of the applications of this conception of the inductometer as a dividing of electrostatic induction.

Method for Determining Inductive Capacity.

The methods for determining electrical resistance which are based upon division of the current may be transferred to the measurement of inductive capacity. In Wheatstone's bridge, for example, first given by Christie, we determine an unknown resistance as a fourth proportion to three known resistances. For this purpose, the current between a point of higher and a point of lower potential is divided. Two points, one on each of the branches, are found having equal potential. They divide, therefore, the resistances of the two branches in like ratio. Then no current passes round a galvanometer introduced between these points. In the differential inductometer the beginning of the branching is the middle inductric plate, whose electric level is V, and the end of the branching is on the two inducteous plates, where the potential is zero. Between the inductric plate and each of the inducteous plates let us now introduce a metal intermediate plate of equal potential, so that there are four divisions whose inductive capacities are proportional to one another in pairs. If the inductive capacity of one of these divisions is altered by introducing another dielectric than air, then the capacity of the other division must also be altered, in order to restore the original proportion. The capacity of this second division is changed by altering the distance between its limiting plates until the original equality of the electrical potential of the two middle plates is again reached. This is recognized by finding that a quadrant electrometer connecting the two shows no deviation. Thus the measurement of inductive capacity is reduced to measuring a length. This method is essentially the same as that recommended by Maxwell and Sir W. Thomson to Mr. Gordon for his measurements of inductive capacity, published a year ago (Phil. Trans.).

Theory of the Electrophorus.

A further instance of the branching of induction is found in the electrophorus. Being about to deduce its theory and to discuss the number of electrically effective layers, I shall start from Faraday's differential inductometer. Such a theory of the electrophorus as takes Faraday's views into account has not yet been given. Faraday has not developed it; and the question as to how many layers are electrically effective is not treated by Maxwell, who views the electrophorus not as a symmetrical construction consisting of a plate of ebonite with two movable metallic coverings, but as an unsymmetrical arrangement consisting of a single ebonite plate covered at the back with metal, and having only one movable metal plate.

In this special manner we shall obtain a theory of the electrophorus, and get a fixed point from which to view the different theories based on electrical action at a distance and expressed in the language of the influence theory. We shall approach nearest to that one of these theories which has lately been supported by Herr v. Bezold (Pogg. *Ann.* cxliii.).

The electrophorus, as I view it, is symmetrical. It is a non-conducting plate with two movable coverings, and can thus be used as a Franklin's disk, a Leyden jar, or a condenser. My electrophorus consisted of a circular ebonite plate 1·5 millim. thick and 30 centim. diameter. The coverings were of zinc and were of the same diameter, one of them being provided with an ebonite handle. For other purposes I used three plates of the same shape, of thinner ebonite, together with other plates of zinc of similar dimensions.

The simplest supposition is that the ebonite plate, lying on the one uninsulated metal covering, being beaten by the fox's tail becomes negatively electric. Then induction starts from this negative layer of this ebonite plate as inductric layer (*inductor*). The inducteous layer (*inducendus*), where the induction ends, is the underlying uninsulated metal plate. After putting on and uninsulating the upper metal plate, however, the induction branches off again from the upper negative layer of the ebonite plate as inductric body (*inductor*), and extends to both the metal plates as inducteous bodies (*inducendus*). If there exists, as we have supposed, only one inductric layer in the electrophorus, namely the negative one on the upper surface of the ebonite plate, the differential inductometer could be viewed as a scheme of the electrophorus. The negative layer of the electrophorus would be represented by the negatively charged middle inductric plate of the differential inductometer; and the two metal coverings of the one apparatus would correspond to the two of the other. Further, this similarity of the two apparatus appears still more complete if we use the differential inductometer as electrophorus for producing electricity. We can uninsulate the one plate of the differential inductometer; and bringing the other near, we can draw out of it a negative spark; then withdrawing it, we can get a positive spark. But one difference now presents itself, and proves the insufficiency of our last scheme. As to the differential inductometer, it is perfectly indifferent whether we uninsulate the right outer plate and insulate and discharge the left one, or if we uninsulate this and change the distance of the right one. We always get positive discharge from that outer plate which we have removed from the negative middle plate. The same kind, namely positive discharge, after removal, must take place if there existed only one inductric layer. Then there is, as Faraday (§ 1255) has already pointed out, no difference as to the kind of induction through air and that through ebonite. Therefore, the ebonite plate being inverted, the upper metal covering (which is now, however, lying on the unrubbed ebonite surface) must give positive electricity when it is lifted, as the plate did before.

This, however, is not the case. If we turn the ebonite plate over, the upper metal covering, on being lifted off, gives negative electricity, while formerly it gave positive.

The differential inductometer, which gave positive in both cases, is therefore not a sufficiently accurate scheme to explain why we get,

(1) when the rubbed surface is above, a + spark,

(2) when the rubbed surface is below, a − spark, from the lifted metal.

The simplest completion of the differential inductometer having only one inductor plate, to make a scheme of the electrophorus, is effected by adding a second inductor plate (a positively charged one), which corresponds with the underside of the ebonite plate. In fact, after beating, the ebonite plate attaches itself so closely to the metal sole, that I could hold the sole vertical without the ebonite plate sliding down. At first the negative layer of the rubbed surface induced positive electricity in the metallic sole. The induction is followed by conduction and discharge; a part of the positive electricity goes over from the sole to the lower side of the ebonite plate. On the other side the negative electricity penetrates deeper from the upper surface into the ebonite; so that there are two layers on the ebonite plate—a negative one on the upper side, a positive one on the lower side. For the understanding of the question, it is here sufficient to regard these layers as plane, without discussing the question of the thickness.

With the help of these two layers we have to explain the two observations:—

(1) The + spark when the metal plate is lifted from the rubbed surface;

(2) The − spark when it is lifted from the unrubbed one.

For the proof of this theory we shall deduce further consequences from it; and we shall have to confirm these by experiment. The complete scheme of the electrophorus consists of four plates:—

(1) One negatively charged;

(2) One positively charged.

These two are as inductric bodies.

(3 and 4) Both metal coverings as inducteous bodies (*inducendus*).

To be brief, we will call the covering on the rubbed negative surface of the ebonite plate "cover;" that on the other, positive, unrubbed side, "sole;" so that we have in succession

cover,
negative layer,
positive layer,
sole.

Let us call b the distance of the sole from the positive surface, and d the distance of the cover from the negative layer.

If c is the thickness of the ebonite plate, a layer of air thinner in the ratio of the inductive capacity of the ebonite (2·2) would correspond to it, *i. e.* of the thickness

$$c' = \frac{c}{2{\cdot}2};$$

so that in our scheme the four metal plates would stand at the distances

$$b, \quad c', \quad d.$$

The rubbed side may contain the quantity of electricity

$$-\mathrm{E};$$

and from the sole the part

$$+\alpha\mathrm{E}$$

of the induced quantity may have passed on the unrubbed surface:

$$\begin{array}{c|c|c|c|c} & b & c' & d & . \\ \text{Sole.} & +\alpha\text{E.} & -\text{E.} & \text{Cover.} & \end{array}$$

Then, according to the equations (1), (3), and (4), the quantity $-\mathrm{E}$ induces a total amount of $+\mathrm{E}$, viz.:—

In the sole.	In the cover.
$\dfrac{+d}{b+c'+d}\mathrm{E}.$	$\dfrac{+b+c'}{b+c'+d}\mathrm{E}.$

In the same manner the quantity of the lower side $+\alpha\mathrm{E}$ induces a total of $-\alpha\mathrm{E}$, viz.:—

In the sole.	In the cover.
$\dfrac{-\alpha c'-\alpha d}{b+c'+d}\mathrm{E}.$	$\dfrac{-\alpha b}{b+c'+d}\mathrm{E}.$

So that all together is induced:—

In the sole.	In the cover.
$\dfrac{+d-\alpha c'-\alpha d}{b+c'+d}$	$\dfrac{+b+c'-\alpha b}{b+c'+d}\mathrm{E}.$

The numerator of the electric quantity in the cover

$$+b+c'-\alpha b$$

is always positive. That is, there is always induced in the metallic covering on the side of the rubbed surface positive electricity; and we get, on removing the plate, a positive spark.

On the other hand, we recognize that the numerator of the electric quantity in the sole

$$+d-\alpha c'-\alpha d = d(1-\alpha)-\alpha c',$$

$\alpha c'$ being constant, changes its sign when d increases: viz. d (that is, the distance of the cover) being small, negative electricity is induced in the sole, and we get, on lifting the sole, a negative discharge. This distance d, however, is small, almost nothing, if we invert the ebonite plate and lay it with its rubbed surface close to the metal plate. Then we get from the upper metal plate (the sole), on lifting it, a negative discharge. Thus the two phenomena which we intended to explain, (1) positive discharge in the original position, (2) negative discharge in the inverted position, of the ebonite plate, are deduced from our theory.

The electrophorus contains, therefore, two inductric layers—one negative, on the upper side, one positive, on the lower side of the ebonite plate.

Experiments with the Electrophorus.

If the theory developed by us is true, the consequence which we are about to draw now must be confirmed by experiment. According to the last formula for the numerator of the electric quantity of the sole, positive electricity must be induced in the sole when the distance d of the rubbed surface from the cover is increased, so that we get

$$d\,.\,(1-\alpha) > \alpha c'.$$

This happens indeed, as we see, if we lay the inverted ebonite plate, not immediately on the cover, but place it insulated some height above the cover, as Herr von Bezold has done already. Now the other plate, the sole, being laid on the unrubbed surface, uninsulated and lifted, gives a positive spark. Whilst the sign of the electricity of the cover (the plate on the side of the rubbed surface) is always positive, the sign of the electricity of the sole (the plate on the side of the unrubbed surface) changes. It is positive when the distance of the other metal plate (the cover) from the rubbed layer is small; on the contrary, it is negative when this distance is great.

In other words, when the cover is on the side of the stronger negative inductric layer, the induction of the latter always prevails; in it positive electricity is induced.

In the sole, however, the nearer but weaker positive layer induces negative electricity. To this there is to be added the positive electricity induced by the stronger but more distant negative inductric layer. The more distant the cover is, the more positive electricity is induced in the sole; so that, the distance of the cover being large, more positive than negative electricity can be induced.

On the two sides of the ebonite plate the sign of the discharge is differently affected.

1. The ebonite plate lying close to the metal plate, the other, lifted, metal plate gives:—

(*a*) The rubbed surface being above, a positive discharge;

(*b*) The unrubbed surface being above, a negative discharge.

2. The ebonite plate lying some height above the metal plate, we get, on the contrary, quite indifferently, whether (*a*) the rubbed surface is above, or (*b*) the unrubbed surface, in both cases positive discharges.

Thus we have the means of recognizing not only—

(1) on which side an ebonite plate is charged positively,

(2) on which side it is charged negatively, but also

(3) if it had been charged originally negatively on the one side or

(4) positively on the other side.

For this purpose we place the ebonite plate to be tested close to an uninsulated metal plate. Then we put a second metal plate on the ebonite disk, uninsulate it, and try the electricity induced in it; the opposite one exists in the upper surface of the ebonite plate.

The ebonite plate is now inverted, put again close to the uninsulated metal plate, and the kind of electricity of the other surface of the ebonite plate is determined in the same manner. Thereby we learn which kind of electricity exists in each surface.

Now the ebonite plate is tested at some height over the metal plate, at first in the original, then in the inverted position; it is, however, to be lifted so high that in both cases the same kind of electricity is induced. Then the plate contains a surplus of the opposite kind, and has been charged with it originally on that side on which according to our trial it exists.

Adding the two quantities of electricity which we have found induced in the sole and in the cover, we get, in accordance with our supposition,

$$-E(1-\alpha).$$

The greater the quantity which is induced in the cover, the less is induced in the sole, and *vice versâ*. If we join the cover with one electroscope and the sole with another, both electroscopes show a divergence on moving one of the plates, but in opposite senses.

Through a conducting wire from the cover to the sole there flows a current on moving one of the plates; so that the quantity which flows to the sole has left the cover; whilst in the electrophorus itself between sole and cover (*i. e.* in the remaining part of the circuit) changes of induction take place. In the ordinary experiments with the electrophorus this conducting wire is represented by the earth, the base being uninsulated. Here we can speak of a circuit only with the same right as in the case of a telegraphic battery with earth-conduction. If the conduction is broken by insulating the sole after having it uninsulated, the electrophorus is without effect. As to the changes of induction in the interior of the electrophorus (which we may imagine as a polarization of the dielectric), I will further adduce the following experiments:—

If the ebonite plate, put on the uninsulated base, is strongly excited, it attaches itself to the base. If now the cover is put on and both metal plates are uninsulated at the same time, it attaches itself to the cover, so that we can lift the plate by means of the cover. A current of positive electricity has flowed at the moment of uninsulating from the sole through the outer conducting wire to the cover. Before the current has flowed, the induction (polarization) went wholly through the ebonite plate; after the current the inductive (polarizing) effect of the excited layer extends, for the greater part, through the thinner interval of air to the cover, and only for a smaller part of it through the thicker ebonite plate to the sole.

Finally, I may mention an experiment in which branching of induction, as well as branching of conduction, may easily be observed. I beat the ebonite plate lying on the uninsulated base, and put on the excited surface of the first another equal, but not excited, ebonite plate, and then on this the metal cover. This cover, being uninsulated and lifted, gave positive sparks, just as if there had been in place of the upper ebonite plate a corresponding layer of air. Immediately after this I tried the upper ebonite plate, endeavouring to use it alone as the disk of an electrophorus. But this trial did not succeed; the plate was ineffective.

Now I repeated the experiment, laid the same plate again on the rubbed surface of the first ebonite plate, put on it also again the cover, but left the apparatus unaltered during twelve hours. In this case there is ebonite on the two sides of the inductric layer of the ebonite plate; induction extends from this layer through ebonite to the sole and to the cover. In the same manner the conduction—the penetration or absorption of electricity—must take place to both sides; and I expected that the negative electricity would penetrate not only into the lower ebonite plate, but would go over to the upper plate also. This is indeed what took place. After twelve hours the upper plate could be used as the disk of an electrophorus. Both plates gave now strong and, as it seemed, equal effects. As was to be supposed, the upper plate was negatively charged on its lower surface, with which it lay on the inductric layer, and had therefore to be inverted in order to give similar discharges to those of the first plate.

If we fix our attention specially on the upper ebonite plate, we find:—

(1) That the effect of *induction* through it began almost instantaneously (for immediately after having laid this upper plate on the excited lower one I could put on, lift, and discharge the cover); and that this effect of induction had disappeared also almost instantaneously when I tried to use the plate alone for the electrophorus.

But, further, we have seen (2) that slowly another *inherent* change took place: a penetration of electricity occurred which made the plate more duringly effective as an electrophorus-disk.

XIV. *Remarks on Thermometry.*
By Edmund J. Mills, *D.Sc., F.R.S.*[*]

THE July number of this Magazine contains an expression of the desire of Professors Thorpe and Rücker for further information on certain points contained in a memoir by myself, and recently published by the Royal Society of Edinburgh. In view of the increasing interest now taken in thermometry, I am willing to comply with their request; but it is only just to state that their object would have been gained at a much earlier date by a private communication, instead of the much longer and indirect method of addressing me in the pages of this Journal.

1. *The Exposure Correction.*—The results of very many experiments with four thermometers have led me to propose a new formula for this correction, viz.

$$y=(\alpha+\beta\mathrm{N})(\mathrm{T}-t)\mathrm{N},$$

instead of the ordinary one

$$y=\alpha(\mathrm{T}-t)\mathrm{N},$$

where $\alpha=\cdot0001545$; for if we regard α as an unknown quantity, and determine it by actual measurements, we shall find that it is a linear function of the exposure. Both α and β are found to depend in part on the individual thermometer. If we put $x=\alpha+\beta\mathrm{N}$, it is easy to calculate when x (i. e. the total correction factor) agrees first for two or more ther-

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[*] Communicated by the Author

mometers. I have given a table of such agreement-points (N) in the case of four thermometers; and if the stated values of N are substituted in the four equations (also given), x has in all cases the same value. This allegation I must distinctly repeat; but I understand Professors Thorpe and Rücker to say that it is unfounded.

On page 3 of the authors' commentary, a calculation is given from which it is inferred that "the exposure corrections below 100° C. are, in the case of three out of four similar thermometers, practically identical." The calculation, however, takes 100° C., and not any lower temperature; and it happens that the correction-factors are then rather close together, while at lower temperatures (to which no reference is made) they are much further apart. Thus the "favourable suppositions" are only favourable at 100° C. to my critics, with whom, at that point, I entirely agree. At lower temperatures their inference is clearly wrong.

[It may be of interest for me to add that, with regard to observational power, I found myself, when in great practice with scale-reading, capable of estimating very fairly $\frac{1}{20}$ millimetre. Verifications of this result were not unfrequently made by kathetometric comparison; but it involves a higher grade of accuracy than the Leeds professors seem disposed to allow.]

My critics proceed thus:—" . . . . . it is impossible for the correction for a thermometer with an exposed column 166 divisions long to be equal to that of another when no part of the column is exposed." But my memoir contains no such statement. They have here mistaken the "correction-factor" for the "correction," which is a very different thing. A correction-factor may be attributed, with perfect possibility, even to an unexposed thermometer.

The authors next point out a real error, which I readily admit. Among some thousands of calculations of which my paper contains the results, it appears that a numerical blunder has been committed. Instead of multiplying by 4, in a particular conversion, I have actually divided by 4. The equation containing this error has never once been used by myself, and would instantly excite the suspicion of the most careless person who employed it. The mistake is thus of no consequence whatever; but more than a page of the Philosophical Magazine is gravely devoted to its discussion.

2. *Zero Movements.*—The term "ascent" of a zero has been used by me to signify its movement. Sign has been always specified, or at least indicated. This is a common, and indeed a necessary practice in physical writings. If it has led to

confusion in the minds of my critics, I am not prepared to suggest a remedy, and must leave their very lengthy disquisition upon this point entirely unassailed. I should add here, that I have nowhere alleged that the motion of a thermometer's zero has any particular limit.

The remainder of the remarks of Professors Thorpe and Rücker under this heading comprise a short dissertation (page 9) on the prudence that should be observed in applying mathematical expressions to represent the results of experiment. This passage appears perfectly clear, though perhaps not wholly novel; but I have so far failed to follow the rest of this section, that I am unable to discuss it. Perhaps it will be satisfactory if, mathematical formulæ apart, I make a statement of matters of fact within my own knowledge.

When a vacuous mercurial thermometer is heated to successively higher temperatures, its zero continues to fall until some particular temperature (say 150°) is reached. After this the zero commences to rise (I must not say "ascend"), as the temperature increases to another particular stage (say 340°). Between these two phenomena there must be a "turning-point." As the temperature continues to increase, the zero again falls; this, of course, involves another "turning-point." The "rising" in the second stage often amounts to as much as 3° or 4°; this enormous effect I can attribute to no other cause than atmospheric pressure.

3. The remaining criticisms do not, as I conceive, involve the imparting of information. It will therefore be sufficient for me to reply that to the statements made in my memoir I still strictly adhere. Some experiments of their own, referred to towards the end of their paper, are cited by its authors *à propos* of a "rising zero," which I have not defined as they suppose.

Professors Thorpe and Rücker indicate that they may have occasion subsequently to further discuss my "Researches." Should they thus honour me, I trust it will be to really essential and important points that they will direct their attention, and preferably by way of experiment. An obvious misprint, a slip in arithmetic, a question of mere nomenclature, and the like are trivialities unworthy, in my opinion, of grave and lengthy publication.

Glasgow, July 1881.

XV. *On a Wave-apparatus for Lecture-Purposes to illustrate* Fresnel's *Conception of Polarized Light. By* C. J. WOODWARD, *B.Sc.*[*]

[Plate II.]

IN the ordinary apparatus for illustrating a plane wave, a series of cranks or eccentrics are attached to one axle, the successive cranks or eccentrics being turned through small, but equal angles. Attached to the cranks or eccentrics are rods, each of which is terminated by a ball, to represent one of the particles of the wave. On turning the axle each ball rises and falls in succession, producing a plane wave. This apparatus is known as that of Powell.

The arrangement I am about to describe consists of two such apparatus, with the rods connected together at right angles to each other. The axles of the apparatus are linked by a rod so that they turn together; and, finally, an adjustment allows the cranks to be placed relatively to each other in any desired position.

The apparatus, divested of supports and accessories, is shown in the figure, drawn in isometric perspective. The cranks 6, 5, 4, 3, &c. turn with the axle X. The cranks are placed successively at 30° angular distance, corresponding in position to the figures on a clock-dial; and as there are twelve cranks, one complete wave is represented by a single turn of the axle.

On the axle X′ is arranged a precisely similar set of cranks. Around each crank is put a metal strap connected with a light wooden rod. These rods, *a*, *b*, *c*, &c., *a′*, *b′*, *c′*, &c., are now connected in pairs at their free ends each by a pin; so that each pair now resembles a pair of compasses with the legs at right angles to each other. Around each pin is loosely wrapped a wire having a bullet-cast at one end; and to the other end of the wire, which, from the action of the bullet, projects vertically above the rods, is attached a pearl bead. When each pair of rods has been thus treated, we have a row of beads representing the ætherial molecules composing one wave.

To the axle X is fastened a metal disk, D, to which is attached near the edge a pin, *p*, surrounded by the loose handle, H. On the axle X′ is a similar disk, but in which eight holes are drilled at 45° from each other. The holes are all tapped, that the pin *p′*, surrounded by the handle H′, may screw into any one of them as desired. A rod, R, connects the two pins *p* and *p′*, so that both axles can be turned together.

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[*] Communicated by the Physical Society.

To describe the action of the apparatus, it should be premised that the starting-point of each crank is when the crank and rod are in the same straight line. Thus, regarding for the moment the first left-hand crank only, it will be at the starting-point, or zero, when the crank 6 has moved on 45° to the left. Similarly with the first right-hand crank, it will be at zero if moved back through 45° to the right; so that, in the position shown in the figure, the left-hand crank has moved through seven eighths of a revolution, while the right-hand crank has moved through one eighth; or the difference of their phases is six eighths, or three quarters; and on turning the cranks, a wave corresponding to circularly-polarized light is produced, each bead describing a circle in succession. Bring now the cranks into the position of the figure, unscrew the pin p', and, keeping the left-hand crank steady, move the right-hand crank to the vertical position upward. Now the left-hand crank, as before, has made seven eighths of a revolution, but the right-hand crank five eighths, *i. e.* a difference of phase of one quarter. On inserting the pin p' and now turning the cranks, the beads will describe the same wave as before. Now keep the left-hand crank in its first position (the position in the figure), remove the pin p', and turn the right-hand crank until it is at 45° from the vertical below and to the left. The left-hand crank, as before, has made seven eighths of a revolution, the right-hand one two eighths, or there is a difference of phase of five eighths; and on now inserting the pin and turning the cranks, the wave produced corresponds to an elliptically-polarized ray. Of course, if the difference of phase be made one eighth or any odd number of eighths, the same kind of wave is formed. Again remove the pin, keep the left-hand crank in its first position, and turn the right-hand crank to 45° above the horizontal and to the right. Insert the pin; and on turning the cranks a plane *vertical* wave will be produced, the cranks being in similar positions, each starting at seven eighths of its revolution. Finally, adjust the right-hand crank till the difference of phase is half a revolution, when a plane *horizontal* wave will be produced on turning the cranks.

I take the opportunity of acknowledging the assistance of my pupil, Mr. William Hall, who made several parts of the apparatus in a simple and ingenious manner.

XVI. *Note on Thermal Conductivity, and on the Effects of Temperature-Changes of Specific Heat and Conductivity on the Propagation of Plane Heat-Waves.* By Professor TAIT [*].

IN the great majority, at least, of investigations (experimental or mathematical) connected with conduction of heat, it has been assumed that the known changes of specific heat of metals do not require to be taken into account. Thus Ångström says, even in his paper on the *Change* of Conductivity with temperature (Pogg. *Ann.* cxviii. 1863):—"Da indess diese Veränderungen, soweit man sie kennt, wenigstens innerhalb der bei den Beobachtungen vorkommenden Temperaturgränzen, nicht bedeutend sind, ... so müssen dieselben den Werth des Wärmecoefficienten nur unbedeutend afficiren können." In my paper on "Thermal and Electric Conductivity" (Trans. R. S. E., 1878), I said that "the change of specific heat with temperature would *increase* the values of k at higher temperatures, and thus reduce the change in conductivity in iron, and increase the small changes indicated for the other substances." But I had not at hand the means of applying these corrections. Recent discussions as to the comparative merits of different experimental methods have led me to investigate the amount of this effect, by the aid of the best data I could procure. A comparison of these seems to leave no doubt that the specific heat of iron *increases* by somewhere about $\frac{1}{700}$ of its amount for each degree of rise of temperature, at least from 0° to 300° C., between which limits the investigations of conductivity have hitherto been carried on.

Besides this result, which I have gathered from various scientific journals, I may adduce from my laboratory book for 1868 the following determinations, which were made with great care by the late Mr. J. P. Nichol, by means of the method of mixtures. The nature of the process employed is such that the results *must* all err in defect, and the more so the higher the temperature. The iron was heated sometimes in oil, sometimes in paraffin.

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[*] From the Proceedings of the Royal Society of Edinburgh, 17th January and 7th February, 1881. Communicated by the Author.

Specific Heat of Iron.

		Mean.
15° to 100° C.	0·1154	0·1152
	0·1127	
	0·1158	
	0·1168	
15° to 150° C.	0·1193	0·1189
	0·1189	
	0·1186	
15° to 200° C.	0·1208	0·1213
	0·1214	
	0·1218	
15° to 250° C.	0·1234	0·1237
	0·1240	
15° to 300° C.	0·1274	0·1275
	0·1276	

From the first two of these means we find that the specific heat at 15° is 0·109 nearly, and that it increases by $\frac{1}{730}$ for each degree.

Now Forbes's experiments on iron indicated that the quantity $\frac{k}{c}$, the ratio of the conductivity to the specific heat, *diminishes* by about $\frac{1}{550}$ part for each degree from 0° C. to 200° C. Hence it is clear that, in this case at least, the alteration of specific heat cannot be neglected in estimating that of conductivity. For it follows from the numbers just given, that the diminution per 1° in the conductivity of iron is really only about $\frac{1}{2500}$ of the whole amount. My own experiments with Forbes's bars gave an average change of $\frac{k}{c}$ *less* than that due to the increase of c alone, thus indicating an increase of conductivity with rise of temperature. Ångström's result, on the other hand, is considerably greater than that of Forbes; but the range of temperatures he employed was not above forty degrees. For reasons pointed out in my paper above referred to, I consider Forbes's estimate of the value of $\frac{k}{c}$, from 0° to 150° C., to be probably very near the truth. In other metals the change of specific heat is usually less than in iron. But so is also that of $\frac{k}{c}$. It would thus appear that we cannot yet state positively that there is any metal whose conductivity becomes less as its temperature rises; and thus the long-sought analogy between thermal and electric conductivity is not likely to be realized.

In the method devised and carried out by Forbes, the change of specific heat must be attended to during the calculations. Thus we cannot, without going over again the whole numerical work connected with what he called the *Statical Curve of Cooling*, estimate accurately what will be the effect of this element upon the values of the conductivity. But we can easily show that its influence upon Ångström's results is to be calculated, at least approximately, by the simple process above.

To avoid the error introduced by supposing rate of surface-loss to be proportional to v, we take (instead of a bar) a plane slab heated and cooled periodically over one surface.

The equation for the consequent distribution of temperature is

$$c\frac{dv}{dt}=\frac{d}{dx}\left(k\frac{dv}{dx}\right).$$

If we assume

$$c=c_0(1+\alpha v),$$
$$k=k_0(1-\beta v),$$

where α and β are small positive constants, and put

$$\kappa=\frac{k_0}{c_0},$$
$$v=u+\omega,$$

where ω depends upon first powers of α and β only, higher powers being neglected, the equation splits into two as follows:—

$$\frac{du}{dt}=\kappa\frac{d^2u}{dx^2}, \quad \ldots\ldots\ldots \quad (1)$$

$$\frac{d\omega}{dt}-\kappa\frac{d^2\omega}{dx^2}=-\kappa(\alpha+\beta)u\frac{d^2u}{dx^2}-\kappa\beta\left(\frac{du}{dx}\right)^2. \quad . \quad (2)$$

For our present purpose it is sufficient to take

$$u=-\mathrm{B}x+\mathrm{C}\epsilon^{-mx}\cos 2\kappa m^2t-mx,$$

which satisfies (1), and shows the ultimate effect of a persistent simple harmonic application of heat to one side of the slab, whose temperature is taken as our temporary zero; the other side being kept at the temperature $-\mathrm{B}s$, where s is the thickness of the slab. Here s must be supposed so large that $\mathrm{C}\epsilon^{-ms}$ is insensible; else the value of u would be so complicated that (2) would become unmanageable.

Substituting the above value of u in (2) and integrating, we obtain the value of ω. It consists of three parts.

We have, first, terms containing x only:—

$$\beta B^2 \frac{x^2}{2} + \frac{\beta}{4} C^2 \epsilon^{-2mx}.$$

These terms show how the mean temperature is altered throughout.

Next, we have the single term

$$\frac{\alpha + 2\beta}{4} C^2 \epsilon^{-2mx} \cos (4\kappa m^2 t - 2mx).$$

This is a small wave of half period, which we need not further consider.

Finally, we have, as the modification of the original wave,

$$C\epsilon^{-mx} \left\{ \left(\frac{\alpha - 3\beta}{4} Bx + \frac{m(\alpha + \beta)}{4} Bx^2 \right) \cos (2\kappa m^2 t - mx) \right.$$
$$\left. - \frac{m(\alpha + \beta)}{4} Bx^2 \sin (2\kappa m^2 t - mx) \right\}.$$

These terms, when combined with the harmonic part of the assumed value of u, may be put in the form

$$C\epsilon^{-m_1 x} \cos (2\kappa m^2 t - m_2 x);$$

where

$$m_1 = m \left(1 - \frac{\alpha - 3\beta}{4m} B - \frac{\alpha + \beta}{4} Bx \right),$$

$$m_2 = m \left(1 - \frac{\alpha + \beta}{4} Bx \right).$$

We thus see the effects of the introduction of the quantities α and β upon the amplitude and phase of the wave; and it is evident that they are of the greater consequence the greater is the difference of mean temperature at the sides of the slab.

Hence the only legitimate mode of applying Ångström's method is to keep the mean temperature the same throughout the slab. This can easily be effected.

It is obvious, moreover, from the values of m_1 and m_2 above, that Ångström's method gives the value of $\frac{k}{c}$ for the mean of the mean temperatures indicated by the two thermometers. Only there is always the extraneous factor

$$1 + \frac{\alpha - 3\beta}{4m} B,$$

which is usually very nearly unity.

I have worked out by the above method the case of two harmonic waves (in the value of u), one of half the period of the other. New terms are thus introduced into m_1 and m_2. They are such as to seriously affect the values of these quantities when x is small; but they rapidly diminish by increase of x.

If the new term in u be

$$D\epsilon^{-mx\sqrt{2}}\cos(4\kappa m^2t-mx\sqrt{2}+E),$$

the additional terms in m_1 are

$$-\frac{\alpha+\beta}{4m}D\epsilon^{-mx\sqrt{2}}\sin X-\frac{\beta}{2\sqrt{2}-1}\frac{D}{m}\epsilon^{-mx\sqrt{2}}\cos X.$$

Those in m_2 are formed from these by making the first term positive, and interchanging the sine and cosine of

$$X=mx(\sqrt{2}+1)-E.$$

It appears from this investigation that Ångström's method, when applied with proper precautions, is theoretically capable of giving very good results. But it is probable that, in practice, the thermometers will have to be supplanted by thermoelectric junctions and a good dead-beat galvanometer. The best thermometers, when employed for rapidly varying temperatures, work by sudden starts.

XVII. *Intelligence and Miscellaneous Articles.*

ON THE PRINCIPLE OF THE CONSERVATION OF ELECTRICITY.

BY M. G. LIPPMANN*.

[Second Paper, continued from vol. xi. p. 475.]

THE principle of the Conservation of Electricity is expressed (see former article, Phil. Mag. vol. xi. p. 474) by the condition of integrability

$$\frac{\partial P}{\partial y}=\frac{\partial Q}{\partial x}. \qquad (\alpha)$$

Put thus in the form of an equation, the principle is capable of new applications. It provides the means not only of analyzing certain phenomena, but also of deducing from them the existence and the law of new phenomena which experience has not yet made known to us. I propose to give some examples of its applications.

As a first example, I take the phenomenon discovered by M. Boltzmann in 1875. This physicist proposed to measure the quantity termed the *dielectric power of gases*. For this purpose he attaches to a firm stand, under the receiver of an air-pump, two parallel metal plates, A, T, which form the two armatures of an air-condenser. A is insulated; T is connected to earth. The condenser is charged to a certain degree by placing plate A in communication for an instant with the pole of a battery whose other pole

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* Translated from the *Comptes Rendus*, May 16, 1881.

is joined to earth: then A is insulated. If the pressure p of the gas in the receiver is now increased, it can be demonstrated that the quantity of free electricity on A is diminished. The insulation remains perfect, and the plate A has not moved; but the capacity of the condenser has become greater by the introduction of the gas into the previously exhausted receiver. The effect is virtually the same as if the distance between the plates had become D times less. The gas possesses, then, the property of making the capacity of the receiver D times greater by its presence, D being what is called the dielectric power of the gas under pressure p. M. Boltzmann has proved that D varies for different gases, and for any one gas varies proportionally with the pressure p.

Such is the phenomenon given by experience, and to which we will apply equation (α). For this end let us take for independent variables the potential x of the plate A, and the pressure p of the gas. Let us write

$$dm = c\,dx + h\,dp,$$

where dm is the quantity of electricity received by the plate A when x increases by dx and p by dp, and where c is the capacity of the condenser when the gas is maintained at pressure p, and h a coefficient which, according to Boltzmann's experiments, is positive. Equation (α) becomes here

$$\frac{\partial c}{\partial p} = \frac{\partial h}{\partial x}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (\alpha')$$

This equation expresses the principle of the Conservation of Electricity.

In order to complete the study of Boltzmann's phenomenon, we must apply to equation (α) that equation which expresses the principle of the conservation of energy. When the piston of the air-pump in M. Boltzmann's experiment is displaced through an infinitely small distance, the volume v of the air contained in the apparatus varies by an amount dv. If we lay down the following equation,

$$dE = p\,dv - x\,dm,$$

dE will represent the differential of the energy; and it can be demonstrated easily that the principle of the Conservation of Energy will be expressed by the condition that dE be an exact differential. To write down this condition, we must express dv as a function of x and of p. Let us put

$$dv = a\,dx + b\,dp,$$

a being a coefficient about which we will make no hypothesis, b being a function of p and perhaps of x also. We have consequently the relation

$$\frac{\partial a}{\partial p} = \frac{\partial b}{\partial x}. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

Substituting for dv its value in the expression of dE, we have

$$dE = (ap - cx)dx + (bp - hx)dp.$$

In order that dE may be an exact differential, we must have

$$\frac{\partial(ap-cx)}{dp}=\frac{\partial(bp-hx)}{dx},$$

or, developing and comparing with equation (1),

$$a=x\left(\frac{\partial c}{\partial p}-\frac{\partial h}{\partial x}\right)-h. \quad\dots\dots\dots\dots\dots\dots \quad (\beta)$$

This equation (β) expresses the principle of the Conservation of Energy. Comparing it with equation (α), equation (β) reduces to

$$a=-h. \quad\dots\dots\dots\dots\dots\dots\dots \quad (\beta')$$

Such is, then, the result of analysis. The two principles of the Conservation of Electricity and of the Conservation of Energy are expressed by the system of equations (α') and (β'). According to experiment, h is a positive quantity; and a is, by equation (β'), always different from zero, and negative. Now a is the partial derivative of v with respect to the potential x. Hence the following phenomenon—that if, while the pressure is constant, the plate A is more and more electrified, the circumambient gas *will contract* under the influence of this electrification. The application of equation (α), then, makes us foresee a new electric property of gases. I say equation (α); for equation (β) alone does not permit us to conclude that a is different from zero. The electric contraction of gases has recently been perceived by an able German experimenter, M. Quincke, in an experiment upon carbonic acid.

M. Boltzmann has verified by experiment that

$$D=1+\gamma p=n^2,$$

γ being a specific constant of the gas, and n its refractive index. Introducing this result into our preceding equations, it follows easily that

$$\Delta v=-\tfrac{1}{2}\gamma c_0 x^2,$$

Δv being the electric contraction of the gas, and c_0 the capacity of the condenser *in vacuo*. And since we have

$$1+\gamma p=n^2,$$

it follows that

$$\gamma=\frac{n^2-1}{p},$$

or, in other terms, that what we call the "*coefficient of electric contraction*" *of a gas* is equal to its refracting-power for light.

In the accompanying memoir (presented to the Académie des Sciences) I have applied the same kind of analysis to various electric phenomena—the expansion of the glass of a Leyden jar during charge, the electrification of hemihedral crystals by compression, pyroelectricity of crystals. I find, amongst other results, the existence and the magnitude of the following phenomena, which have not yet been verified by experiment:—(1) The dielectric power of glass augments under the action of a mechanical traction; (2) crystals possessing non-superposable hemihedry of form change their form when submitted to electric forces, and this deformation may sometimes be in an opposite sense to that which electric attraction would itself produce; (3) a pyroelectric crystal is warmed or cooled by the approach of an electrified body, according to whether the body be positively or negatively electrified.

The phenomena thus foreseen are the inverse of the phenomena from which they are respectively deduced. To establish their existence, equation (α) is in each case necessary. Finally, we may remark that the sense of the inverse phenomenon may always be defined by the following rule, which is an extension of that given by Lenz for induction:—*The inverse phenomenon is always of such a sense that it tends to oppose the production of the primitive phenomenon.*

The process of calculation which I have employed in this memoir, and which consists in translating a physical law by a condition of integrability, was introduced into science by Sir W. Thomson and by M. Kirchhoff. An examination of the work of those eminent physicists will, I think, be convincing that the principle of the Conservation of Electricity is for Electricity that which the principle of Carnot is for Heat.

MR. R. SHIDA ON DR. C. R. ALDER WRIGHT'S NOTE PUBLISHED IN THE PHILOSOPHICAL MAGAZINE FOR JULY 1881*.

In answer to Dr. C. R. A. Wright's note (page 76, Phil. Mag. for July 1881) on my paper and my recent note, I have to state, (1) that by what I said in my note was meant that the calculation confirmed the truth, in the case of my experiment described in my paper, of the very fundamental law of Ohm, namely

$$C=\frac{E}{b+r}=\frac{v-v'}{r}, \text{ or } E=(v-v')\frac{b+r}{r}$$

(an equation identical with that shown in his note after elaborate steps of reasoning), where E denotes the E.M.F. of the cell in the circuit consisting of (b) the internal resistance of the cell and (r) the resistance external to it, and $v-v'$ denotes the difference of potential between the poles of the cell when the current C is flowing through the circuit; and (2) that what I measured both electrostatically and electromagnetically in my experiment was *one and the same E.M.F.*, the E of the Thomson gravity Daniell, but not ($v-v'$) the difference of potential between the poles, which I do not call the E.M.F. of the cell as he (Dr. Wright) does.

[The "two-per-cent." error alleged by Dr. Wright is a creature of his own imagination, due seemingly to misunderstanding of Ohm's Law!—W. T.]

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* Communicated by Sir William Thomson.

NOTE ON THE PHOTOGRAPHIC SPECTRUM OF COMET *b* 1881.

BY WILLIAM HUGGINS, D.C.L., LL.D., F.R.S.

On the evening of June 24 I directed the reflector furnished with the spectroscopic and photographic arrangements described in my paper "On the Photographic Spectra of Stars"* to the head of the comet, so that the nucleus should be upon one half of the slit. After one hour's exposure the open half of the slit was closed, the shutter withdrawn from the other half, and the instrument then directed to Arcturus for fifteen minutes.

After development the plate presented a very distinct spectrum of the comet, together with the spectrum of the star, which I have already described in the paper referred to above.

The spectrum of the comet consists of a pair of bright lines in the ultra-violet region, and a continuous spectrum which can be traced from about F to some distance beyond H.

The bright lines, a little distance beyond H, with an approximate wave-length from 3870 to 3890, appear to belong to the spectrum of carbon (in some form, possibly in combination with hydrogen) which I observed in the spectra of the telescopic comets of 1866 and 1868.

In the continuous spectrum shown in the photograph the dark lines of Fraunhofer can be seen.

This photographic evidence supports the results of my previous observations in the visible spectra of some telescopic comets. Part of the light from comets is reflected solar light; and another part is light of their own. The spectrum of this light shows the presence in the comet of carbon, possibly in combination with hydrogen.

On the next night, June 25, a second photograph was obtained with an exposure of an hour and a half. This photograph, notwithstanding the longer exposure, is fainter, but shows distinctly the two bright lines and the continuous spectrum, which is too faint to allow the Fraunhofer lines to be seen.

Postscript, July 9, 1881.—I have since measured the photographs of the comet's spectrum; and I find for the two strong bright lines the wave-lengths 3883 and 3870. The less-refrangible line is much stronger; and a faint luminosity can be traced from it to a little beyond the second line, 3870. There can be no doubt, therefore, that these lines represent the brightest end of the ultra-violet group which appears under certain circumstances in the spectra of the compounds of carbon. Professors Liveing and Dewar have found for the strong line at the beginning of this group the wavelength 3882·7, and for the second line 3870·5.

I am also able to see upon the continuous solar spectrum a distinct impression of the group of lines between G and *h*, which is usually associated with the group described above. My measures for the less-refrangible end of this group give a wave-length of 4230, which agrees as well as can be expected with Professors Liveing and Dewar's measure 4220.

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* Phil. Trans. 1880, p. 669.

In their paper "On the Spectra of the Compounds of Carbon" (Proc. Roy. Soc. vol. xxx. p. 494), Professors Liveing and Dewar show that these two groups indicate the presence of cyanogen, and are not to be seen in the absence of nitrogen. If this be the case, the photograph gives undoubted evidence of the presence of nitrogen in the comet, in addition to the carbon and hydrogen shown to be there by the bright groups in the visible part of the spectrum. On this hypothesis we must further suppose a high temperature in the comet, unless the cyanogen is present ready formed.

I should state that Mr. Lockyer regards the two groups in the photograph, and the groups in the visible spectrum, as due to the vapour of carbon at different heat-levels (Proc. Roy. Soc. vol. xxx. p. 461).

It is of importance to mention the strong intensity in the photograph of the lines 3883 and 3870, as compared with the continuous spectrum and the faint bright group beginning at 4230. At this part of the spectrum, therefore, the light emitted by the cometary matter exceeded by many times the reflected solar light. I reserve for the present the theoretical suggestions which arise from the new information which the photographs have given us.—*Proceedings of the Royal Society*, No. 213, 1881. *Communicated by the Author.*

THE EXISTENCE OF THE LUMINIFEROUS ÆTHER.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In the papers which I have communicated to your Journal on the above subject, I have endeavoured to show that the phenomena of Optics can be explained without calling in the aid of any substance of the nature of the æther, and also that what may be termed ordinary matter possesses the requisite properties to enable it to act as the medium which serves for the transmission of the undulatory movement. Although I have not anywhere in these papers laid claim to originality, yet I was quite unaware at the time of writing that the same ideas had been previously propounded. Until after the publication of the second of these papers, I regret to say that I had not read 'The Correlation of Physical Forces.' I find that so far back as 1842 the distinguished author of this work had published his views on the subject, which in almost every particular are the same as those held by myself. All credit for originality is therefore due to Sir W. R. Grove; and it is probable that, if I had known how completely he had anticipated the conclusions to which I have arrived, my papers would never have been written. As it is, however, it is due to Lord Justice Grove to make this statement; and in doing so I take the opportunity of expressing the pleasure one feels at finding his views shared by so eminent an authority.

I have the honour to be, Gentlemen,
Your obedient Servant,
ERNEST H. COOK.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FIFTH SERIES.]

SEPTEMBER 1881.

XVIII. *On the "Rotational Coefficient" in Nickel and Cobalt.*

By E. H. HALL, *Ph.D., late Assistant in Physics in the Johns Hopkins University, Baltimore*[*].

THIS article may be considered as the continuation of one published in the 'Philosophical Magazine' for November 1880, under the title "On the new Action of Magnetism on a Permanent Electric Current," in which were given the results of some quantitative investigations of a certain phenomenon recently discovered in the Physical Laboratory of the Johns Hopkins University. It will perhaps be remembered that the essential feature of this phenomenon is the setting up, in a conductor bearing an electric current, of an electromotive force at right angles to the primary electromotive force, when the said conductor is subjected to the action of a magnetic force at right angles to the direction of the current.

In the article alluded to, results were given as obtained with gold, silver, tin, platinum, iron, and nickel. The magnitude of the effect observed, relatively to the strength of the primary current, the intensity of the magnetic field, and the dimensions of the conductor, had not been determined with any accuracy in the case of nickel and tin, though it was known to be comparatively large in nickel and small in tin. The other metals ranged themselves, as regards the numerical magnitude of the effect exhibited, in the following order, viz. iron, silver, gold, platinum—the effect observed in iron being

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[*] Communicated by the Physical Society, having been read at the Meeting on May 28, 1881.

several times greater, and that in platinum several times less, than the effect in gold or silver. The fact of greatest interest, however, was that, if we called the direction of the transverse effect in iron +, that in the diamagnetic metals, *and in nickel and platinum also*, would be −*.

In view of this remarkable disagreement in behaviour between the two strongly magnetic metals iron and nickel, it seemed highly desirable to make a quantitative investigation of the effect in nickel as soon as possible, and extend the examination to the other strongly magnetic metal cobalt. Most of the experiments to be described in this article relate, therefore, to nickel and cobalt. The examination of the latter was a hasty one, and may well be described first.

No thin strips of the metal being at hand, a slice was sawn from a small block of moderately pure cast cobalt and worked into the form of a cross. To the extremity of each arm of this cross was soldered a thin strip of copper 2 or 3 centim. long, for the purpose of making the electrical connexions. The cross of cobalt with the copper strips attached was now fastened with hard cement to a strip of glass and worked down with a file to sufficient thinness. Before placing the cross upon the glass its thickness, and that of the glass also, was measured by the calipers. After cementing the two together, the total thickness was found, and, again, the thickness of the whole after the cross had been filed down. The thickness of the cross in its final condition was thus estimated at ·45 millim., to which value an uncertainty of perhaps 10 or 15 per cent. attaches.

With this apparatus it was found that the direction of the transverse effect in cobalt is +, *i. e.* the same as that in iron. As to the magnitude of the effect, $\frac{M \times V}{E'}$ was found to be 44×10^{10}, placing cobalt between silver and iron. The specimen of cobalt used, however, contained some nickel (how much is not known accurately); and this doubtless counteracted in part the effect of the cobalt. It seems probable, however, that, allowing for all errors, the transverse effect in cobalt is less than that in iron, other things being equal. The magnetic field used was about 9000 (cm.-gr.-sec.), stronger than has yet been used with iron.

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* These signs are given to avoid tedious repetitions. I have here called the effect in iron + simply because its direction in this metal is that which the conductor itself bearing the current would follow, if free to move across the lines of magnetic force under the action of the ordinary "ponderomotive" force. No significance further than this is at present attached to this choice of signs.

We now return to the consideration of nickel.

The original experiment with this metal had been made with a specimen so irregular, that it had not been possible to determine the magnitude of the transverse effect except in the most general way. The direction had been determined beyond question. The specimen of nickel now employed, and with which the results to be given were obtained, was quite different in appearance and physical condition from the first specimen—though it was obtained in about the same manner, viz. by stripping off a piece of nickel plating from the metal upon which it had been electrolytically deposited. The first specimen was very brittle, the second quite tough. The latter was about ·001 centim. thick. As to its purity hardly any thing is known except what is told by its physical characteristics. It is probably affected by all the impurities of ordinary nickel plating. It contains very likely a little cobalt, and perhaps a trace of iron. I understand, moreover, from Professor Wolcott Gibbs, that nickel plating deposited in the usual manner (*i. e.* from an ammoniacal solution) is much affected in its physical properties by nitrogen in some way retained by the metal. It would have been desirable, of course, in all cases to work with pure metals; but such were not at hand, or easily obtainable in the proper form, and it was not thought best to defer the experiments until pure specimens could be obtained*.

The second specimen of nickel showed an effect of the same sign as the first, and numerically greater than the effect which had been observed in the specimens of iron and cobalt used.

It now became a matter of great interest to determine whether the transverse effect had really any connexion with the magnetic properties of the metals. It was determined therefore to make a series of experiments, keeping the primary current through the metal as nearly as practicable always of the same strength, but varying within wide limits the intensity of the magnetic field. We should in this way ascertain whether the transverse effect was simply proportional to the strength of the magnetic field, or was related to it in some more complicated manner.

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* This may strike some readers as unwise. It has even been suggested that the difference in behaviour of iron and nickel may be due to impurities in one or the other. This suggestion implies that the transverse effect in these metals is so related to the magnetic properties that, as they resemble each other in one respect, they should also in the other, but at the same time admits that slight impurities, such as would certainly be very far from reversing the magnetic property of either metal, may reverse the transverse effect in the same. This does not seem probable.

By the term "strength of the magnetic field," as just used, is meant the intensity of the field between the poles which obtains when the metal plate is not in the field. This intensity is measured, as described in the article already alluded to, by withdrawing suddenly from the field a small coil of wire and observing the effect upon a galvanometer in circuit with the coil. This gives what is called the magnetic induction in this part of the field. In general, the magnetic induction in any magnetized space would be changed by introducing into that space a body capable of being magnetized by induction. The well-known expression for the magnetic induction within any such body placed in a magnetic field is (Maxwell's 'Treatise,' vol. ii. art. 428)

$$\mathfrak{B}=\mathfrak{H}+4\pi\mathfrak{I}\,; \quad . \; . \; . \; . \; . \; . \; . \; . \quad (1)$$

where $\mathfrak{H}$ is the *magnetic force* within the body (Thomson's 'Polar Definition,' reprint, p. 397), and $\mathfrak{I}$ is the *intensity of magnetization* (Maxwell, art. 384).

Now, in case of uniform magnetization, $\mathfrak{H}$ is equal to the intensity of the field as it would exist if the body magnetized by induction were removed (*i. e.* just what we measure by means of the coil and galvanometer), together with the force exerted by what we may call the magnetism induced on the surface of the magnetized body. This latter force will, of course, depend upon the shape and dimensions of the body. If it is a very thin disk, the reaction of the induced magnetism will, as Maxwell remarks, be equal to $-4\pi\mathfrak{I}$; and in this case, writing $\mathfrak{F}$[*] for the intensity of the magnetic field as above defined, we have

$$\mathfrak{H}=\mathfrak{F}-4\pi\mathfrak{I}. \quad . \; . \; . \; . \; . \; . \; . \; . \quad (2)$$

Substituting in (1), we have

$$\mathfrak{B}=\mathfrak{F}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3)$$

which means that, in a very thin disk magnetized by induction, the magnetic induction is just what it would be in the space occupied by the disk if the disk were removed from the field. Now the strip of nickel which we employ has a width 600 or 800 times its thickness; and it has been assumed that we may, for our present purpose, regard it as such an infinitely thin disk as Maxwell supposes. The error resulting from this assumption may easily be seen to be small. At the centre of the strip of nickel the real value of $\mathfrak{B}$ would be perhaps $\frac{1}{10}$ of one per cent. greater than the value as above determined. At a point 1 millim. from the edge of the strip the error might amount to $\frac{1}{3}$ or $\frac{1}{4}$ of one per cent.; while at $\frac{1}{10}$ millim. from

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[*] Called M in previous article.

the edge it would perhaps be two or three per cent. The average of the real values of $\mathfrak{B}$, therefore, at points along the line running across the strip from one side connexion to the other, is probably a rather small fraction of one per cent. greater than the value obtained on the assumption that $\mathfrak{B}$ is equal to $\mathfrak{F}$. This error is, to be sure, not constant; but it is nearly so up to $\mathfrak{B}=$ about 5000; and when it begins to change rapidly, it grows smaller.

Its influence upon the curve given further on must be very small. We assume therefore, as stated above, that by determining the strength of the magnetic field by means of the coil and galvanometer before the nickel is placed in the field, we ascertain with sufficient accuracy the value of the magnetic induction in the nickel strip itself when placed in the magnetic field. The advantage of determining this quantity is of course very great; for though we are probably unable to say what is the exact physical nature of magnetic induction, we do attach to the quantity represented by that term a very definite and important mathematical significance.

It was designed, therefore, to investigate the law of the variation of the transverse effect with the variation of the magnetic induction. Nickel was the best metal to experiment upon, for the following reasons: the strip of this metal at hand was very thin; the transverse effect appears to be essentially more powerful in nickel than in iron or cobalt; the magnetic permeability of nickel changes more rapidly than that of iron or cobalt with high magnetizing-powers.

As it was desired to determine simply what function of the magnetization the transverse effect would prove to be, the primary current through the nickel strip has been kept approximately constant, the greatest variation from the mean being probably not many per cent., as will be shown further on. Within these limits it has been assumed that the transverse effect may be considered a linear function of the direct current.

It should be here stated that this latter relation has not yet been proved to hold rigidly even in a non-magnetic conductor like gold; and the matter must some time be investigated, though there seems to be no reason to think that the assumption, as above limited, can prove to have involved any considerable error.

The intensity of the magnetic field, and so the magnetic induction in the nickel plate, has been varied from about 1600 to about 10,000 in absolute (cm.-grm.-sec.) measure.

In the course of this investigation I have become indebted to nearly every one connected with the Physical Laboratory of the Johns Hopkins University, but particularly to Mr. S.

H. Freeman, Fellow in Physics, and Mr. H. R. Goodnow, Special Student in Physics, who for a while carried on the experiments together. Mr. Freeman especially worked with me for a long time; and several suggestions of his in regard to the arrangement of apparatus and the method of experimenting were adopted with great advantage to the work.

In my last article on this subject the results of measurements were given in the form $\frac{\mathrm{M} \times \mathrm{V}}{\mathrm{E}'}$, where M was the strength of the magnetic field*, V was the direct current divided by the section of the conductor, and E′ was the transverse electromotive force per centimetre of the width of the strip. In that article were given certain reasons for thinking the above quantity more likely to be a constant for any given metal than the quantity $\frac{\mathrm{M} \times \mathrm{E}}{\mathrm{E}'}$, where E is the electromotive force per centimetre of the *length* of the metal strip. Recent developments, to be spoken of further on, raise the question whether the ratio $\frac{\mathrm{M} \times \mathrm{E}}{\mathrm{E}'}$ will not after all prove to be the more fundamental and invariable quantity; but as E is rather difficult to determine with accuracy, and as in any given strip of metal V is likely to remain under ordinary conditions of temperature &c. very nearly proportional to E, the use of the former quantity will be retained for this article at least. The values of M [$\mathfrak{F}$] will be given separately, however; and, for convenience in plotting the results, the quantity $\frac{\mathrm{E}'}{\mathrm{V}}$ will be used instead of $\frac{\mathrm{V}}{\mathrm{E}'}$. The values of $\mathfrak{F}$ will, in plotting, be laid off as abscissas, and the values of $\frac{\mathrm{E}'}{\mathrm{V}}$ be taken as ordinates. This method of plotting gives a simple curve in the present case, and puts the results of the experiments in form to be compared with those of previous investigations of some of the magnetic properties of nickel. It is this quantity $\frac{\mathrm{E}'}{\mathrm{V}}$ which, after Maxwell, in accordance with the suggestion of Mr. Hopkinson†, is now called the "rotational coefficient" of nickel.

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* Called $\mathfrak{F}$ in this article.

† Phil. Mag. Dec. 1880, p. 430. Prof. Rowland has (Phil. Mag. April 1881, p. 254) remarked upon Mr. Hopkinson's note. Maxwell did not know any such effect to exist. In fact he expressly stated that it probably did not exist; yet, seeing the possibility of it, he let fall the phrase which seems now best fitted to define this newly discovered property of the metals.

Mr. Hopkinson has suggested "rotational coefficient of resistance;" and possibly some quantity might be found which would demand that title. At first sight $\frac{E'}{V}$, which is an electromotive force divided by a quantity proportional to a current, would seem to be of the nature of a resistance; but it is to be noticed that the electromotive force E′ is not the cause, but the effect, of the current implied in V.

In the experiments which I have described in previous papers, no account was taken of the temperature of the conductor experimented upon. When these experiments upon nickel, however, had been going on for a long time, it began to be suspected that the temperature of the room, and so of the nickel plate, did exercise a very considerable influence upon the magnitude of the transverse effect as expressed by the ratio $\frac{E'}{V}$.

A few hasty experiments with considerable ranges of temperature in the room indicated very decidedly that the temperature was a factor to be considered, and that the higher the temperature the greater the value of E′, other things being equal. The magnitude of this influence can hardly be determined from results thus far reached. It may prove that the transverse electromotive force E′ is no more increased by a rise of temperature than the direct electromotive force E* is; and in this case it would appear, as intimated above, that the ratio $\frac{E'}{E}$ is the one to be investigated rather than $\frac{E'}{V}$.

Future investigation must determine this matter; and meanwhile it has been sought to avoid evil consequences by regulating, as well as practicable, the temperature of the nickel plate. Sometimes an experiment had to be made at a rather high temperature for instance; and an attempt would then be made to balance this by making another with about the same strength of magnetic field but at a low temperature, or *vice versâ*. There was, however, even now no attempt to determine the actual temperature of the nickel; but a thermometer was hung up with its bulb close to the plate, and as nearly as practicable always in the same position with respect to the latter, and both plate and thermometer were protected from sudden changes of temperature. As the nickel was of course heated by the current, its temperature must have been always considerably higher than that indicated by the thermometer.

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* Apparently E′ is in nickel affected by temperature more than E in most metals would be; but the rate of increase of the resistance of nickel with rise of temperature seems not to be known.

Moreover this difference must have varied somewhat with the strength of the direct current; so that the temperature read can be assumed to give only a very rough indication of the *changes* in temperature of the nickel.

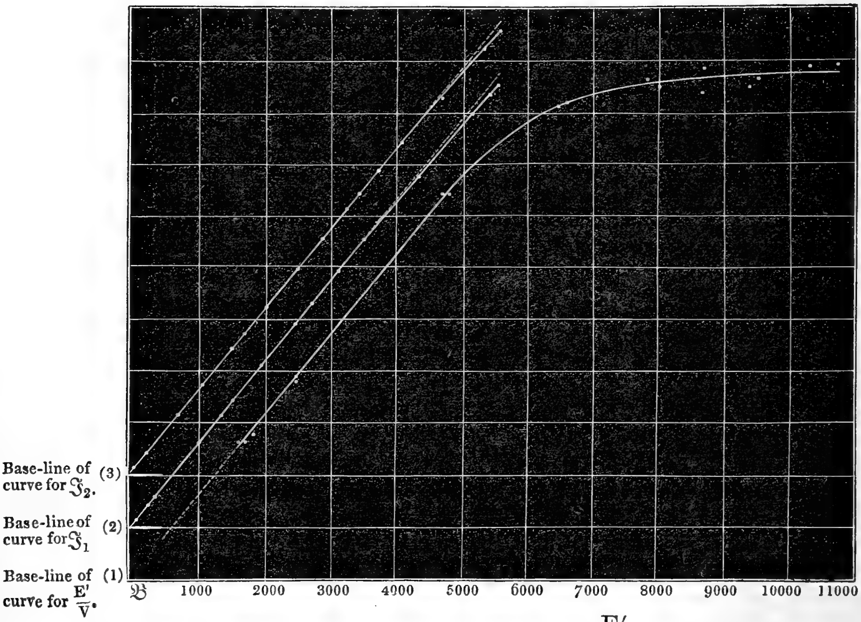
None of the numerical results of measurements made with nickel before the disturbing influence of temperature was discovered are here published. In some of the results afterwards obtained, however, the effects of variations of temperature can apparently be detected, as will be pointed out hereafter.

The general method of experiment has been already sufficiently described in previous papers. There will now be given in tabular form the most important data involved in this examination of nickel, and the values of $\frac{E'}{V}$ obtained. The absolute strength of the primary current through the nickel strip in any case is not given, as, by the method of experiment, both the constant of the galvanometer used to measure this current and the horizontal intensity of the earth's magnetism at this galvanometer (this intensity being assumed to be constant during any one determination of $\frac{E'}{V}$) are eliminated from the formula for $\frac{E'}{V}$. There will be given, however, the tangents of the angles of deflection of the galvanometer-needle, in order to show about what were the limits of variation of the primary current. It may be well to state that this current was what one Bunsen cell would send through—say, six or eight ohms. It will be seen that there are variations of about 6 per cent. in tan α; and the actual variations in the primary current may possibly have been considerably greater than this; for on March 11th, 12th, and 14th the galvanometer stood in a different room from that in which it was placed for the previous observations, and the horizontal intensity of the earth's magnetism was probably somewhat different in the two places. I have, however, as stated above, assumed that within the limits of these variations the value of E′ is a linear function of the direct current. It is evident that no large error can result from this assumption.

It will be seen from the table that the experiments began with the smallest values of the magnetizing force and went on by stages to the highest. This is the proper course to follow in order to avoid at any stage of the magnetizing force the effects of a previous stronger magnetizing force. It must, however, be stated that, before the series of experiments whose results are here published was begun, the nickel had already been several times subjected to a magnetizing force of about 7500, *i. e.* four or five times as great as the forces with which this series begins. The question, of course, arises whether there may not have been induced by this means a permanent magnetism sufficient to affect the results of subsequent experiments. In order to settle this question as far as possible, a small piece of nickel film, of the same quality as the strip in use, was first subjected to the action of a field of about 7000 or 8000. It was then placed in a field of perhaps 1500 or 1600, whose direction was such as to tend to reverse any permanent magnetization which might have been induced in the film by the previous field. It was found that now in the second field the nickel became magnetized, temporarily at least, in the direction of that field. No attempt, I believe, was made in any case to detect the permanent magnetization. In this trial the small piece of nickel film was magnetized, not in the direction of its thickness, but in a lateral or longitudinal direction; so that we do not here have an exact parallel to the case of the strip; but it seems probable that magnetization in the direction of the shortest dimension would be much more easily disturbed than that in a longitudinal direction. Moreover, just before the series of experiments was begun whose results are here published, quite a long series was made with magnetizing forces about equal to those with which the published series begins; and this treatment would have tended, no doubt, to obliterate any traces of permanent magnetism due to the action of previous higher forces, even if this permanent magnetism had been much greater than we have any reason to suppose it was. On the whole, therefore, the probability of any considerable error from this source seems to be very small.

Date.	Temperature.	Tan α.		𝔉 or 𝔅.	$\frac{E'}{V}\times 10^{10}$.
Feb. 24, 1881……	18·5°	·330		1667	209·3
25, „ ……	22·0	·332		1655	211·1
26, „ ……	21·5	·335		1664	208·1
26, „ ……	16·0	·336		1735	213·2
28, „ ……	19·5	·333		2512	314·3
28, „ ……	20·0	·333		2512	307·0
Mar. 1, „ ……	20·0	·330		4734	596·1
2, „ ……	19·5	·327		4775	596·4
5, „ ……	19·0	·338		6540	735·5
7, „ ……	20·0	·339		6415	726·7
7, „ ……	20·5	·340		7996	761·0
10, „ ……	21·5	·324		7791	771·0
11, „ ……	21·0	·342	Galv. in new position.	8712	783·5
11, „ ……	18·5	·343		8644	755·1
12, „ ……	20·0	·338		9561	772·4
12, „ ……	18·0	·338		9708	759·8
14, „ ……	21·0	·326		10720	793·3
14, „ ……	21·0	·323		10290	785·6

Laying off the values of $\mathfrak{B}$ on the base-line, and taking the values of $\frac{E'}{V}$, plotted on a convenient scale, as ordinates, we have curve (1). It will be seen that this curve is nearly straight for a considerable distance, and that if this portion were extended backward it would pass very near the origin. Between the points corresponding to $\mathfrak{B}=5000$ and $\mathfrak{B}=8000$ the line tends strongly to the right, and thenceforward it continues as if asymptotic to some horizontal line not very far above.



The points marking the highest values of $\frac{E'}{V}$ do not fall so well in line as one might wish; but by looking at the table it will be seen that there were considerable variations of temperature accompanying these observations; and to these variations the irregularities can perhaps be in some part attributed.

We see now at once from the diagram that $\frac{E'}{V}$ is not proportional to $\mathfrak{B}$, the magnetic induction in the nickel. Can we find any magnetic quantity to which it is more simply related?

If we turn to the observations of Prof. Rowland on nickel*, we find that they can, as he says, be plotted in several ways.

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* Phil. Mag. Aug. 1873 and Nov. 1874.

In order to compare them with the observations above given, we need to plot them in some manner that will lay off the values of $\mathfrak{B}$ (M in Prof. Rowland's first paper) on the base-line. We may then take as ordinates the values of the magnetic permeability, as Rowland has done in his first paper (plate iii.), or the values of κ, Neumann's coefficient of induced magnetization, or the values of $\mathfrak{H}$, the "magnetic force"* within the nickel, which would be a reversal of one method used by Rowland in his first paper (plate ii.); or, finally, we may use the values of $\mathfrak{I}$, "the intensity of magnetization according to the German theory," as Rowland calls it in his second article.

Having plotted these various curves we may compare them with (1) above, in order to determine whether our quantity $\frac{E'}{V}$ corresponds most nearly to μ, to κ, to $\mathfrak{H}$, or to $\mathfrak{I}$.

The curve for $\mu\left[\frac{\mathfrak{B}}{\mathfrak{H}}\right]$ will, long before $\mathfrak{B}$ has reached the higher values used in the curve for $\frac{E'}{V}$, have reached a maximum and returned nearly to the base-line. The curve for $\kappa \left[\frac{\mathfrak{I}}{\mathfrak{H}}\right]$ will be very similar to that for μ. We do not, then, find suggested a close connexion between μ or κ and the quantity we are studying.

The curve for $\mathfrak{H}$ bends *upward*, and is therefore quite dissimilar to that for $\frac{E'}{V}$.

The values of $\mathfrak{I}\left[\mathfrak{B}\frac{\kappa}{\mu}\right]$ obtained from two of Rowland's series†, made either with different specimens of nickel or with one specimen under quite varied conditions, give the curves (2) and (3). A separate base-line is taken for each of the three curves; and the ordinates of (2) and (3) have been plotted on different scales, in order to make the general inclination of those curves agree with that of (1). The values of $\mathfrak{B}$, however (and this is the essential particular in the plotting), are given on the same scale for all three curves. The important facts about the lines (2) and (3) are that they are sensibly straight for a long distance, that they appear to come nearly straight from the origin, and that they begin to bend perceptibly toward the horizontal when $\mathfrak{B}$ becomes 4000 or 5000. Although these lines are carried only a short distance beyond this region,

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* Thomson's 'Polar Definition,' reprint, p. 397; and Maxwell's Treatise, art. 398.

† Phil. Mag. Aug. 1873, p. 153, and Nov. 1874, p. 327.

we can yet be sure that the bend is not due to faulty observations; for to make an error of 1 per cent. in the value of $\mathfrak{J}$ at this point would require an error of very many per cent., say 20 or 30, in the value of μ as determined by Prof. Rowland. From the manner and rate at which μ was changing at the points where his experiments ceased, it seems almost certain that these lines would continue to bend, and for a time to bend rapidly. Indeed the curve in which Prof. Rowland has continued μ beyond the range of his experiments would indicate that the curves (2) and (3), if continued a short distance further, would turn downward and approach the base-line. This, however, would mean that the magnetization $\mathfrak{J}$ actually *decreases* after a certain point with *increase* of the magnetizing-force. The possibility of this is spoken of by Rowland*; but there seems to be no experimental evidence of such an effect; and if it does not exist, it appears altogether probable that the lines (2) and (3) would become asymptotic to horizontal lines lying considerably higher than any points reached by the curves as here given.

We can therefore say that, so far as actual experiments have gone, there seems to be much tending to prove a very simple and intimate relation in nickel between the transverse effect and the "magnetization according to the German theory."

It would, of course, be desirable to test for some more minute agreement than has yet been traced between the curves for $\frac{E'}{V}$ and $\mathfrak{J}$; but such a testing would probably be difficult to make. An exact agreement could not be expected; for it would probably be almost impossible to obtain exactly the same quality and condition of metal in the very different shapes required for experiments on $\mathfrak{J}$ and experiments on $\frac{E'}{V}$.

There are, however, certain minute characteristics which would belong to all curves for $\mathfrak{J}$. Thus (2) and (3) should not be straight at any point. They are lines of double curvature, the steepest part of each being not far from $\mathfrak{B}=2000$. The curvature in this region, however, is very slight; and to detect a corresponding curvature in the line for $\frac{E'}{V}$, if such exists, would be a matter of considerable difficulty, though not, perhaps, impossible.

Having gone thus far with nickel, we might, were it not for the anomaly presented by the sign of the rotational coefficient

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* Phil. Mag. Nov. 1874, p. 322.

in iron and cobalt, make a prediction by analogy as to what would prove to be the character of the curves for $\frac{E'}{V}$ in these metals. We should say that they would be sensibly straight for a much longer distance than the curve for nickel, and that in fact it might be difficult to carry the magnetization far enough to detect any marked departure from a straight course. So great a difference in behaviour as is indicated by a reversal of the sense of the transverse effect, however, makes any such predictions hazardous.

This difference of sign in the rotational coefficients of the magnetic metals is so anomalous and so important a fact, that one returns again and again to its consideration. Quite recently the determination of this sign for all three metals has been made anew. I have now tested, in all, four plates of iron (three of them having been cut from the same sheet, but the fourth being of a different thickness and probably of a somewhat different character), two plates of nickel (certainly very different from each other in condition), and one specimen of cobalt. With all these the record is perfectly consistent. Nevertheless it would be desirable to examine more specimens, and those differing widely in character. Different experimenters have observed many peculiar effects in iron under the influence of magnetism and the electric current, magnetism and mechanical strain, or the combined influence, which in a certain form we have here, of all three; and these effects appear to differ greatly, and sometimes to be of different signs, in soft iron and hard iron or steel. Thomson has found* that, under conditions of the above character, soft iron and nickel are, in certain apparently very important particulars, opposed in behaviour. I have looked in vain through all the facts of this kind with which I am acquainted for any plausible explanation of the fundamental phenomenon of the transverse action, nor can it be said that any clue has been found to the cause of the diversity observed. Nevertheless the opposition which Thomson has found in the behaviour of soft iron and nickel, under conditions of magnetism and mechanical strain, furnishes an analogy which should not be lost sight of. Thomson has moreover noticed that the effect which he was studying in soft iron became reversed in this metal at a very moderate value of the magnetizing force. It might be well to test the direction of the transverse effect also with very small intensities of the magnetic field.

An extended examination of the effect in iron and cobalt, similar to that which has been made in the case of nickel,

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* Phil. Trans. May 1878.

should be undertaken as soon as practicable. It will require very great intensities of the magnetic field and a very large battery* to carry these metals through a range of magnetization corresponding to that through which nickel has been examined. It seems doubtful whether the magneto-electric machine can be here employed, as the current which it produces may not be sufficiently uniform to be used with advantage.

The examination of the non-magnetic metals also should be continued as fast as circumstances will permit, with the object of determining the sign and, when practicable, approximately the magnitude of the rotational coefficient in every case.

In my article of last November I stated that, in accordance

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* In the experiments here detailed, the largest battery used has consisted of 48 large Bunsen cells arranged 8 in series. The resistance of each cell was probably something more than an ohm; the resistance of the electromagnet is, I think, rather less than an ohm. The resistance of the connexions was considerable, however; and the battery probably gave about its best effect.

After this powerful battery had been applied to the electromagnet, a rather singular effect was observed on returning to the use of weak currents. In making observations in the usual way to determine the strength of the field produced by these weak currents, it was found that the impulses given to the galvanometer-needle were very capricious. These observations may be arranged under two heads, + and −, according to the direction of the current through the electromagnet, this current being usually reversed after each withdrawal of the little coil from between the poles. The observations being arranged in this way, it would be found that there were occasional sudden changes of many per cent. in the readings in the same column. Of course the most obvious explanation of the phenomenon was that some connexion was loose, either in the circuit of the galvanometer and the test-coil or in that of the electromagnet. That the fault was not in the former circuit was made probable by the fact that by means of the earth-inductor, which was in the same circuit, quite uniform deflections of the galvanometer-needle were produced. To test for a fault in the magnet circuit, a tangent-galvanometer was introduced into it and its deflections observed during the series of observations on the strength of the field.

The readings of the tangent-galvanometer decreased slowly with the running-down of the current; but the changes were quite regular, and not at all of a character to account for the irregularity of the other observations. The most plausible explanation I could finally propose was, that this irregularity in the strength of the magnetic field was due to a sort of uncertain struggle between the action of the present weak magnetizing current, and the magnetization previously induced by the strong currents in the poles of the electromagnet, which are not, I believe, of very soft iron, and are probably capable of considerable permanent magnetization. I do not by any means feel able to assert, from my rather hasty observations, that there can be no other explanation. I have, however, simply thought the matter of sufficient importance to justify me in recording what seemed to be the fact.

with Prof. Rowland's suggestion, I had tested the Kerr effect with one specimen of nickel, and found it to be of the same sign as the effect which Kerr had observed with iron. In order to prevent mistakes, the experiment was repeated with iron, or rather, I suppose, with steel, the result being the same which Kerr had obtained. The surface of nickel first used in this way was the coating upon one of the plates of Prof. Rowland's absolute electrometer, the metal beneath being brass. Two other specimens of nickel have since been tried. One was a coating deposited electrolytically directly upon the iron pole of the electromagnet, the other was a nickel film fastened with soft cement to a plate of glass. Probably none of these specimens was pure; but (and this is a matter of more importance) the third was of precisely the same character and origin as the specimen in which the transverse effect was studied. The Kerr effect is of the same sign in all three plates of nickel, *i. e.* of the same sign as the effect in iron.

One specimen of cobalt has also been tested for this effect. A block of cast cobalt, quite similar to that from which was cut the cross mentioned in the first part of this article, was sawn in two, and one of the fresh surfaces was made quite smooth with a file and then polished with emery. It is not difficult to get a sufficiently good surface. An hour's work might prepare it.

With sunlight and a tolerably strong magnetic field, say 4000 (cm.-grm.-sec.), the rotation produced by cobalt was detected, and found to be of the same sign as that observed with nickel and iron.

The fact that nickel behaves like the other magnetic metals in optical effect, but differently from them in the transverse electrical effect, is on its face undoubtedly an argument against the theory which refers the two effects to the same cause. In order if possible to examine the optical effect in a somewhat different manner, an attempt has been made to detect an action of magnetized nickel upon polarized light transmitted directly through it. For this purpose a thin piece of glass was coated over a part of its surface with nickel by Wright's process*, the action being stopped before the nickel film became thick enough to be opaque. It was found, however, that the glass alone, although only about $\frac{1}{6}$ millim. thick, perceptibly rotated the plane of polarization of the light sent through it when subjected to the very strong magnetic field employed. The action produced by the nickel and glass together was of the same sign as that produced by the glass alone; and as the magnitude of the effect could not in either case be measured

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* Amer. Journ. of Science, Jan. 1877, p. 49, and Sept. 1877, p. 169.

with any accuracy, the experiment was quite negative in result. I now, however, took a glass tube, fused the end, and blew out the bubble till it burst. A piece of the exceedingly thin film thus obtained was subjected to the action of the magnet, and most strenuous endeavours were made to detect its action upon the beam of polarized light. This action must have been exceedingly slight, though there is some evidence, which it is not necessary to give, that it was detected. The glass, however, was coated as the first piece had been, and again with its coating subjected to the action of the magnet. The trial was, for certain reasons, rather unsatisfactory; and although no rotation of the plane of polarization was now detected, I do not think this fact can be taken as evidence that the effect of the nickel had counterbalanced the effect of the glass. Both these experiments on direct transmission have been, we may say, quite negative; but these details are given as marking out a line of research which will probably be some time resumed.

An endeavour has also been made to detect a possible rotational effect due to reflection from silver when strongly magnetized*. For this purpose two strips of silver upon glass were used; and these strips were fastened one upon each pole of the magnet, the silvered surfaces being turned toward each other and as nearly parallel as practicable. The poles being brought near together and the light being let in between the silvered surfaces at a large incidence, it was possible to obtain twenty or thirty successive reflections before the beam emerged toward the analyzing Nicol. Certain difficulties were introduced by this arrangement; but in spite of these I think that, if the action of silver had been one tenth as strong as that of iron, the effect would have been detected. No such effect was observed.

XIX. *On the Results obtained from a Modification of Bunsen's Calorimeter.* *By Prof.* B. STEWART *and* W. STROUD†.

A DESCRIPTION of this instrument was brought before this Society on June 26, 1880, and afterwards appeared in the Proceedings of the Physical Society, vol. iv. p. 52, and Philosophical Magazine, vol. x. p. 171. The results obtained at that time were not very good, owing

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* In this experiment I had the very efficient assistance of Mr. Arthur W. Wheeler, Fellow in Physics, whose untimely death is so deeply lamented.

† Communicated by the Physical Society, having been read at the meeting on June 26, 1881.

greatly to the large bore of the capillary tube, which necessitated the use of a large quantity of substance, the consequence being that the mercury in the bulb was so much heated that the loss of heat by radiation was not inappreciable during the time of performing the experiment.

Since then a new instrument has been obtained from Mr. Casella, having a much finer capillary stem with an empirical scale engraved on it. By means of it very good results have been obtained with very small quantities of substances; in fact, it has been found (within certain limits) that the smaller the mass of the substance the better.

At first we endeavoured to make allowance for the loss of heat during the experiment by constructing a curve of cooling, and adding to the rise obtained by dropping in the substance the amount that had been lost while the experiment was being performed, according to the curve of cooling. This method, however, did not yield good results.

Our final method of operating is comparative only; that is to say, one metal, whose specific heat is accurately known, is chosen as the standard to which all other substances are referred, and, in order that the conditions may be the same in all cases, we first ascertain the rise obtained by introducing a given weight of mercury at a given temperature (that of the air), and when the instrument has cooled throw in the substance whose specific heat is desired. During the two experiments the temperature of the air is usually nearly constant; and in that case no correction for temperature is required.

A second point to be noted, is to put in such a weight of substance as will produce about the same rise as was obtained with the mercury. It is thus possible to make the determination of the relative specific heats under almost precisely the same circumstances, a condition which is eminently conducive to good results.

In practical working with the instrument the following hints may prove useful:—

1. The test-tube is always filled with water to the same height before being cooled, this height being that of the level of the copper or slightly above it.

2. About ·6 grm. of mercury is placed at the bottom of the tube, unless the substance to be experimented on is attacked by mercury. The object of this is to allow the solids that are introduced to part with their heat more freely. The substance comes thus into much more intimate heat-contact with the mercury of the thermometer than it would if it rested upon the glass alone. If, for instance, some pieces of sulphur

are allowed to fall to the bottom of the tube, they sink partly into the mercury, and quickly give up their heat to it, so that it is more nearly the same as if the mercury had been heated.

3. A large-sized box is employed, in which plenty of ice is placed, so as to completely surround the instrument.

4. As the mercury sometimes moves very sluggishly, gentle tapping, not vertically, but horizontally, has been found very beneficial.

We will now take an example to illustrate the method of working :—0·396 grm. of mercury was weighed out and allowed to remain for some time in contact with the bulb of a thermometer. Meantime the instrument was gently tapped and the height read. The mercury was then poured in after the thermometer-reading (18°·6 C.) had been taken. The rise obtained on the scale was 15. An experiment with iron was then made ; and since the specific heat of iron is about four times as great as that of mercury, ·144 grm., or about one fourth the weight of the mercury, was taken. The temperature was still 18°·6 ; and the maximum rise obtained was 18 ;

$$\therefore \text{ specific heat of iron } = \cdot 0333 \times \frac{18}{15} \times \frac{396}{144} = \cdot 110.$$

The following results have been obtained :—

Substance.	Weight.	Temperature.	Rise.	Specific heat. Hg=·0333.
Mercury	·364	16·8	15	
Sulphur	·148	17·1	32	·174
Mercury	·325	16·2	10	
Iron......................	·144	16·3	15	·110
Sulphur	·134	16·2	22	·178
Mercury	·396	18·6	15	
Iron	·144	18·6	18	·110
Sulphur	·081	18.6	16	·174
Mercury	·400	18·0	14·5	
Iron......................	·144	18·0	18·0	·115
Sulphur	·083	18·0	15·5	·175
Mercury	·505	15·8	14·5	
Iron......................	·145	16·2	13·5	·112
Sulphur	·118	16·2	17·0	·177

		True.
Mean specific heat, iron......	·1118	·1138
„ sulphur	·1756	·1776

XX. *On a Systematic Interruption in the Order of Numerical Values of Vulgar Fractions, when arranged in a Series of consecutive Magnitudes. By* Sir G. B. Airy, *K.C.B., Astronomer Royal*[*].

SOME years ago I prepared, with the aid of William Ellis, Esq. (Assistant of the Royal Observatory of Greenwich), the numerical logarithmic values of all the Vulgar Fractions $\frac{m}{n}$, in which m and n are prime to each other and do not exceed 100; and arranged them in a Table in order of magnitude of the logarithms. The number of the fractions is 3043. On occasion of introducing this Table at a discussion of the Institution of Civil Engineers, which involved the determination of the number of teeth of wheels required to produce a given proportion of angular speed, the Council of that Institution decided to print the Table in the Selected Papers of their Transactions.

The form of the Table may be understood from the following specimen (taken at hazard):—

Vulgar fraction.	Logarithm.	Difference.
$\frac{64}{53}$	·08190	·00009
$\frac{93}{77}$	·08199	·00020
$\frac{29}{24}$	·08219	·00023
$\frac{81}{67}$	·08242	·00011
$\frac{52}{43}$	·08253	·00014
$\frac{75}{62}$	·08267	·00007
$\frac{98}{81}$	·08274	·00024
$\frac{23}{19}$	·08298	

The average value of difference for the first 72 fractions is ·00010; for the following 116 fractions it is ·00014; for the next following groups of 116 each, it is ·00016, ·00016, ·00017, ·00018, ·00019, and so on, slowly increasing till we come to high numbers. As representing the ordinary range of differences, these numbers are too large, because they include the effect of the special large differences, of which I have now to treat.

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[*] Communicated by the Author.

In ranging the eye down the columns of differences, it is seen at once that there are some differences much larger than the rest. On close examination, it is seen that they belong exclusively to the Vulgar Fractions whose denominators are small—1, 2, 3, &c. I now give the vulgar fractions and the logarithms for several of these instances, accompanied with the fractions next preceding and next following, to show the magnitude of the differences.

Denominator of Vulgar Fraction = 1.

$\frac{99}{50}$	·29667		$\frac{98}{33}$	·47272		$\frac{99}{25}$	·59770	
		·00436			·00440			·00436
$\frac{2}{1}$	·30103		$\frac{3}{1}$	·47712		$\frac{4}{1}$	·60206	
		·00441			·00437			·00450
$\frac{99}{49}$	·30544		$\frac{100}{33}$	·48149		$\frac{97}{24}$	·60656	

$\frac{99}{20}$	·69461		$\frac{95}{16}$	·77360	
		·00436			·00455
$\frac{5}{1}$	·69897		$\frac{6}{1}$	·77815	
		·00455			·00450
$\frac{96}{19}$	·70352		$\frac{97}{16}$	·78265	

Denominator of Vulgar Fraction = 2.

$\frac{100}{67}$	·17393		$\frac{97}{39}$	·39571		$\frac{94}{27}$	·54177	
		·00216			·00223			·00230
$\frac{3}{2}$	·17609		$\frac{5}{2}$	·39794		$\frac{7}{2}$	·54407	
		·00223			·00223			·00229
$\frac{98}{65}$	·17832		$\frac{98}{39}$	·40017		$\frac{95}{27}$	·54636	

Denominator of Vulgar Fraction = 3.

$\frac{97}{73}$	·12345		$\frac{98}{59}$	·22038		$\frac{100}{43}$	·36653	
		·00149			·00147			·00145
$\frac{4}{3}$	·12494		$\frac{5}{3}$	·22185		$\frac{7}{3}$	·36798	
		·00147			·00149			·00151
$\frac{99}{74}$	·12641		$\frac{97}{58}$	·22334		$\frac{96}{41}$	·36949	

The order of these numbers (which have been examined much further) is sufficiently clear. For the critical or simple values of $\frac{m}{n}$, the logarithmic differences immediately preceding or following these simple values are $\frac{\cdot 00440}{n}$ nearly. Now ·00440 is nearly the logarithm of $1+\frac{1}{100}$. So that the values of the three neighbouring fractions in the first instance with denominator 1 are, $2\times\left(1-\frac{1}{100}\right)$, 2, $2\times\left(1+\frac{1}{100}\right)$; in the second instance, $3\times\left(1-\frac{1}{100}\right)$, 3, $3\times\left(1+\frac{1}{100}\right)$; in the third instance, $4\times\left(1-\frac{1}{100}\right)$, 4, $4\times\left(1+\frac{1}{100}\right)$, &c. And with denominator 2, the values in the first instance are $\frac{3}{2}\times\left(1-\frac{1}{200}\right)$, $\frac{3}{2}$, $\frac{3}{2}\times\left(1+\frac{1}{200}\right)$; in the second instance, $\frac{5}{2}\times\left(1-\frac{1}{200}\right)$, $\frac{5}{2}$, $\frac{5}{2}\times\left(1+\frac{1}{200}\right)$, &c. And so for the other denominators. And, between the values thus set down, there is no other value of a vulgar fraction whose numerator and denominator do not exceed 100.

Now the remarkable circumstance attending these large logarithmic differences is, that they all occur in the midst of small differences. Thus we have

$\frac{89}{45}$	·29618	
		·00010
$\frac{91}{46}$	·29628	
		·00010
$\frac{93}{47}$	·29638	
		·00010
$\frac{95}{48}$	·29648	
		·00009
$\frac{97}{49}$	·29657	
		·00010
$\frac{99}{50}$	·29667	
		·00436
$\frac{2}{1}$	·30103	
		·00441
$\frac{99}{49}$	·30544	
		·00009
$\frac{97}{48}$	·30553	
		·00009
$\frac{95}{47}$	·30562	
		·00010
$\frac{93}{46}$	·30572	
		·00011
$\frac{91}{45}$	·30583	

and so for the others. Thus it appears that, though in general the most confident reliance may be placed on this Table for finding a vulgar fraction whose value shall very nearly correspond to a given value, yet the particular cases must be excepted in which the given value approaches *very* nearly to one of the critical fractions, $\frac{2}{1}, \frac{3}{1}$, &c., $\frac{3}{2}, \frac{5}{2}$, &c., $\frac{4}{3}, \frac{5}{3}, \frac{7}{3}$, &c.; and so for others.

This is the point to which, as the leading subject of this paper, I wished to invite the reader's attention. But, as regards the practical use of the Table, in every case (including all these excepted cases) the Table may be made available for exhibiting two vulgar fractions whose product will exhibit the required number with great accuracy, and that in many different ways. Suppose, for instance, the number to be produced is 2·0098, whose logarithm is 0·30315. The nearest logarithms in the Table are 0·30103 and 0·30544, which are far too wide for any accurate purpose. But (among many equally favourable combinations) there is no difficulty in fixing on the tabular numbers $\frac{91}{61}$ whose logarithm is 0·17371, and $\frac{97}{72}$ whose logarithm is 0·12944. The sum of these logarithms is 0·30315, as was required; and therefore $2{\cdot}0098=\frac{91}{61} \times \frac{97}{72}$.

On performing numerically the product of these vulgar fractions, it is found to be 2·00979.

Instead of sum of logarithms of vulgar fractions, difference might be used if it were thought desirable.

Royal Observatory, Greenwich,
1881, August 4.

XXI. *Note on the Laboratory at St. John's College, Oxford.*

By R. H. M. Bosanquet[*].

In the paper on Beats recently published, the examination of certain simple forms of Beat was described. The then existing arrangements were not free from disturbing noises (engine, shafting, &c.); and it appeared to me to be a waste of time to proceed further with these investigations until some improvement in this respect could be effected.

A small hydraulic engine was procured and attached to the main supply. The pressure available proved, however, insufficient, as I had anticipated; and the attempt was consequently a failure.

Under these circumstances I applied to the College for

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[*] Communicated by the Author.

permission to erect an additional room near the laboratory, in such a position that I could drive the bellows in the new room by a belt from the old shafting, running through the windows. Permission was accorded; and the new room is now in process of erection. When it is finished, I hope to make a more thorough examination of the phenomena in question in the direction indicated in the above paper.

The engine originally employed proved to be defective, in consequence of its excessive consumption of steam. A small 2-horse-power vertical engine has been substituted for it. This is amply sufficient for the work required ; and the steam available is more than enough. The engine is now kept going the whole of every working day; and it is consequently always available for any work that may be required.

The bellows suffered materially from the high temperature generally maintained in the laboratory. It has now been remade, and the table on which the slides work has been leathered. It answers well under these conditions, admitting of the employment of a heavy wind for mechanical purposes, such as it would hardly have borne in its original form.

A few words may be not out of place on the question of leathering the table. This practice is never resorted to as a rule by English organ-builders, but always by German builders. English builders consider that they can plane all the surfaces concerned so perfectly that leathering is unnecessary. In actual organ-work small channels are also resorted to, to carry off any wind that may escape between the planed surfaces. However this may have answered with old English organs, in which a very light pressure of wind was employed (generally about 2 inches of water), it certainly fails to give satisfactory results with the heavy pressures now constantly used; and I am of opinion that the tables ought to be leathered much more generally than is the case. In such an instrument as my bellows I consider the leathering of the tables to be essential.

There is another point of importance. When the bellows is loaded beyond a certain point (in my instrument to about 8 inches of water), the action of the escape-valves when the bellows is full gives rise to vibration. It is proposed, in order to get rid of this, to adopt a form of escape-valve made in two pieces, so that a small bit at the end of the valve is lifted first, and then the valve itself later. In this way the sudden shock which causes the vibration will probably be got rid of. But the load of 7 inches is sufficient for all purposes at present; and the improvement in question will be only with a view to the general perfection of the instrument.

A few words may be said about the improvements in the arrangement of the bottle-notes* with which my investigations on beats have been conducted. It was found that the clips used to hold the india-rubber tubes, through which water was admitted to the bottles, were not sufficiently tight, and the arrangement of a small reservoir to each note was not convenient. The corks were also frequently defective; and it was found difficult to procure them sound when of the proper size. A number of india-rubber corks were obtained, two inches in diameter; and the bottles were fitted with these. A number of glass stopcocks were also obtained, such as are used in ordinary chemical apparatus. In this way the difficulty as to the tightness of the water-tubes was completely overcome.

An india-rubber tube was carried from the water-supply of the laboratory, and slipped on to any of the glass-stopcock tubes into which water was to be introduced. An aspirator was similarly employed at first to remove water when required. But it was ultimately found more convenient simply to detach the end of the india-rubber tube from the water-supply, and let the end hang down over the waste. When the india-rubber tube is full of water, this constitutes a simple and efficient aspirator. By these means any of the notes can be tuned with considerable accuracy; also the notes can be made to change slowly and continuously at any desired speed, by running the water slowly in or out. This is an important point in the demonstration of the properties of difference-tones, as some, for instance De Morgan, have maintained that the combination-tones only exist at definite points. By this arrangement the change of the difference-tones can be followed continuously throughout their whole course. The ordinary difference-tone was long ago demonstrated by Tyndall continuously, so that the point is not new; but the present method of demonstration is very convenient.

The bottles do not give good notes much above the middle of the treble staff (about a'). For notes above this, so far as I have examined them, I have used ordinary organ-pipes, both stopped and open, as in this region little depends on the quality of the notes employed. But the particular phenomena I have hitherto worked at are most clear in the lower part of the scale, in which the peculiar purity of the bottle-notes is available for the demonstrations.

During the last severe winter the laboratory was almost uninhabitable, and little was done. An arrangement for heating by gas with ventilation proved a failure; the moisture

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* Phil. Mag. 5th ser. vol. viii. p. 292.

condensed in the ventilating-tubes, and injured the machinery. The arrangement has been removed.

The principal work done since that time has been the construction of two disk machines and the clock governor. One rather elaborate disk machine with clock governor attached is finished and ready for experiment. The disks are made of brass, and have thirty radial slots. If the machine is successful, it will enable determinations of tuning-forks to be made with great accuracy by merely striking them and looking at them through the revolving disk.

A good deal of incidental work has been done, on the principle that it is best to provide complete and efficient apparatus for every point as it arises. In particular, considerable additions have been made to the lathe, including an overhead motion (the existing back-motion proving insufficient for some purposes).

In slotting the disks in the lathe, it was found that saws of the ordinary form were unsuitable for the purpose, as they soon choked when imbedded in the brass. This difficulty was overcome according to a principle suggested by Willis. Where choking takes place, the number of cutting-teeth is to be reduced. A small steel circular saw was therefore constructed with four teeth only on the circumference; by means of this the disks were slotted on the lathe without difficulty. A new tool, however, has been recently invented in Oxford, which promises to overcome this difficulty by the application of more power to the ordinary saw. It will also extend greatly the applicability to all forms of circular cutter, whose use has hitherto been limited by the same difficulty.

The first disk machine constructed consisted simply of a solid foundation carrying the spindle on which the disk revolved with a small driving-pulley, and a counting-arrangement. It was driven by a fine string which passed round the driving-pulley and a large pulley on the shafting over head. The string slipped without much friction on the driving-pulley. So long, then, as the speed of the shafting was greater than that corresponding to the required speed of the disk, the disk could readily be kept to any exact speed by the application of any simple form of break. I think this arrangement very promising; but, from various circumstances, it did not seem to be exactly what I was immediately in want of, and I proceeded to construct the second machine, which is of a more elaborate character.

The driving part of the second machine is worked by an endless rope and a weight. The rope passes also through an independent machine by which the winding-up is effected either by the engine or by hand. In this way a power is secured which is independent of the variations of the ultimate source of power. The arrangement by which the endless rope drives and is wound up are identical, and deserve a moment's attention.

This arrangement is founded on a combination attributed to Sir C. Wren, described in Willis's 'Principles of Mechanism.' The description, however, is either incomplete or inaccurate; and I had to try some models before I arrived at the form I have adopted.

The arrangement of Wren is figured at p. 431 of Willis. It consists of two parallel cylinders, in each of which four circular grooves are cut. The rope is passed round the cylinders as if it were wound on the two together from outside, being in contact with half a circumference at each point of contact, and thence passing to the other cylinder. I have found the peculiarities on entering and leaving the system best avoided. In Willis's description the machine is supposed to be applied to winding up a weight, and the force is shown as if applied by a handle to one of the cylinders only. I made a model, and found that it would not do, as I suspected; for there are only three half turns of the rope on the cylinder to which the power is applied, and that is not enough to secure adhesion. The second cylinder, with its three or four more half turns, contributes nothing to the application of power. But it seemed to me that if both cylinders were driven by an intermediate toothed wheel, so that they were rigidly connected, the end required would be attained, as there would be then six half turns of the rope, equivalent to three whole turns, in contact with the power, and this would be enough to secure adhesion. And this in fact turned out to be so. I had the cylinders cast with the grooves upon them, and then bored them out in the lathe true to the grooves, so as to avoid removing the rough skin of the cast metal in the grooves. This gives the rope ample hold; and the arrangement works satisfactorily. A separate rope-ratchet is employed in the winding arrangement, which prevents the cylinders from letting the weight down, and works noiselessly.

The pair of driving-barrels drives a train of which the lowest wheels are cast iron and the upper gun-metal. The mode in which the power is transmitted to the disk-spindle is peculiar. A screw of half-inch pitch was cut in the lathe on the half-inch steel spindle; this is driven by a gun-metal wheel about 3 inches in diameter, on which 18 teeth are cut; and the action between these teeth and the screw presents some curious points. The only model I could find for such an action is that of musical boxes, whose fly-wheels are driven by an arrangement of this kind. In these the teeth of the driving-wheels are cut quite roughly; they are mere slits, and are cut straight, not to the angle of the screw. I first cut the required teeth in the shape of square slots, cut to the angle of the screw, but not rounded or finished in any way. The wheel so cut drove the disk beautifully. I then gave some further cuts to the sharp edges, and rounded them off so as to fit the surface of the screw rather neatly. The wheel then refused to drive the disk at all. It appeared as if the greater surface in contact increased the friction so as to stop the action. Eventually I cut away the greater part of the rounded edges of the teeth again, and the wheel now drives the disk quite well. I believe that straight square slots, in which the surface in contact would be reduced to a minimum, would be the best form of tooth for this purpose.

The differential-velocity machine, which forms the essential part of the clock governor, has been constructed according to the description given in my original paper. No points calling for mention arose in connexion with it.

A number of electro-pneumatic levers have been supplied by Messrs. Bryceson, for the purposes indicated in my original paper. Connected with the bellows, having wind of a pressure equivalent to 7 inches of water, each lever lifts easily a 2-lb. weight through nearly an inch, on passing a current through its circuit. A pair of these levers is applied to drive one side of the differential-velocity governor from the clock, while the other side of the governor is driven by the disk-machine.

When this was all ready, and the electric connexions were tried, it appeared that there was something wrong with the circuits, which consist of two underground cables connecting my rooms with the laboratory. As much with a view to general convenience as for this particular examination, I took the opportunity of forming a sufficient set of resistance-coils and other testing-apparatus. The resistance-coils were copied from standards in my possession. One of the cables proved to have been cut across, forming a fault with an earth-connexion. The Leclanché battery, which had always given trouble, proved also insufficient for maintaining the requisite power for any length of time. A suitable battery has not yet been obtained; but that will not present any difficulty. A clock with wooden seconds' pendulum has been prepared for occasional use in the laboratory itself, that we may not be interrupted by any further failure of the circuits. But it is judged best to keep the standard clock in my rooms, where it is unaffected by the variations of temperature that occur in the laboratory. Some preparation has been made for other work; but I confine myself here to what has been actually done. It is hoped that the difficulties are now for the most part overcome, and that the attainment of further results is not far distant.

XXII. *Remarks on* Dr. Mills's *Researches on Thermometry.*—No. II. *By* T. E. Thorpe, *Ph.D., F.R.S., and* A. W. Rücker, *M.A.*[*]

In a criticism on Dr. Mills's Researches on Thermometry, published in the July number of the Philosophical Magazine, we stated that we believed that he had committed an error which "upsets all the inferences drawn by him from his formulæ" relating to the movement of the zero of a mercurial thermometer with changes of temperature. In his reply, Dr. Mills informs us that he has "so far failed to follow" the section of our paper in which this remark occurs, "that he is unable to discuss it." It is evident that the question at issue between us cannot thus be left unsettled. We purpose therefore briefly to notice some of the points on which Dr. Mills has replied to us, and to attempt to make still clearer those which he disregarded as unintelligible.

Exposure Correction.—Dr. Mills is wrong in thinking that we questioned the accuracy of his statement that the "correction-factors" for his thermometers were the same at certain temperatures named by him. What we stated (Phil. Mag. July 1881, p. 12) was, that his method of presenting his results "tends to exaggerate the importance of the variation of the corrections." To this we adhere—and the more strongly, inasmuch as Dr. Mills's immediately succeeding remarks afford an example of the confusion introduced into the discussion of the subject by his habit of dealing, not with the quantities which it was his object to determine (*e. g.* corrections, positions of the ice-point), but with subsidiary quantities (correction-factors, total remaining ascents), by which they are only indirectly measured.

We stated that, in our opinion, Dr. Mills's experiments prove that the "exposure corrections [expressed by $y=(\alpha+\beta N)(T-t)N$] below 100° C. are in the case of three out of four similar thermometers practically identical;" and we supported our view by an example calculated for 100° C. Dr. Mills retorts that our inference is clearly wrong, because at lower temperatures the differences of the *correction-factors* (*i. e.* of $\alpha+\beta N$) are greater than at 100°. This we fully

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[*] Communicated by the Authors.

admit; but, on the other hand, at the lower temperatures both N and $T-t$ must be less than at 100°. They, too, are factors of the correction; and if their variations are taken into account, our statements that the example worked out by us was favourable to Dr. Mills's views, and that the *corrections* for three of the thermometers were practically identical below 100°, are strictly correct. Let us investigate this point a little more fully.

The corrections may be considered as practically identical if, in the case of any thermometer, we may substitute for the correction peculiar to it the mean of the corrections obtained from all the thermometers.

For the thermometers referred to by us (2, 3, and 6) the expression for the mean correction is

$$y=(\cdot 00013591+\cdot 000000051152\,\mathrm{N})\mathrm{N}(\mathrm{T}-t).$$

Subtracting this from the correction for thermometer 2, we get the difference

$$\delta=(\cdot 00000557-\cdot 000000013272\,\mathrm{N})\mathrm{N}(\mathrm{T}-t).$$

If N varies, the maximum value of this expression occurs when $\mathrm{N} = 210$ scale-divisions (each of which on Dr. Mills's thermometers was a millimetre in length, and corresponded approximately to $0^{\circ}{\cdot}25$ C.). Making the same liberal assumption as in our last communication, that $\mathrm{T}-t=100$, we obtain, if we write Δ for the maximum difference from the mean correction, $\Delta=0{\cdot}06$ division.

For thermometer 3 we get (neglecting signs) $\Delta= 0{\cdot}07$ div. when $\mathrm{N} = 335$ div. In the case of thermometer 6 the two curves intersect below 400 div. The correction-difference is a maximum and $=0{\cdot}01$ div. when $\mathrm{N}=111$ div., is zero when $\mathrm{N}= 222$ div., and is $=0{\cdot}05$ div. when $\mathrm{N}=400$ div. The largest of the above values of Δ is a little above the error of reading; and we may for a moment digress to say that we do not think there is any difference of opinion between ourselves and Dr. Mills as to the value of this quantity. We said it is "impossible to read with certainty to less than a tenth" of a millimetre. Dr. Mills can read "fairly" (*i. e.* not with certainty) to a twentieth. We have frequently had occasion to read the same thermometer independently. We invariably agree to $0^{\mathrm{c}}{\cdot}01$ (*i. e.* on our thermometers to a tenth of a millimetre), thus showing that the error of reading is $\pm 0{\cdot}05$ millim.

To return, however, to our argument. If, instead of taking the three thermometers with respect to which our statement was made, we take all four, we find for thermometer 4 (that previously excluded) $\Delta = 0{\cdot}23$ div. when $\mathrm{N} = 400$ div. For thermometer 2, $\Delta=0\cdot11$ when $N=273$. In the case of thermometer 3, $\Delta=0\cdot01$ when $N=156$; δ vanishes when $N=312$, and is 0·01 when $N = 400$. Finally, in the case of thermometer 6, the largest value of δ is 0·13, which occurs when $N=400$.

Of these quantities, the value of Δ for thermometer 4 is considerably in excess of the error of reading. It is not in this case, however, a question of the error of reading only. An experiment conducted with an exposed scale is in that respect conducted under unfavourable circumstances; and the other errors thus introduced are far more important. This can readily be shown from Dr. Mills's own paper.

He says (Trans. Roy. Soc. Edinb. 1880, p. 570) that there appeared to be in the experiments some source of variation which I could not detect." Thus the value of the correction-factor varied between 0·000113 and 0·000164 in the first of the three series of observations of which details are given, between 0·000136 and 0·000165 in the second, and between 0·000140 and 0·000166 in the third. On the other hand, the mean correction-factor for all the thermometers, and that for thermometer 4, both calculated for $N=400$ div. (for which in this case δ has its largest value), are 0·000155 and 0·000149 respectively. The difference between these numbers is evidently well within the above limits of error, and is in fact exactly equal to the probable error of a single determination as calculated by Prof. Mills.

On the whole, then, we are disposed to extend rather than to withdraw our previous statement, and to say that Dr. Mills's experiments go far to prove that the exposure correction below 100° C. is practically the same for all similar thermometers. Nor is this a matter of slight importance. Dr. Mills's investigation of the exposure correction of one of his thermometers involved 2160 readings. Had his conclusion that "each thermometer is proved to have its own independent equation for exposure correction" been correct in the sense that the differences were important, every observer working with exposed thermometers, who wished to attain the highest possible degree of accuracy, would in like manner have been compelled to make hundreds or even thousands of observations on each instrument. He would have had either to repeat Dr. Mills's research, or to undertake some not less difficult and laborious comparison with a standard for which the correction was known. Dr. Mills cannot, therefore, be surprised if, before consenting to accept such a yoke, we closely examine the grounds upon which it is imposed. We believe that he has rendered a greater service to his fellow-workers than he himself supposed—that he has removed, instead of imposing a burden. If he had bestowed some of the attention he has devoted to the variations of the correction-factor on those of the correction itself, he would not have left it for us to point out that the latter are negligible. As it is, the error introduced by taking the mean correction for similar thermometers instead of that for the thermometer actually employed, never exceeded the probable error of an observation ; and the chances appear to be three to one that, with a given thermometer, under the most unfavourable circumstances of exposure and temperature-difference, it cannot exceed half that amount.

Passing on to the next point noticed by Dr. Mills, he is mistaken in thinking that, by a confusion between the correction-factor and the correction, we have attributed to him the statement that it is possible "for the correction for a thermometer with an exposed column 166 divisions long to be equal to that of another when no part of the column is exposed." As a matter of fact his paper does, owing to a misprint, implicitly contain this statement; and the passage to which he refers is a *reductio ad absurdum* to show that a misprint must exist.

Zero-movements.—On the question of nomenclature we have little to add. We agree that words are sometimes used in other than their usual significations in "physical writings." In such cases, however, good reasons for departure from ordinary usage are generally forthcoming, and the word is defined on its first introduction in its new sense.

We now come to the point which Dr. Mills fails to understand ; and in our further remarks upon it we will confine ourselves to the case of Henrici's thermometer.

Unfortunately, we cannot discuss the question "mathematical formulæ apart," as Dr. Mills seems to wish. The whole point turns upon the deductions drawn by Dr. Mills from his mathematical formulæ, which cannot, therefore, be dispensed with.

The "total remaining ascent" of Henrici's thermometer is expressed by Dr. Mills by the equation

$$y = 2{\cdot}100({\cdot}931)^x - {\cdot}099(1{\cdot}360)^x. \quad . \quad . \quad . \quad (1)$$

We concluded that the position of zero (Z) is connected with y by the relation

$$Z = -2 + y, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

a conclusion supported by the fact that this equation holds for all the five values of y and of the "zero observed," given in Table VII. But if this is so, since y has no maximum or minimum value, Z cannot have one either. The motion of the zero is thus given by the formula as unchangeable in direction. It is not reversed when $y=0$; and the evidence, if of any value outside the range of the experiments, is directly opposed to the theory of the three movements of the zero of a thermometer.

To make the matter clearer, we reproduce Dr. Mills's table with three additional rows, which are separated from the rest by a line, and with some additions to the headings of the columns.

x.	Zero observed. Z.	y $=2+\mathrm{Z}$.	y calculated from formula (1).
0	0·00	2·00	—
1	−0·10	1·90	1·92
3	−0·25	1·75	1·73
4	−0·40	1·60	1·61
5	−0·60	1·40	1·45
9	−1·81	......	0·19
9·35	−2·00	......	0·00
10	−2·41	......	−0·41

The last three numbers in the second column are calculated from the formula $y=2+\mathrm{Z}$. The Table shows that the zero continues to descend though y becomes negative. The same considerations may, *mutatis mutandis*, be applied to Dr. Mills's own thermometers. The only one for which there is a critical value is thermometer 3; and that corresponds to a negative value of x. There is, we believe, no flaw in this argument, and it certainly is not abstruse; but, in view of the ambiguity of the term "total remaining ascent," we assumed, when writing before, that Dr. Mills might be able to explain the difficulty. We therefore reminded him of certain canons for the use of empirical formulæ, which, if his conclusions were not invalidated by the previous objection, were applicable to the case under discussion. His admission that these were not novel does not explain his disregard of them.

In concluding his reply to our paper, Dr. Mills seeks to reduce our criticisms to "an obvious misprint, a slip in arithmetic, a question of mere nomenclature, and the like."

The misprint was only incidentally noticed in a discussion as to whether his method of exposition did not exaggerate the importance of the variations of the exposure correction—a discussion which, as we have seen, involves the whole scope and meaning of this portion of his work.

The slip in arithmetic occurred in the formula which, though Dr. Mills has never employed it himself, is the first to which any one desirous to make use of his results would turn.

The question of nomenclature was avowedly introduced to show that Dr. Mills had involved the subject in some obscurity, and that therefore, if we had misunderstood him in the more important matter to follow, we must ask for indulgence.

The remainder of our paper was occupied with a discussion as to the accuracy of his deductions from his equations. He himself says (*loc. cit.* p. 574) it is important to illustrate these; and he can therefore hardly stigmatize as triviality the question as to whether the illustrations are correct.

XXIII. *Inverse Problem of Criticoids. By* Sir JAMES COCKLE, *M.A., F.R.S., F.R.A.S., F.C.P.S., Mem. Lond. Math. Soc., Corr. Mem. Lit. and Phil. Soc. Manchester, Hon. Mem. Roy. Soc. New South Wales, late Pres. Queensland Phil. Soc.*[*]

1. IN a "Supplement on Binomial Biordinals," recently printed in the 'Proceedings of the London Mathematical Society' (vol. xiii. pp. 63–72), I have, in certain cases, linked a binomial of the second with one of the third order wherein the symbolical factors of both numerator and denominator are in arithmetical progression. The theory of criticoids expounded in these pages sheds a light upon this result and gives a foreshadowing of others.

2. All binomial terordinals may be included in

$$\frac{d^2y}{dx^3}+\frac{3}{x}\,\frac{a+ex^n}{1+x^n}\,\frac{d^2y}{dx^2}+\frac{3}{x^2}\,\frac{f+gx^n}{1+x^n}\,\frac{dy}{dx}+\frac{1}{x^3}\,\frac{h+kx^n}{1+x^n}\,y=0.$$

3. For brevity I put

$$\begin{array}{lll} 1+x^n, & a+ex^n=\mathrm{X}, & a_1, \\ f+gx^n, & h+kx^n=a_2, & a_3, \end{array}$$

and denote differentiations by accents, thus, and by a multiplication, changing the terordinal into

$$\mathrm{X}x^3y'''+3a_1x^2y''+3a_2xy'+a_3y=0.$$

4. Take the criticoid, viz. deprive the equation of its second term by assuming a new dependent variable $z, =yx^n\mathrm{X}^{\left(\frac{e-a}{n}\right)}$, and divide by the leading coefficient. We thus get

$$z'''+3sz'+tz'=0,$$

wherein s and t are defined by

$$\mathrm{X}^2x^2s=\mathrm{L}+\mathrm{M}x^n+\mathrm{N}x^{2n},$$
$$\mathrm{X}^3x^3t=\mathrm{P}+\mathrm{Q}x^n+\mathrm{R}x^{2n}+\mathrm{S}x^{3n}.$$

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[*] Communicated by the Author.

5. The form of the quadricriticoid s does not vary with the order of the equation. Consequently the formulæ which I gave in this Journal for February 1875* enable us to write down at once

$$\begin{aligned}&L=f-a^2+a,\\&M=f+g-2ae+(n+1)a-(n-1)e,\\&N=g-e^2+e;\end{aligned}$$

and the interchanges (a, e), (f, g), and $(n, -n)$ reverse the order L, M, N.

6. The last coefficient t, which I call the cubicriticoid (Phil. Mag. for March 1870), is

$$\frac{a_3}{x^3X}-3\frac{a_1a_2}{x^2X^2}+2\left(\frac{a_1}{xX}\right)^3-\left(\frac{a_1}{xX}\right)'',$$

where, in differentiating, it should be noticed that

$$\frac{a_1}{xX}=\frac{e}{x}+\frac{a-e}{xX}.$$

7. We obtain on development,

$$\begin{aligned}&P=h-3af+2a^3-2a,\\&Q=2h+k-3(ag+ef+af)+6a^2e\\&\qquad\qquad+(n+1)(n-4)a-(n-1)(n-2)e,\\&R=h+2k-3(ag+ef+eg)+6ae^2\\&\qquad\qquad-(n+1)(n+2)a+(n-1)(n+4)e,\\&S=k-3eg+2e^3-2e,\end{aligned}$$

where the interchanges (a, e), (f, g), (h, k), and $(n, -n)$ reverse the order P, Q, R, S.

8. Hence, writing

$$\begin{aligned}a-e&=\eta,\\L-M+N&=b,\\P-Q+R-S&=2B,\\3(P+S)-(Q+R)&=6C,\end{aligned}$$

we find the following relations:—

$$\begin{aligned}&b=-\eta(\eta+n),\\&B=(\eta-n)\eta(\eta+n)=-(\eta-n)b,\\&C=b+(N-L)\eta.\end{aligned}$$

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* This paper of February 1875 has a sequel in the Number for May 1880.

9. This last is obtained from

$$C=(a+e)^3-4ae(a+e)+(g-f+n)\eta$$
$$=(a^2-e^2+g-f+n)\eta$$

by eliminating $g-f$ and introducing b.

10. These relations give rise to the system

$$B=\tfrac{1}{2}(3n+\sqrt{n^2-4b})b, \quad \ldots\ldots \quad (1)$$
$$C+b=-\tfrac{1}{2}(N-L)(n+\sqrt{n^2-4b}), \quad \ldots \quad (2)$$
$$\eta=-\tfrac{1}{2}(n+\sqrt{n^2-4b}), \quad \ldots\ldots \quad (3)$$

wherein the radical is to be taken with the same sign throughout.

11. Thus far the process is direct. Now invert the problem and suppose that we have given us the equation in z, and that we are asked whether it can be made to take a binomial form. The answer is this. When (1) and (2) are identically satisfied it can be made to take such a form. And since (3) only determines $a-e$, we may give any value we please to one of the quantities a, e, and we shall nevertheless have four disposable quantities, f, g, h, and k, wherewith to satisfy the four outstanding equations which, apart from (1), (2), and (3), have still to be satisfied.

12. Proceeding thus we shall be led to a symbolical terordinal which I write

$$f(D)z+x^nF(D)z=0,$$

wherein

$$f(D)=[D]^3+3a[D]^2+3fD+h,$$
$$F(D)=[D]^3+3e[D]^2+3gD+k,$$

and F(D) is obtained from f(D) by the change of a, f, h, into e, g, k respectively.

13. Now let

$$h=3(a-1)f-a(a-1)(2a-1), \quad \ldots \quad (\text{iv})$$
$$k=3(e-1)g-e(e-1)(2e-1); \quad \ldots \quad (\text{v})$$

then

$$f(D)=(D+a-1)\{D^2+2(a-1)D+3f-a(2a-1)\},$$
$$F(D)=(D+e-1)\{D^2+2(e-1)D+3g-e(2e-1)\},$$

and the factors of both f(D) and F(D) are in arithmetical progression; and the transfer of x^n from the left to the right does not affect this property.

14. But (iv) and (v) are respectively equivalent to

$$P+3L=0, \quad \ldots\ldots\ldots \quad (4)$$
$$S+3N=0. \quad \ldots\ldots\ldots \quad (5)$$

Consequently when (1), (2), (4), and (5) are satisfied identically, and η is determined from (3), and f, g, h, and k from the outstanding equations, the factors are in arithmetical progression.

15. Let $n=3$, then

$$X^3x^3s'=-2L+(M-8L)x^3+(4N-5M)x^6-2Nx^9;$$

and if the terordinal in z is of the form

$$z'''+3sz'+\tfrac{3}{2}s'z=0,$$

the conditions (4) and (5) are satisfied identically. Moreover $2B=9b$ identically; and if $b=\frac{9}{4}$, (1) is satisfied identically, as is (2). But this last terordinal can, if $4b=9$ be linked with

$$z''+\tfrac{3}{4}sz=0,$$

a biordinal (compare Proc. L. M. S. vol. xiii. p. 65, art. 41, p. 67, art. 47, p. 68, art. 52, and p. 70, art. 61).

16. Next, if we start from

$$y''+3sy=0,$$

we can derive from it

$$y'''+3\lambda y''+3sy'+(3s'+9\lambda s)y=0,$$

whence, taking the criticoid,

$$z'''+3(s-\lambda^2-\lambda')z'+(3s'+6\lambda s+2\lambda^3-\lambda'')z=0.$$

17. Write this in the form

$$z'''+3s_2z'+t_2z=0,$$

and define λ by

$$3Xx\lambda=A+Ex^3;$$

we get

$$X^2x^2s_2=L_2+M_2x^3+N_2x^6,$$
$$X^3x^3t_2=P_2+Q_2x^3+R_2x^6+S_2x^9;$$

and we now deal with the suffixed quantities ($s_2, L_2, ..$) as we did with those unsuffixed ($s, L, ..$), but ultimately expressing the suffixed in terms of the unsuffixed quantities. Thus we take the system ($n=3$),

$$B_2=\tfrac{1}{2}(9+\sqrt{9-4b_2})b_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)_2$$

$$C_2+b_2=-\tfrac{1}{2}(N_2-L_2)(3+\sqrt{9-4b_2}), \quad . \quad . \quad (2)_2$$

$$\eta_2=-\tfrac{1}{2}(3+\sqrt{9-4b_2}). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)_2$$

18. Developing, we find

$$L_2=L-A^2+A,$$
$$M_2=M-2AE+4A-2E,$$
$$N_2=N-E^2+E;$$

and, introducing for convenience two new quantities defined by

$$A+E,\quad A-E=\Omega,\quad \omega,$$

we get

$$b_2=b-\omega(\omega+3).$$

19. Again, we have

$$\begin{aligned}
P_2&=6(A-1)L+2A^3-2A,\\
Q_2&=6(A^2E+AM+EL)+3M-24L-4A-2E,\\
R_2&=6(AE^2+AN+EM)-15M+12N-20A+14E,\\
S_2&=6(E-1)N+2E^3-2E.
\end{aligned}$$

20. Hence we find

$$\begin{aligned}
B_2&=(\omega+3)\{\omega(\omega-3)+3b\},\\
C_2&=b(\Omega-2)-(2\omega+3)(N-L)+\omega(\omega\Omega+3)
\end{aligned}$$

and, consequently,

$$C_2+b_2=(b+\omega^2)(\Omega-1)-(2\omega+3)(N-L).$$

21. The equations corresponding to (4) and (5) are

$$(2A-1)\{A(A-1)+3L\}=0, \quad . \quad . \quad . \quad (4)_2$$
$$(2E-1)\{E(E-1)+3N\}=0, \quad . \quad . \quad . \quad (5)_2$$

which give rise to four sets of relations.

22. First, take the set

$$A(A-1)+3L=0,\quad E(E-1)+3N=0;$$

we deduce from it

$$3(N-L)=\omega(\Omega-1), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$
$$3(N+L)=-\tfrac{1}{2}(\Omega^2+\omega^2)+\Omega; \quad . \quad . \quad . \quad . \quad (7)$$

and the penultimate combined with the last equation of art. 20 gives

$$C_2+b_2=\left(3\frac{b}{\omega}+\omega-3\right)(N-L).$$

23. Again,

$$N_2-L_2=N-L+\omega(\Omega-1)=4(N-L);$$

so that the equation $(2)_2$ becomes, after reduction and substitution,

$$\omega(\omega+3)+3b+2\omega\sqrt{9-4b+4\omega(\omega+3)}=0.$$

24. Write $(1)_2$ in the form

$$B_2-\tfrac{9}{2}b_2=\tfrac{1}{2}b_2\sqrt{9-4b_2},$$

which reduces to

$$(2\omega+3)\{\omega(\omega+3)+3b\}=b_2\sqrt{9-4b_2};$$

then, since

$$(2\omega+3)^2=4\omega(\omega+3)+9,$$

if we introduce an auxiliary W, such that

$$W=\omega(\omega+3),$$

the above equivalent for $(1)_2$ can be put under the form

$$(W+3b)\sqrt{4W+9}=(b-W)\sqrt{9-4b+4W};$$

while the last equation of art. 23 becomes

$$W+3b=-2\omega\sqrt{9-4b+4W},$$

if we transpose. Hence, by division,

$$\sqrt{4W+9}(=2\omega+3)=-\frac{b-W}{2\omega};$$

or, substituting for W and reducing,

$$3\omega(\omega+1)=-b,$$

whence

$$\omega=\tfrac{1}{2}(-1+\sqrt{1-\tfrac{4}{3}b}).$$

25. But, squaring the equivalent for $(1)_2$,

$$(W+3b)^2(4W+9)=(b-W)^2(9-4b+4W),$$

whence

$$(3W+b+3)^2=9-12b,$$

and

$$W=\omega(\omega+3)=-1-\frac{b}{3}+\sqrt{1-\tfrac{4}{3}b}.$$

26. Solving this quadratic in ω,

$$\omega=\tfrac{1}{2}(-1+\sqrt{1-\tfrac{4}{3}b}),$$

or

$$\omega=-\tfrac{1}{2}(5+\sqrt{1-\tfrac{4}{3}b});$$

of which solutions the latter is irrelevant, because it does not solve the equivalent of $(2)_2$. But the former satisfies both $(1)_2$ and $(2)_2$.

27. Recurring to art. 22, and writing (7) in the form

$$6(N+L)=1-(\Omega-1)^2-\omega^2,$$

and eliminating $\Omega-1$ between (6) and (7), we find

$$1-6(N+L)=\left(\frac{3}{\omega}\right)^2(N-L)^2+\omega^2; \quad . \quad . \quad (8)$$

whence

$$\sqrt{1-12\mathrm{L}}=\frac{3}{\omega}(\mathrm{N}-\mathrm{L})+\varpi,$$

$$\sqrt{1-12\mathrm{N}}=\frac{3}{\omega}(\mathrm{N}-\mathrm{L})-\omega;$$

and we can give (8) the form

$$\sqrt{1-12\mathrm{L}}-\sqrt{1-12\mathrm{N}}=2\omega=-1+\sqrt{1-\tfrac{4}{3}b}.$$

28. We need not rationalize this equation, which indeed is, practically, rational. For, if we take the criticoid of the biordinal

$$(\mathrm{D}-\beta_1)(\mathrm{D}-\beta_2)u+(\mathrm{D}-\alpha_1)(\mathrm{D}-\alpha_2)x^2u=0,$$

we get a result of the form

$$(1+x^2)^2x^2v''+(l+mx^2+nx^4)v=0;$$

where, taking

$$\alpha_1-\alpha_2,\quad \alpha_1+\alpha_2-\beta_1-\beta_2,\quad \beta_1-\beta_2=\mathrm{J},\ \mathrm{U},\ \mathrm{I},$$

we have

$$4l=1-\mathrm{I}^2,\quad 4m=(\mathrm{U}-2)^2-(2+\mathrm{I}^2+\mathrm{J}^2),\quad 4n=1-\mathrm{J}^2.$$

29. In the last biordinal change the independent variable from x^2 to t^3; then write x in place of t and take the criticoid. We get a result of the form

$$(1+x^3)x^2y''+(\lambda+\mu x^3+\nu x^6)y=0;$$

and this, compared with the biordinal of art. 16, gives

$$\lambda=3\mathrm{L},\quad \mu=3\mathrm{M},\quad \nu=3\mathrm{N}.$$

30. But

$$\lambda=\tfrac{9}{4}l-\tfrac{5}{16},\quad \mu=\tfrac{9}{4}m-\tfrac{5}{8},\quad \nu=\tfrac{9}{4}n-\tfrac{5}{16},$$

relations which yield

$$\lambda-\mu+\nu=\tfrac{9}{4}(l-m+n),\quad \lambda-\nu=\tfrac{9}{4}(l-n).$$

31. Consequently

$$12\mathrm{L}=4\lambda=9l-\tfrac{5}{4}=1-\tfrac{9}{4}\mathrm{I}^2;$$

so

$$12\mathrm{N}=1-\tfrac{9}{4}\mathrm{J}^2,$$

and

$$b=\mathrm{L}-\mathrm{M}+\mathrm{N}=\tfrac{1}{3}(\lambda-\mu+\nu)=\tfrac{3}{4}(l-m+n).$$

32. Hence

$$1-12\mathrm{L}=\tfrac{9}{4}\mathrm{I}^2,\quad 1-12\mathrm{N}=\tfrac{9}{4}\mathrm{J}^2,$$

and

$$1-\tfrac{4}{3}b=1-(l-m+n)=(\mathrm{U}-2)^2\div 4,$$

33. It follows that the relation of art. 27 may be represented by some case of

$$\pm 3(\mathrm{I} \pm \mathrm{J}) \pm (\mathrm{U}-2)+2=0;$$

and when this single condition is fulfilled, the biordinal can be linked with a terordinal whereof the factors in both numerator and denominator are in arithmetical progression. The values of the arbitrary or indeterminate quantities A and E will, of course, have to be properly assigned.

34. Returning to art. 21, we get a second and a third set of relations, viz.

$$\mathrm{A}=\tfrac{1}{2}+\sqrt{\tfrac{1}{4}-\mathrm{L}}, \quad \mathrm{E}=\tfrac{1}{2},$$

and

$$\mathrm{A}=\tfrac{1}{2}, \quad \mathrm{E}=\tfrac{1}{2}+\sqrt{\tfrac{1}{4}-\mathrm{N}};$$

and in either case we have

$$(\Omega-1)^2=\omega^2.$$

35. Taking the second set ($\mathrm{E}=\frac{1}{2}$), we see that $\Omega-1=\omega$, and that $(5)_2$ leaves N free. But it would seem that the four conditions cannot be fulfilled unless L, M, N be connected by at least two relations. This last remark applies also to the third set ($\mathrm{A}=\frac{1}{2}$), wherein $\Omega-1=-\omega$, and $(4)_2$ leaves L free.

36. In the fourth set ($\mathrm{A}, \mathrm{E}=\frac{1}{2}$) we have both L and N left free and $\Omega-1$, $\omega=0$. And if we take the radicals positively, $(1)_2$ and $(2)_2$ are each satisfied by $b=0$ or by $\mathrm{U}=-2$. Thus, bearing in mind Boole's transformations, we may say that when U is an even integer, the biordinal can be linked with a terordinal of the form described in art. 33. This last result is confirmed by, and confirms, another process by which I have arrived at it, and (compare Proc. L. M. S. vol. xiii. pp. 67–68), combined with what is otherwise known, leads to the conclusion that when, of the three expressions I, U, J, one is $4(i \pm \frac{1}{3})$, i being an integer, and the remaining two are unevenly even integers, then the biordinal is finitely soluble.

2 Sandringham Gardens, Ealing,
July 25, 1881.

XXIV. *Note on the Index of Refraction of Ebonite.* *By* W. E. Ayrton *and* John Perry[*].

In a note communicated to the Royal Society (printed in 'Nature,' No. 596, vol. xxiii. March 31, 1881), we described how, by using a selenium cell, lent us by Mr. Bidwell, and a

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[*] Communicated by the Physical Society, having been read at the Meeting on June 25, 1881.

pair of delicate Bell's telephones, we had succeeded in showing, 1st, that there was refraction when intermittent radiation from the oxyhydrogen light passed through an ebonite prism; and, 2ndly, that the index of refraction of that ebonite was approximately 1·7.

Exceedingly great care had to be taken, in consequence of the feebleness of the sounds given out by the telephones; and, from the nature of the experiment, we obtained the index of refraction for that narrow band of rays which experienced least absorption.

Shortly after these results were published, Prof. Fitzgerald, of Dublin, suggested, in conversation, the possibility of checking them by measuring the polarizing angle of light reflected from ebonite, on the assumption that the refracted ray is at right angles to the reflected one when giving maximum polarization. Subsequently Dr. Jellett was so kind as to make these experiments, the results of which Prof. Fitzgerald permits us to quote. The mean index of refraction for ebonite thus obtained, on Fresnel's theory, was 1·611.

Later on we repeated our selenium experiments, replacing the intermittent oxyhydrogen light with an intermittent electric light, and increasing the electromotive force in the selenium telephone-circuit to about 60 volts. A confirmation of our former result was obtained; but, although we were able to take greater precautions to ensure accuracy, we obtained no more than a confirmation; and from the difficulty of hearing the weak sounds in the telephones, we felt that the index of refraction thus measured might be as much as 1·8 or as small as 1·6.

In the course of these experiments, however, it was noticed that visible red rays were certainly refracted; and consequently we proceeded to make measurements according to ordinary optical methods, using the apparatus shown in the figure. L (fig. 1) is a fairly powerful electric light produced by a Gramme machine; C is a glass lens giving a parallel beam of light, part of which passes through the slit, S, $\frac{1}{20}$ inch wide, and falls on the edge of the ebonite prism P. F is a frame holding tissue-paper, which can be moved about P as centre, and which carries an index, I, pointing to the graduations on the circle, as seen in fig. 2. There was a fine vertical line in the middle of the tissue-paper; and HG, forming about one third of the paper, was well blackened. First this screen was moved into such a position that the edge of the prism threw a black shadow which was bounded by the fine central line, and between that line and the blackened portion H G was a thin band of white light. In fact a narrow beam from the slit fell

Fig. 1.

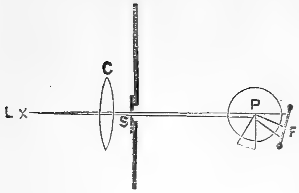
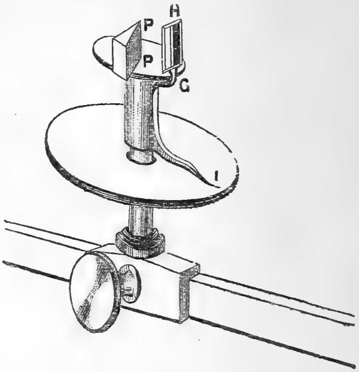


Fig. 2.



on the edge of the prism; and half was stopped by the prism, the other half going on. The index-reading in this position was taken; and now the screen was moved round until a red spectrum was seen. At the least-refrangible end this spectrum terminated nearly abruptly, as the ordinary visible spectrum usually does; and this end was made to coincide with the central line in the screen, and the index-reading taken when, after moving the prism itself, it was supposed that we had minimum deviation. The index-reading was also taken in the same way for the most-refrangible end of the visible spectrum; but as this did not die away at all abruptly, and as the whole spectrum was very faint, the second set of measurements merely gives a rough idea of the amount of spectrum that was visible. The mean of a number of observations made by different observers, and the results of which were closely in accord, gave 1·66 as the index of refraction for the well-defined least-refrangible end, and 1·9 as the average result for the badly-defined most-refrangible end.

As the slit in the metal diaphragm used with the selenium experiments had to be much wider than that employed with the simple light-experiments, we cannot of course tell what exact part of the spectrum produced the sound: probably it was at about the least-refrangible end of the visible spectrum; but it may have been the dark rays just beyond.

Summing up the results of the various experiments, we have for the index of refraction of certain specimens of ebonite:—

Ebonite prism, selenium, and telephones . .	about	1·7
Measurement of polarizing angle by reflection	„	1·611
Least refracted end of visible spectrum produced by ebonite lens having an angle of 28°·5 .	about	1·66

In a paper by Captain Abney and Colonel Festing, recently read before the Physical Society and printed in the Philosophical Magazine for June, on the Transmission of Radiation through Ebonite, reference is made to our original experiments; and the authors say that, judging from the figure accompanying our Note, they should think that the thickness of the ebonite prism traversed by the intermittent beam must have been about one fourth of an inch. We are afraid that that figure is liable to give this misconception; in drawing it we were merely paying attention to the directions of the incident and refracted beam, and not to the actual thickness of the ebonite, which was in fact very small indeed where the intermittent beam passed through it.

XXV. *On Instruments for Measuring and Recording Earthquake-Motions.* *By* Thomas Gray, *B.Sc., F.R.S.E.*[*]

[Plate III.]

I. *Rolling-Sphere Seismograph.*

THE instrument which I have called a rolling-sphere seismograph will be readily understood from the accompanying sectional drawing (fig. 1). A sphere of lead, iron, or any other heavy substance rests on a flat plate, B, made truly plane and furnished with three levelling-screws, L. An arm, A, fixed to the base, B, is so formed that a circular ring fixed to its end is held in a horizontal position with its centre vertically above the highest point of the sphere. This ring carries a species of spring universal joint, consisting of four very light bent springs, *j*, arranged at right angles to one another and meeting in a small round disk, *b*, at the centre. The lower end of the lever, *l*, passes through this ring *b*, and is fixed to it at such a point that its lower end, which is rounded, just fits a small hole in the top of the sphere S. Between S and *b* a small sphere, *s*, is fixed to the lever *l*, and is so proportioned that the lever *l*, when pushed at *b*, tends to rotate around a point a little above its lower end, thus diminishing the push on the sphere S. The springs *j* serve to allow the lever *l* to turn in any direction, and are made so light that they can only make the ball roll with a very long period. When thus proportioned they serve the purpose of a universal joint, and at the same time give a little stability to the parts, thus preventing the plate P, if it be put in motion, from causing the ball to roll over. The lever *l* is a rod of bamboo which is at the same time

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[*] Communicated by the Author.

very light and rigid; at the upper end the rod is flattened and hinged just above the bend by a piece of tough Japanese paper glued to its upper side. This gives a very light, flexible, and sufficiently strong hinge. The plate P may be in a plane which, by proper starting-apparatus, will be set in motion at the beginning of an earthquake, or it may be taken as part of a roller or of a circular plate kept continuously in motion by proper mechanism.

This instrument possesses the advantage of being compact, of writing the actual motion, and of being capable of recording with approximate accuracy earthquakes of much greater magnitude than can be recorded by most of the instruments in ordinary use.

The multiplication given by an instrument of the sort may be determined experimentally; or it may be approximately calculated by taking a point O at a height equal to seven fifths of the radius of the large sphere S as nearly steady. In a locality where the motion of the ground is considerable, the lever l must of course be made to give little, or perhaps no multiplication.

In several instruments of this class which have been constructed for the observation of earthquakes in Japan, a segment only of the sphere S has been used, with an independent mass placed on a pivot at the centre of oscillation (arranged in this case to be above the centre of the sphere), and of such magnitude as to make the equilibrium nearly neutral. This method of construction allows the radius of the sphere to be much increased; but it introduces a difficulty of adjustment, a complication of parts, and generally a slight want of symmetry, which causes a little uncertainty in the interpretation of the records.

The static records* given by this machine are very interesting. These are simply enlarged representations of the motion of the earth; and are in many cases very curious, having generally a resemblance to the curves obtained when two harmonic motions, not in the same direction, are combined.

II. *Rolling-Cylinder Seismograph.*

In this instrument (fig. 2) a pair of hollow cylinders, C, C′, made perfectly equal in thickness and in length by turning in a lathe, are placed, with their axes horizontal and mutually at right angles, on a smooth plane plate, P, furnished with levelling-screws, S. An arm, A, fixed to the plane, and therefore forced to move with it, passes over the top of one of the cylinders,

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* That is, the records taken with the plate on which the motions are recorded at rest relatively to the earth.

and carries two recording-levers, l, l'. The lever l is pivoted at p and p'; and its action can be readily understood from the figure. The lever l' is crank-shaped, and is hinged to a cross piece b, fixed to the front of the cylinder C′ and to the arm A at $p_{\prime}$. This cross piece rotates as the cylinder rotates; and by properly adjusting its length and the ratio of the arms of the lever l, the multiplication can be made the same as for l. A drum carrying smoked paper (shown at D), or a circular glass plate, may be revolved in front of the plate, and hence a record of the different movements of the earth obtained.

In this instrument the principle of neutral equilibrium can be carried out to great perfection, as cylinders can be made with great accuracy. It seems probable, from the preliminary trials that have been made, that this arrangement may be improved by allowing a smaller cylinder to roll freely inside the large one. The advantage of this is the stability it gives, and the quickness with which the cylinder comes to rest in its new position after any motion.

An interesting modification of this machine might be made by placing two equal cylinders on a horizontal plate with their axes parallel, and placing on them a second horizontal plate so that its upper surface should always be in a plane through their instantaneous axes. This could be done by causing the plate to rest by means of arms on two pairs of smaller cylinders of proper dimensions, so disposed that each of the larger cylinders should bear at each end one of the smaller cylinders coaxial with the larger cylinder and projecting from it. A third cylinder placed on the upper plate with its axis at right angles to those of the first two would, for small motions of the earth, have a line in itself which would remain approximately at rest. Hence by proper registering-apparatus, perhaps similar to that adopted for my rolling-sphere seismograph, a record of the motion of the earth might be obtained.

The instrument is shown in the diagram as arranged for the registration of small motions; but it is easy to modify this arrangement so as to provide for the registration of motions ranging in amplitude from a fraction of a millimetre to several feet. All that is necessary is to arrange that, for very large motions, the multiplying-lever shall go out of action, allowing a direct-record arrangement to take its place. A very simple method of writing large motions would be to attach a fine point to the end of the cylinder at its centre, and allow this point to write on a plate placed in front of it and fixed to the base-plate P. For countries where the earthquake-motion is measured in centimetres instead of in millimetres, as is the case in Japan, an instrument of this form might prove of great value, while by the arrangement shown in the figure it can be made to write any motion of sufficient magnitude to be appreciated.

The point p' is, of course, placed in the instantaneous axis, of which the position is easily found in the case of this instrument. Let h be the height of the instantaneous axis above the plate P, R the external radius, and r the internal radius of the cylinder, then

$$h=\frac{3R^2+r^2}{2R},$$

from which it is easy to see that, in the case of a thin cylinder, the instantaneous axis is nearly on the inner surface of the cylinder. In the case of the instrument actually constructed, $R=8$ and $r=7$, and therefore

$$h=\frac{192+49}{16}=15{\cdot}06 \text{ nearly.}$$

III. *Pendulum Seismograph.*

About a year ago (Jan. 1880) I began to make experiments on horizontal levers pivoted at one end and loaded at the other, with the view of obtaining a body in approximately neutral equilibrium, which by its inertia would give a means of recording the motions of the earth during an earthquake. The levers which I used were about 30 centimetres long between the load and the pivots; and the load was generally about 5 kilogrammes. This mass remained steady even when an oscillatory motion of two or three centimetres was given to the pivoted end of the lever, if that motion was at right angles to the length of the lever. When the motion was oblique, however, there was a tendency for the mass to change its position, probably due to the direct impulses given to it not quite neutralizing each other. This change of position is not of vital importance if the successive motions be recorded on a moving plate; but as I was not at that time inclined to use clockwork in connexion with the machine, and it formed a great objection when a static record was used, I did not go further in the matter. The experimental machine, however, proved an excellent instrument for class-room illustrations of inertia; and as such I have since used it in my lectures on that subject. It is, of course, easy to give an arrangement of this kind sufficient stability to prevent permanent displacement; but when so arranged it has little, if any, advantage over an ordinary pendulum, and has some marked disadvantages.

I came to the conclusion at that time that the ordinary vertical pendulum is the most convenient, and probably the most accurate, instrument for static records of earthquakes; and a machine, based on the pendulum principle, was described by me before the Seismological Society of Japan in March 1880; and a description with drawing is given in the 'Proceedings.' A description of that machine, and of some improvements I have since made in it, forms the subject of this part of my paper.

The machine consisted of an ordinary simple pendulum about 3 feet in length, the bob of which was of considerable mass. From the centre of inertia of the bob three threads radiated, and were attached to three light pulleys arranged at equal distances apart on the circumference of a horizontal circle having its centre at the centre of the bob. To these pulleys very light indices were attached, the points of which turned above graduated arcs, and showed the motion of the earth magnified twenty-five times. The three components were taken for the purpose of showing without ambiguity the direction of the motion, if it had a definite direction, and of giving information as to whether there was a multiplicity of directions of motion. This machine was of course only capable, under the most favourable circumstances, of giving the amplitude of the greatest motion, this being indicated by the permanent displacement of the pulleys and indices.

The improvements on this machine, to which I now call attention, are, first, a method of rendering the pendulum dead-beat, and, second, a modification of the mode of fixing the pointers to the pulleys.

A well-known objection to pendulum machines is their tendency to acquire a swinging motion during the earthquake, this of course causing their indications of extent of motion to be untrustworthy*. I have found, after a considerable number of trials, that this objection can be almost wholly overcome by a very simple process. Let a rod be adjusted in such a way that it can slide freely in a vertical direction through holes in two plates, one above and the other below the centre of the bob, but as near to its centre as possible. Load this rod until, with its sharp point touching a glass plate, it offers sufficient friction to bring the pendulum to rest after one half-swing, when the initial displacement is about that of the largest earthquake likely to occur. A load of 30 grammes will be found sufficient in Japan for a 3-foot pendulum; the bob of which weighs 10 kilogrammes. With this simple addition to the pendulum its swinging will be almost wholly avoided, and

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* See Professors Ayrton and Perry's paper, "On a Neglected Principle in Earthquake Measurements," Phil. Mag. Aug. 1879.

the amplitude-indications rendered nearly absolute. It may be objected that this friction will cause the amplitude for each individual motion to be shown too small. But I find on trial that the permanent displacement which this is capable of producing is almost inappreciable, and that the displacement due to a motion of the glass plate backwards and forwards at a period nearly corresponding with the period of the pendulum is not very great, even if two or three times repeated.

The improvement in the mode of attaching the pointers was called for by the fact that, even with the lightest pointers which could be conveniently made, the rotational energy imparted to them by the shock made them swing too far round. This I got over by hanging the pointers by a bifilar suspension from the lower side of the pulleys. When the pulleys and pointers are so arranged, the pull on the pendulum can be made almost infinitesimal; and yet, since the indices are not forced to move with the pulley, but rather tend to check its motion, the indications are very nearly accurate.

I may remark that the indications of these machines, a considerable number of which have been in use for some time, tend to show that there is no definite direction of movement, or, in other words, that the successive impulses contain vibrations in different azimuths. No doubt much of this is due to the existence of direct and transverse vibrations, such as we may expect from the theory of vibrations in elastic solids.

Besides exercising a controlling power on the pendulum, the friction-point can be made to describe very interesting curves on the glass plate if it be previously smoked. These curves in some of the larger earthquakes have indicated a very complicated motion of the earth.

IV. *Double-Lever Seismograph**.

In the course of my experiments on the horizontal-lever arrangement, I was informed by Prof. Milne, to whom I had shown my experiments, that a very similar arrangement had

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* This paper is for the most part extracted from an account of a seismograph constructed on the principle described, which was communicated to the Seismological Society of Japan at its meeting on January 26, 1881. The only differences between the instrument here described and that figured in the 'Transactions' of the Society, are some variations in size and in details of construction which have been suggested by experience. The instrument there described was made very small, the horizontal levers and frame carrying them being all surrounded by the ring R, which had in that case an internal diameter of about 12 centimetres. It was found inconveniently compact, and was besides difficult to construct accurately. As the instrument now described is almost wholly of wood, its first cost is very small.

been for some time used by Prof. Chaplin at the Tokio University. I have since heard from Prof. Chaplin that he obtained the idea from a Zöllner's pendulum. Prof. Chaplin was no doubt the first in this country, perhaps the first anywhere, who applied this certainly valuable arrangement to the measurement of earthquake-motion. While talking over my experiments, Mr. Milne expressed a strong desire to have a direct record of the earthquake from some such arrangement; but a suitable mode of obtaining it did not then suggest itself. Since that time, however, it has occurred to me that two levers hinged together will not only give this arrangement, but will avoid at the same time one very serious source of disturbance, namely the shock which the mass receives in consequence of the direction of motion not being at right angles to the direction of the lever. Acting on this idea, I made a sketch of the arrangement described in this paper, and showed it to Mr. Milne, who at once offered to give the instrument a trial if I did not care to do so. In consequence of this, one of these instruments has been in course of construction for some time, but unfortunately has not yet (March 23) been finished sufficiently for exhibition to the Society.

The principle and form of this machine will be readily understood by reference to fig. 3. A post, P, resembling in all respects a light gate-post, is fixed firmly in the ground; and to it a species of gate, A, is hinged in a manner almost identical in form with that usually adopted. The hinge, H, is capable of being moved backwards and forwards, its position being regulated by the nut, N, and from side to side, its position in that direction being regulated by two screws, *s*, put in from opposite sides of the post. These adjustments are all that are necessary, after the instrument is properly constructed, to allow neutral equilibrium to be obtained. The lower pivot H rests against the bottom of a conical hole cut in such a way that the pressure will be along the axis of the cone. The upper hinge H is a knife-edge resting against the side of a round hole. The hinges H′, H′ are exactly similar in construction to H, H.

The gate A is 15 centim. long. and 60 centim. high; the central piece is a round bamboo tube of about 4 centim. diameter, and the cross pieces hard wood firmly fixed to the central piece. By adopting a symmetrical form like the above, the proper positions of the hinges H, H′ are readily calculated, while the tubular form of the vertical piece gives great torsional rigidity with small weight. The front gate, B, is similar in form but in every way lighter, as it does not require to withstand a twisting force such as acts on A. The lower half of B is slightly different, a light bracket being introduced for the purpose of raising the ring R, and allowing room for the registering-lever. The ring R is pivoted so that it can turn round a vertical axis, thus rendering more definite the point which remains steady. A stiff bar, b, fixed to the post P at one end, carries the universal joint in which the writing-lever, l, turns. This universal joint consists of four very light bent springs, fixed at the lower end to a round ring, and at the upper end to a small disk through which the lever l passes, and to which it is soldered. The four springs are arranged at right angles to each other, and offer a slight elastic resistance to the rotation of the lever l, and consequently of B and A. This resistance is made so small that the period of oscillation of B is very long. The reason for using this peculiar form of joint is to prevent a gradual displacement of R by the top of l when the plate on which l writes is kept moving by clockwork. The static friction, even with the precaution taken in this machine, will probably be much greater than the pressure of l; but a slight motion might take place during the shaking. The ideas of calculating the positions of H and H′ and of pivoting the ring R which are used in this paper, are taken from Prof. Ewing's paper "On an Astatic Horizontal-Lever Seismograph," communicated at last meeting to this Society, or rather from a conversation which I had with him previous to the reading of that paper. I have generally been in the habit of finding approximately the steady part by experiment; and in many cases this is almost the only satisfactory method*.

In using this instrument, the two gates A and B are adjusted so that their planes are accurately at right angles to each other, each inclined at an angle of 45° to the front of the post P. The gates are so made that when in this position the lever l is vertical. At the lower end of the lever l a thin sewing-needle, n, slides up and down through two small loops. Under this needle the plate p, smoked to receive the record, is placed. If it is desired to separate the different motions, p must be moved by clockwork.

V. *On a Conical-Pendulum Seismograph.*

In a paper on "Steady Points for Seismographs," communicated to the Seismological Society of Japan in March 1881,

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* In the small instrument which I described before the Seismological Society, the levers were so light and small compared with the ring R that no determination of the exact proper positions of the axes were made. That instrument promises to act very fairly as a seismograph; but I expect the instrument just described to act better, because of smaller friction, due to great height, finer and more scientifically arranged hinges, and greater length of arm.

I suggested that a conical pendulum would probably be found a good arrangement for registering one component of the motion of the earth during an earthquake. Since that time an instrument on this principle has been made; and as it has been found to be very suitable for this purpose, a short description of it may be interesting.

The instrument is shown in plan and elevation in fig. 4. In that drawing P is a post which is intended to be fixed firmly in the ground, and may project above it from a foot to a foot and a half. W, W are weights fixed to the ends of the cross bars *b*, each pair forming the bob of a conical pendulum. The suspending wires, *t*, *t*, of these pendulums are attached to cross pieces, *c*, *c*, slotted and made to slide under clamping-screws, *s*, *s*. By sliding the cross pieces *c*, *c* backwards and forwards and turning them round to one side or other, the point of suspension can be adjusted until it is vertically above the point *p* at which the small cross arm *a* rests. This cross arm *a* serves the double purpose of causing the bob of the pendulum to move in a circle, if it moves at all, and of forming the short arm of the lever *l*, which is used for writing a component of the motion of the earth. When the pendulum is adjusted in the way just described the bob is evidently in neutral equilibrium in any position; and hence any error due to oscillatory motion is avoided. Since, however, it is desirable that the instrument should have a small amount of stability, it may be made to have a long period of oscillation round a definite point by adjusting the point of suspension a little forward, or by fixing the top of the suspending wire and allowing its torsional rigidity to control the motion of the bob.

The only points where friction acts in this machine are *p*, *p′*; and since the pressure on these points is small, they can be made very fine and almost frictionless. In this respect the conical-pendulum machine has a decided advantage over most other machines of a similar nature, such as hinged-horizontal-lever machines. The levers *l*, *l* are arranged so that they come out at an angle of 45° to the direction of the component they are intended to measure, and parallel to each other. This arrangement is adopted for the purpose of getting the record of the two rectangular components side by side. When the record is made in this way, the direction of motion of the earth can be much more easily determined than if they were recorded at different points of the same drum or revolving plate. The question of change of phase-relation between the two components can also be much more easily investigated with this arrangement. Another advantage which this apparatus possesses, is the very small mass required for the moving parts, and the very great mass which can be put into the weights W without increasing the friction, the only change required being that the wire *t* should be lengthened. The question of massiveness in the portion whose inertia gives the writing-power is important, as the multiplication which can be used is practically proportional to this mass. Very large multiplication is not generally necessary for earthquakes of ordinary magnitude; but there is a large class of tremors which have a great interest to investigators; and these may be recorded by giving a multiplication of, say, from twenty to fifty, according to the degree of minuteness aimed at. Of a considerable number of machines which I have devised and tried, I think this is decidedly the most sensitive; and it is probably the most sensitive and at the same time accurate recording-instrument now in use.

It is of course to be understood that, in connexion with this seismograph, some form of clockwork arrangement must be used for the purpose of supplying a moving surface on which the different motions of the pointers are to be recorded. When such an arrangement is adopted, the horizontal amplitude and direction and the period of each individual motion are recorded, the period of course being reckoned from the rate at which the drum is moved by the clock. The records are most convenient for use when taken on smoked glass or transparent smooth paper, because in that case, by simply varnishing the plate or paper sheet, the record can be preserved and used as a negative for obtaining photographs.

VI. *Hydrometer Vertical-Motion Seismograph.*

Some time ago I made several attempts to obtain an instrument capable of recording the vertical movements in an earthquake shock. I propose in the present paper to describe two of the most successful of these attempts.

My first attempt in this direction was suggested to me by observing the motion of a Nicholson's hydrometer when immersed in a liquid and slightly displaced from its equilibrium position. The period of up-and-down oscillation of such an instrument can, by varying the thickness of the stem, be made almost any length desired. I therefore proposed to use an enlarged hydrometer, with the lower basket removed and ballast placed in the bottom of the hollow foot. For the sake of ease of regulation I attach the upper end, by means of a thread, to a pulley through the centre of which, at right angles to its plane, a wire has been passed and then soldered. This wire is stretched between two springs, and can be twisted round its own axis, thus allowing the force of torsion to regulate the position of the float. An instrument of this kind was made for water as the floating medium, and gave results which justify the assumption that, by causing it to write on a moving plate, it is capable of giving valuable information as to the magnitude of the vertical movement. This instrument has, however, the disadvantage of being very large, and consequently inconvenient; and I propose therefore to use mercury instead of water. Fig. 5 indicates a form which may be adopted in such a case.

Referring to the figure, F is a varnished, or otherwise protected, lead float placed in a wood or iron vessel, V, filled with mercury. A fine wire is fixed to the lower end of the float, and, passing loosely through a hole in the plate *p*, serves to prevent the float from turning round. A weight, W, sufficient to completely submerge the float is applied to the stem above the surface of the mercury. To the upper end of the stem a a wire or thread, W, is attached, which is passed through a small hole in the top of the vessel V, and then over the pulley *l*, to which the index, *i*, is attached. With this arrangement the period of vertical oscillation can be made very long; and consequently each individual motion of the earth can be recorded on a moving surface, such as a drum D, relatively to an undulating line due to the slow up-and-down movement of the float. By using the pulley *l*, the sensibility of the instrument is made constant for any position within the range of the motion.

VII. *Compensated Spring Astatic Seismograph for Vertical Motions.*

The following is a description of another instrument which I have contrived for registering vertical motions, and which seems well adapted for that purpose. A description of it, with some account of my experiments with various arrangements leading to the form adopted, was given at the meeting of the Seismological Society of Japan, on the 28th of April of this year. The instrument is shown in fig. 6, which is a section through the frame, and shows in elevation the acting parts of the apparatus. A vertical spring, S, is fixed at its upper end by means of a nut, *n*, which rests on the top of the frame F, and serves to raise or lower the spring through a short distance as a last adjustment for the position of the cross arm A. The arm A rests at one end on two sharp points, *p*, one resting in a conical hole and the other in a V-slot; it is supported at B by the spring S, and is weighted at C with a lead ring, R. Over a pin at the point C a stirrup of thread is placed which supports a small trough, *t*. The trough *t* is pivoted at *a*, has attached to it the index i (which is hinged by means of a strip of tough paper at h, and rests through a fine pin on the glass plate g), and is partly filled with mercury.

The reasons for this mode of suspension are:—First, the arm A, by allowing the spring to be held stretched by means of a lever and weight instead of by a weight directly applied, increases the period of free vertical oscillation of the spring and weight. For let ϕ = angle turned through by the arm A from its normal position at time t, e = consequent elongation of the spring, E = total normal elongation of the spring, l = length of long arm of lever A, l' = length of short arm of A, g = force of gravity in unit mass, T = period of vertical vibration, M the mass of the lead weight R,—then, supposing ϕ small, the lever A very light, and the mass of R collected at its centre of inertia, and neglecting the influence of the trough &c., we have for the equation of motion,

$$\frac{d^2\phi}{dt^2}+\frac{g}{\mathrm{E}}\frac{l'}{l}\phi=0,$$

and therefore

$$\mathrm{T}=2\pi\sqrt{\frac{\mathrm{E}}{g}\frac{l}{l'}}.$$

From this we see that the period increases as the square root of $\frac{l}{l'}$; and hence an advantage in length of period is gained by attaching the string as shown. A disadvantage, of course, is that a smaller mass (in the ratio of l' to l) is required to produce a given normal elongation of the spring than if the weight were applied directly.

Secondly, the mercury in the trough t acts as a compensator to prevent the ring R moving when the top of the spring is moved through a distance short compared with the distance between the pivots on the trough t. The action of this part is as follows:—When the plane carrying the spring S is raised and lowered, the point a rises and falls, but in consequence of the inertia and slow period of R the point C remains behind. In consequence of this the end of the trough t falls and rises relatively to a; and the mercury, running backwards and forwards, puts more or less force on the point C, and hence tends to keep this point stationary.

If the length of the trough be x, the distance between the pivots y, and the width w, then for a rise of a through a distance h we have the centre of gravity of a prism of liquid of depth h moved from the centre of the trough to a point $\frac{x}{3}$ from the end. This gives a displacement equal to $\frac{1}{6}x$, and therefore an increase of weight on $C = \frac{1}{6}\frac{x}{y}hw\delta$, where δ is the density of mercury in this case.

Now the increased elongation of the spring, c remaining stationary, is $\frac{l_1}{l}h$; therefore the increased force is

$$\frac{l_1}{l}h\frac{W}{E},$$

where W is the normal force on C. Hence we have

$$\frac{l_1}{l}\cdot\frac{W}{E} = \frac{1}{6}\frac{x}{y}w\delta$$

for an astatic arrangement. In this equation x, y, w, and δ may vary; but y is generally determined with reference to a convenient multiplication, and δ will generally be either the density of mercury or of a liquid whose density is approximately unity. Hence, x or w being decided on, the other is determinate.

The case in which the arm A is not used can evidently be got from the above by putting $\frac{l_1}{l} = 1$. This arrangement with a long spring is, no doubt, best when the vertical displacement is likely to be great. A round tube has an advantage over the rectangular form of trough t for small motions, as it can be arranged so as to render the system astatic when t is horizontal, but to acquire stability when slightly displaced. For large displacements, however, the rectangular form, when of suitable depth, is no doubt best.

Instruments of the class here described are not to be relied on for static records, owing to the ease with which a large displacement can be given to the lever. For such purposes an instrument with a somewhat short natural period, controlled by some kind of frictional resistance, acts best.

Before the above method of compensation suggested itself to me, I had attempted compensating by placing the axis B below the line joining p and c. This method has the disadvantage, that generally a mass, either that of the weight or the spring, has to receive a motion of translation in order that the point C may remain at the same height. There is also another disadvantage, namely that after a small displacement the equilibrium becomes unstable.

Several other methods have suggested themselves to me—such as allowing the ring R to roll backwards and forwards, on a properly proportioned axis, in a curve at the end of the lever. This would produce compensation by lengthening the arm $p\,c$ when the lever inclined downwards, and shortening it when it inclined upwards. Another method would be to attach one end of an independent string at the point B and the other end to a point p, so that, if the arm A was deflected, a couple would be introduced tending to keep it deflected. This method would have the advantage of ready adjustment, by means of a screw attached to the end of the compensating-spring. I do not consider any of these methods so nearly perfect or so simple as that which I have given prominence to in this paper; but the main object has been to call attention to the principle of compensation for such instruments.

XXVI. *The Microphonic Action of Selenium Cells.*

By Dr. James Moser*.

When I began these researches on the Transformation of the Energy of Light into that of Sound by the Photophone, I held the opinion which is still common, that there are two kinds of photophones and three forms of light-rays. My experiments led me to the conclusion that there is only one way in which light acts photophonically. The effect of radiation on selenium-cells is, in fact, the same as that exerted on the majority of solid, liquid, and gaseous bodies used as non-electric photophonic receivers. Though rays may have different wave-lengths, all rays are the same in kind. There are not three kinds—heating, luminous, and chemical, but one and the same ray may have heating, chemical, and luminous effects.

In February last, when I began these experiments, I believed that the photophone could inform us as to the direct correlation between light and electricity. A current circulating around a beam of polarized light changes the plane of its vibration. Hence we are led to conjecture that there may exist further relations between light and electricity, and that, as the electric current or lines of magnetic force affect the beam, so, inversely, the beam may influence the electric current or the magnetic lines of force; and we may conjecture that such influence may manifest itself in the photophone.

I therefore tried to change the magnetic condition of an iron plate by light. I hoped, for instance, to get an electric current in the coil of the telephone at the moment when

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* Communicated by the Physical Society, having been read June 25th, 1881.

its iron plate is exposed to radiation. I did not succeed in observing such a current.

Prof. W. G. Adams was the first to show that selenium on illumination develops an electromotive force. Still supposing that in the photophone a direct correlation between the energy of light and that of the electric current might reveal itself, I endeavoured to observe and to measure the electromotive force developed in selenium cells on their exposure to light.

I made selenium cells according to the method of Bell and Tainter as modified by Bidwell. Two thin copper wires were wrapped parallel side by side several times round a strip of mica. Selenium was then melted, and crystallized between them. In this way I obtained cells which were very sensitive and yielded, when connected with the battery, a clear sound in the telephone. According to my last measurement, the resistance of one of those cells in the dark is 3000 ohms, diffuse daylight reducing it to 2700. The measurements were made with an astatic-mirror galvanometer, the resistance of the coils of which was 6860 ohms. The scale was about one metre distant from the mirror, each division one fortieth of an inch. There were other galvanometers at my disposal, and a battery of twenty Leclanché cells. The galvanometer proved itself more sensitive than the telephone.

The above-mentioned selenium cell yielded by intermittent radiation of lime- or electric light clear and distinct sounds; illumination by the same sources of light deflected the image of the slit strongly.

But the same light which, when a current of an external battery flows through the cell, makes the telephone to sound or the spot of light to slide along the whole scale, is not able to produce an observable electromotive force in the selenium cell. This non-appearance of a current (the circuit being formed only of selenium cell and galvanometer, or of selenium cell and telephone) is, in my opinion, very noteworthy; for the problem which I wish here to solve is not to make photophones without batteries, but to explain the efficiency of the selenium cells by the qualities they actually possess.

It is possible to prepare pieces of selenium which show an electromotive force on being illuminated. I shall return to this point further on. But cells and pieces of selenium *not* possessing this property nevertheless show photophonic action. Therefore the photophonic action cannot be explained by this property, which is, or at least may be, wanting in the selenium cells.

I proceeded further, always endeavouring to find a correlation of the energies. The current flowing through a selenium cell experiences a counter force—a polarization. I tried to observe, and, as far as possible, to measure it. This polarization is shown by *all* my cells and preparations of selenium. I found it, however, when measured by the compensation method, to be only about the thousandth of a volt. In order to observe the polarization as near as possible to its maximum, I employed a rotating switch, which I constructed by means of two alternating tooth-wheels, and which changed the current eighty times per second.

I next endeavoured to increase the electromotive forces originating in the selenium cell, as well as those produced eventually by light. I tried to attain this by taking the two wires of different material; then I prepared selenium pieces where selenium was simply between two straight parallel wires. At first I made a selenium cell, just like the one described above, replacing, however, the one copper wire by one of platinum, so that the selenium was between copper and platinum. But the polarization, which again could be easily observed, was here also very small. On measuring, it was found again about one thousandth of a volt. Also this cell did not produce, either in the dark or on illumination, any current which could be observed by the galvanometer.

In order to increase the polarization, I gave the polarized selenium cell greater capacity. I took, instead of the wires, two larger plates, each 6 centim. long, 3 centim. wide, and about 1 millim. thick. My intention was to begin the experiments with three of such pairs, with

copper, selenium, copper;
zinc, selenium, zinc;
zinc, selenium, copper.

With the latter of these cells I succeeded (after having sent through it the current of a strong battery) in observing a polarization of about 0·4 volt. The cell had now become a polarization battery, giving rise to a current long after it was separated from the primary battery.

In all these experiments, and especially in the last, a superposition of polarizations could be distinctly observed; so that here we have to deal, not simply with thermo-electric currents, but with electro-chemical decomposition.

An observation, however, which I now made gave a different direction to these researches. But their aim remained unchanged—namely, to explain the efficiency of the selenium cells, to understand the transformation of energy of light into that of sound in the selenium cell, regarding it from a more general point of view and not merely as an isolated phenomenon in selenium, but, by finding the general law, to remove selenium from its isolated position. This, however, may be done either by comparing other bodies with it (which is the method of previous investigations on the subject, and was my own also till now), or this may be effected by inserting selenium in the series of other bodies. The latter method I shall now describe.

I intended to make such cells of copper, selenium, copper, or of zinc, selenium, copper plates, in the same way as the above-described photophonic cells. On a copper plate I melted amorphous selenium, and put on it the second metal plate of zinc or copper. Then I heated gradually, so that the selenium became crystalline; and I then annealed it. Whilst the amorphous selenium adheres very well to the metal plates, these cells proved very brittle when the selenium was crystalline; the selenium always split off from the copper plate. In order to find how to avoid this splitting-off, I examined the matter more closely; thus, I left out the second metal plate, and experimented with copper and selenium only.

The preparation was, in accordance with that of the photophonic cells, the following:—One of the copper plates to be used for the cells was heated on a large brass plate of about 3 millim. thickness; this was covered by a thin sheet of mica, on which the selenium which eventually flowed off was collected. The copper plate having reached the melting-point of amorphous selenium, was covered with this substance and thereupon removed from the brass plate. On being removed it cooled quickly; and on it now a black-brilliant layer of amorphous selenium remained. The larger brass plate having also cooled, the copper plate covered with selenium was again laid on it and again slowly warmed, so that the selenium crystallized. The temperature was still further raised, near to the point at which the crystalline selenium begins to melt. The selenium was then annealed.

But, notwithstanding this caution, it was not possible to fix the selenium on the copper plate. It exfoliated and split off. Every lamina which exfoliated was on the upper side light grey, and on the lower side blue-black, not brilliant but dull. In the same way the copper plate had now a similar dull blue-black covering. This blue-black body is cuprous selenide, Cu_2Se. There were thus three layers—copper, cuprous selenide, and selenium; and in the copper-selenium-zinc cell two other layers—selenide of zinc and zinc; so that all together there were five layers.

I took thinner copper plates; but the splitting-off still occurred.

This experiment shows that between the copper and the selenium, or rather the cuprous selenide, there is only a slight and imperfect contact.

But the same is the case in the photophonic cell. In this also the three layers—copper, cuprous selenide, selenium—follow one after the other. The plates give only an enlarged view of what is to be observed in the photophonic cell on a smaller scale, and with more difficulty, by the eye. Thus we see that in the photophonic selenium cell also we have only a similar slight and imperfect contact, which is to be influenced by radiation. It was first by this experiment that it became obvious to me that a microphonic effect is the essential part of the action of the selenium cells.

I was confirmed in this conclusion, that *the selenium photophone is a microphone*, when I learned that Mr. Sumner Tainter had constructed a photophone in which selenium was replaced by carbon. Indeed his apparatus, the zigzag line filled with carbon on the silver-coated glass plate, is nothing else than a microphone. If we remember the apparatus described by Mr. Hughes as a thermoscope, we understand that the selenium photophone of Messrs. Graham Bell and Sumner Tainter agrees in its principle with the carbon photophone of the latter; and, again, this is in its main features the same as the thermoscope.

Thus my attention became now more directed to those passages in the literature where the degree of the resistance at the surfaces of contact between the selenium and the metal, in comparison with the total resistance, is discussed.

In 1875, before the invention of the microphone, Dr. Werner Siemens observed a high resistance at the surfaces of contact. He arrived at the conclusion "that an essential part of the resistance of the selenium is in its limiting layers at the surfaces of contact"*.

Mr. Sabine is of the same view. Finding, *e. g.*, in one piece of selenium with several transversal platinum wires the resistances of the junctions to be 429, 479, 498, and 428 megohms, the resistance of the selenium itself between the wires, however, much smaller (22, 13, and 0 megohms), he remarks:—"It is clear from these measurements that a large portion of the observed resistance of a so-called selenium resistance may, and frequently does, reside in the junctions, and not in the selenium. Therefore the larger we make the surface of contact between the platinum and the selenium, the less likely are we to find an otherwise sensitive piece of selenium rendered comparatively insensitive by the introduc-

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* Pogg. *Ann.* clix. p. 140.

tion of high junction resistance. In this respect the form of selenium plate designed by Dr. Werner Siemens, in which the platinum wires form gratings or interlying spirals, is unquestionably the best form to employ when the object in view is to obtain a high sensitiveness to light" *.

Both authors assume the selenium to possess a special sensitiveness, and this quality to be damaged by the high resistance of the junctions. Therefore they aim at annihilating this resistance at the surfaces of contact. Aiming at this annihilation, they enlarge the surfaces of contact and thus diminish the resistance.

The surfaces of contact should indeed be enlarged. The reasons, however, for which this must be done are, in my opinion, just the opposite to those influencing these two observers. It is the high resistance at the junctions which is the variable, and which is necessary for the microphonic action. The case of this resistance alone being the variable I shall discuss immediately. That the resistance at the junctions is high is a necessary consequence of the imperfect and variable contact. To annihilate it is to render the photophone insensitive. And, *vice versâ*, the more extensive the surfaces of contact are, the more sensitive is the selenium cell.

A further strong support, amongst others, is given to this theory by the observation of Mr. Bidwell †, who has made very numerous experiments with selenium cells. "He got the best speech from cells of high total resistance . . . The selenium should, however, have a low specific resistance."

These observations are easily understood by the microphonic action of the cells; for, that the total resistance is to be great and yet the specific resistance of the selenium is to be small, has no other meaning than that there must be a bad contact between good conductors.

The next question is, if this microphonic action is confined only to the surfaces of contact. I must here recall that Messrs. Draper and Moss ‡, distinguishing three crystalline modifications of selenium, observe as to their conductivity, "Between these two forms of granular selenium—the apparently nonconducting and the comparatively highly conducting—there is another, of intermediate resistance. This modification is highly sensitive to light."

Just so, according to Rammelsberg §, selenium exists in four allotropic modifications, three of which are crystalline.

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* 1878, Phil. Mag. (5) v. p. 404.

† London Phys. Soc. Jan. 22, 1881; Tel. Journ. ix. p. 52.

‡ 1873, Proc. Irish Ac. (2) i. p. 533, Nov. 10.

§ 1874, Pogg. *Ann.* clii. p. 151.

For evidence of the microphonic action I was at first contented to observe that selenium in this crystalline state, in which it is most brittle and the most powdery, is also most sensitive to light. Afterwards I succeeded in obtaining a piece which shows this very clearly. Between two platinum wires of 3 centim. length was a selenium plate 8 millim. in width and hardly 1 millim. in thickness. Heated in air and annealed, the whole surface had taken the light grey colour of a crystalline state; half the upper surface, on the side of the one platinum wire, was smooth and continuous; the other half, near the other platinum wire, was powdery and granular. Alternately one of the two halves was kept constantly dark, and the other half at the same time intermittingly illuminated. Thus I could observe that almost the total sensitiveness of the whole plate resided in the granular coarse half of the surface. This shows that microphonic effects occur also in the selenium itself. But even if the selenium were perfectly homogeneous, and remained so during the illumination, the rays would produce heat and change of volume, which would have a microphonic influence on the contact.

By such microphonic changes the efficiency of the selenium cell would be explained, and thereby the selenium would be removed from its isolated position and coordinated with all the other bodies in which change of volume and of form under the influence of radiation have been observed.

I, however, do not deny that light may have other effects on the selenium. That such is the case is indeed one of the causes which have hitherto masked the microphonic action of the cells. That my present view of the simple action of the selenium did not present itself at once is owing to two causes.

In the first place, there is the fact that the microphonic variations or changes in volume and in form, attributed commonly to the so-called heat-rays, are produced in the selenium by the illuminating rays.

In the second place, complication is introduced by the fact that selenium exists in four allotropic modifications, and that light is able to exert on these divers influences. Though these are not essential to the action of the photophone, it has nevertheless been assumed by others, and at first by myself, that such changes might be the key for the explanation of the selenium photophone.

Of these two impediments which stood in the way of the perception of the microphonic action of the cells, I intend to speak more in detail. And first a few words on what, for the sake of brevity, may be called the heating effect of the light-rays.

In the beginning of April* I repeated the experiment of Mr. Graham Bell, and allowed light to pass through ebonite. In order to be able to make a quantitative observation I connected the selenium cell, on which the light was to fall, not only with the telephone, but also with a galvanometer. While, however, it appeared that a photophonic effect took place through the ebonite, it was shown that this effect was only a small part of the direct effect when the ebonite diaphragm was away. Now, as ebonite allows passage only to red and ultra-red rays, we learn from this experiment two things:—

(1) That the illuminating rays are those which are especially absorbed by selenium, and that these produce the greater part of the photophonic action.

(2) That it is even possible to make *a selenium photophone without light*—that is, with exclusion of illuminating rays and by the influence of heat-rays only.

That there is simply a heating effect of the illuminating rays in the selenium photophone has perhaps not yet been sufficiently insisted on, because the most modern researches on light-rays are not yet assimilated to the general view of physics. We find still the conception of three different kinds of rays—heating, illuminating, and actinic; whilst it has long been demonstrated that there is only one form of rays, differing from one another, however, in wave-length and intensity. On the body on which the ray falls depends whether its energy is perceived as heat, or light, or chemical effect. In order to be effective a ray must be absorbed. The bodies, however, on which the rays fall select the rays they absorb in the most various manner, which we recognize by the endless varieties of absorption-spectra. The absorbed rays alone are able to exert an effect; they only can warm the body. And heating occurs not only by red and ultra-red rays, but by the rays which are absorbed. Only the absorbed rays can produce (and that is the point in question here) changes of volume and of shape, and in this way influence the contact of current-conducting parts.

As the last-mentioned experiment demonstrates, selenium absorbs principally the illuminating rays. When, therefore, selenium is exposed to radiation, the change of volume and of shape is produced chiefly by the illuminating rays; *selenium is heated by light.* It must be the illuminating rays which make the selenium cell act microphonically.

That light, however, may produce in selenium other changes than heat and deformation, which are essential to

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* Lond. Phys. Soc. April 9, 1881; 'Nature,' xxiii. p. 595.

the efficiency of the selenium cell, has been the second cause which masked the microphonic action of the cell. These are changes in the material; they concern rather the chemistry of selenium. They are indeed purely chemical if we view the existence of selenium in four allotropic modifications as a chemical quality. Into these chemical changes I shall enter here only so far as it is necessary for the proof that they are not essential to the microphonic action of the selenium cells, since my aim now is the physical one of correlating two forces.

When, now, we no longer consider the selenium cells especially, but selenium in general, we find that light can produce in it, if the selenium is an element and pure[*], no other chemical changes than those which induce the transformation of one of the allotropic modifications into another one. But such a transformation is connected with development of energy; for in 1851 M. Hittorf[†] observed a rise of temperature of 90 degrees when the vitreous modification changed into the crystalline; and Regnault[‡] (1856) in a similar case, observed an elevation of temperature of 130 degrees. With a proper arrangement (that is, in a closed circuit) we shall get, instead of the development of heat, an electric current—just as, for instance, two solutions of the same salt, but of different concentration, on being mixed together, give a development of heat, but, when brought into a circuit in a proper manner, produce electricity equivalent to this heat.

I have already mentioned above that Prof. W. G. Adams thus observed electromotive forces on illuminating selenium connected only with a galvanometer, but that generally the photophonic cells do not give such a development of electricity, and that therefore this property of selenium cannot be used for the explanation of the efficiency of the photophonic cells. (Here I will add that of course thermoelectric currents can be produced by differential heating of the two junctions. In order to get such a great difference of heating, the cell must be exposed to greater heat. Thus I brought near the focus of the sun's rays only one small part of the cell, and found a small deflection of 10 divisions, or about half a centimetre.)

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[*] On melting selenium, a grey film was formed on the surface. This being removed by a platinum spatula, the amorphous selenium showed a brilliant surface; and only such selenium was employed, in order to get congruent results.

Selenium crystals of 3 millim. length were obtained by sublimation on the cover of the crucible. At first amorphous selenium condensed; afterwards these sublimated crystals, which were insoluble in water, were formed.

[†] Pogg. *Ann.* lxxxiv. p. 216.

[‡] *Ann. Chim. et Phys.* (3) xlvi. p. 284.

That by radiation the allotropic modification of selenium may be changed, but that such changes do not occur in the selenium cells, was proved most conclusively by one piece of selenium, which had the anomalous property of showing increased resistance on being illuminated.

Till now I had prepared all selenium cells and all pieces of selenium as I have described above, in the open air or in an air-bath. I never succeeded in obtaining selenium pieces of such low resistance as Messrs. Adams and Day got in three of their pieces, the resistances of which were 55, 58, 68 ohms. These three pieces differ, it is true, strongly from most of the other pieces in their resistances, which were as high as 7,600,000 ohms. I therefore followed their method almost literally. I laid several pieces of amorphous selenium, furnished with platinum electrodes and wrapped in paper, for twenty-four hours in sand which had been warmed before by a red-hot iron ball,—a process in which the influence of the aqueous vapour evolved from the paper is not excluded. In this way I got, indeed, pieces of the comparatively low resistance of 700 and 2000 ohms; whilst that of the other pieces prepared at the same time amounted still to 350,000 ohms.

The selenium piece of lowest resistance (700 ohms) showed at first so variable a resistance, that this could scarcely be measured. Then, when it became more constant, a determination of 700 ohms was possible. But on exposure to light, the resistance *increased*. This behaviour is just the opposite of what all my other cells or pieces of selenium show. And it is equally in contradiction to the diminishing of resistance found in all other cases by all other observers; only Messrs. Adams and Day mention at the end of their numerous observations, that one single piece out of the great series behaved like this of mine, increasing its resistance on being illuminated. This piece of mine in which I observed this anomalous quality, exhibited in general the tendency to rise steadily in resistance; so that there occurred, on illuminating it, a continuous increase of resistance, and, on darkening, a feeble but never a complete return to the original value. Every exposure to light again renewed this increase; and even a slight shaking of the table changed the resistance. This tendency to increase the resistance was manifested so strongly, that on the following day I found the resistance had risen from 700 to 5600 ohms.

But this piece of selenium showed still another anomaly, contributing, however, to the explanation of the first. I intended to examine the polarization produced in the selenium by the primary battery-current when separated from the battery and connected with the galvanometer by means of a switch. And now I found a secondary current which went through the galvanometer always in the same direction, quite independent of the direction of the primary battery-current. Even when the primary current was produced by a single Leclanché cell only, the deflection by the secondary current was 80 divisions, or about 5 centim. This secondary current is therefore no polarization-current; for in that case its direction would be always opposite to the primary. But here a change of direction did not occur, the secondary current always flowing from the platinum electrode A to the platinum electrode B, and never in the opposite direction.

The passage of a current also, like illumination or concussion, occasioned a quicker increase of resistance.

All these qualities, however, are very easily understood; and the very appearance of the piece suggests the explanation. There are in it several modifications of the selenium side by side. At the transformation of the one modification into another, as I have pointed out above, heat or electric effect must take place. A state of equilibrium is not reached before that modification has been formed the formation of which is attended by the maximum development of energy—thermal or electric. Such modification again, as also mentioned above, has a different resistance from the one from which it originated; and therefore the variation of resistance is a necessary consequence of the change of modification. While the transformation goes on, we observe the electric current; after the transformation is complete we observe the change of resistance.

Such changes in the modification can be also occasioned by light: a pencil of amorphous selenium assumes, when exposed to the daylight, a grey crystalline surface.

Two conditions distinguish these changes from the photophonic ones, and prove that, as a rule, they do not occur in the photophonic cells.

On darkening the cell in which only a microphonic action took place, the original state was reached again which it had before being illuminated; in the piece of selenium wherein a change of modification occurred, this was not the case. This is the reason why the light-spot returns to its original position on the galvanometer-scale in the case of the selenium cell, but does not in the case of the selenium piece. And, secondly, by such changes of modification a phenomenon is produced which has been called "fatigue." Such fatigue (that is, an insensitiveness) must take place as soon as the store of the one modification which is transforming itself into the other is exhausted. But it cannot take place if we have to deal with a microphonic action alone. And, indeed, in the selenium cells I never could observe this fatigue. The sensitiveness of the last-described piece is now extremely lowered.

And thus we see that there are two different effects which light may have on selenium and on selenium cells. The one is more of a chemical, the other more of a physical character. The one is a changing of modification, and is not essential to the efficiency of the selenium photophone. In this latter we have to deal essentially with a heating effect, changing volume and contact—in brief, with a microphonic action.

Therefore, as to the photophonic efficiency of selenium, I see no reason to separate it from all other bodies; and I no longer believe that there is any prospect of finding an unknown power or a new relation of forces in this substance.

The above experiments were performed in Prof. Guthrie's Physical Laboratory at the Science Schools, South Kensington. The permission granted me, at his request, by the Department of Science and Art to carry out my ideas experimentally is only one of the many acts of kindness and courtesy which I have received from scientific men and Societies during my sojourn in England.

Physical Laboratory, South Kensington,
June 1881.

XXVII. *Remarks on* Prof. Hughes's *Papers on Molecular Magnetism. By* Prof. G. WIEDEMANN[*].

IN the 'Proceedings of the Royal Society of London,' vol. xxxi. p. 532, vol. xxxii. pp. 25, 213 (1881), Prof. Hughes has published a series of papers on the relations between magnetism and torsion, upon which I beg leave to make the following remarks:—

In his first paper, Prof. Hughes has quoted from my work on Galvanism and Electromagnetism (1st ed. vol. ii. p. 438, instead of p. 447 as cited) an experiment of mine, by which I had shown that an iron wire through which a current flows is magnetized by torsion. He says that Sir William Thomson had attributed this magnetization to a spiral arrangement of the current in the twisted wire.

Now, in the passage above referred to, I state distinctly that, if an iron wire is twisted during or *after* the passage of a current, it becomes magnetic. In the latter case there is no longer any current in the wire. I have observed this magnetization directly by the deflection of a magnetic mirror, and determined its direction. In accordance with many other observations I have fully treated the rotations of the magnetic

[*] Communicated by the Author.

molecules upon which this magnetism depends, and illustrated it by a woodcut*. Still more fully are these phenomena and others related to them treated in the volumes of Poggendorff's *Annalen* from 1858 to 1862, quoted in my work†.

In the year 1866 (Pogg. *Ann.* cxxix. p. 616) I showed that when a current is directed through an iron wire extended in the axis of an induction-coil and the wire is twisted or untwisted, induced currents are produced in the coil; further, that similar induction currents are produced by twisting or untwisting the wire after having interrupted the current passing through it; also that analogous induction currents pass through the wire itself when, after the passage of the current, its ends are joined to a galvanometer and it is twisted or untwisted.

These results in each case follow directly from my former direct observations on the magnetization of an iron wire by twisting or untwisting during or after the passage of a current.

It is not difficult to vary the experiments, as, in fact, Prof. Hughes has done. My purpose being to establish the principles of these phenomena, I considered it best to study them under the simplest possible conditions.

It gives me great pleasure to find that the experiments of so distinguished a philosopher as Prof. Hughes, which either agree completely with my own or have been made with some variations of method, confirm the results which I obtained about twenty years ago, and that, in describing molecular magnetism as a singular form of magnetism, he has independently come to the same conclusions as those published by me in the years 1858–1862, when enunciating the theory of the relations between magnetism and mechanical changes of magnetic bodies.

Leipzig, August 6, 1881.

XXVIII. *On* Mr. R. Shida's *Note published in the Philosophical Magazine for August* 1881.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE contents of Mr. Shida's last note, and the addendum thereto by Sir William Thomson, render it imperative upon me unwillingly to trouble you again upon this subject—I trust, for the last time. In Mr. Shida's original papers (Brit. Assoc. Reports, 1880, and Phil. Mag. Dec. 1880) he stated that, in the course of his electrostatic valuations of v,

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* *Galvanismus*, 1st ed., ii. p. 453; 2nd ed., iii. (1) p. 573.

† Pogg. *Ann.* ciii. p. 563 (1858); cvi. p. 161 (1859); cxvii. p. 203 (1862); and *Monatsber. d. Berlin. Acad.* 1860, 29 Nov.; also in the *Annales de Chimie et de Physique*, the *Archives de Genève*, Silliman's Journal, &c.

"the E.M.F. of Thomson's gravity Daniell was measured by comparing it before and after the above experiment [the electromagnetic valuation] directly with that of the above battery by means of Sir William Thomson's quadrant-electrometer:" the which wording conveyed to me the impression that both cell and battery were compared under the same conditions, viz. with insulated poles, no current flowing. Incidentally referring to Mr. Shida's paper (Phil. Mag. May 1880), I noticed that such a mode of working would introduce an error due to polarization, the E.M.F. of the cell being less when generating a current than when its poles are insulated. Mr. Shida rejoined (Phil. Mag. June 1881) that the quadrant-electrometer comparison was made whilst the cell was generating a current, so that the polarization-error was avoided. I then pointed out (Phil. Mag. July 1881) that, if Mr. Shida made the quadrant-electrometer comparison whilst the cell was generating a current, the electrometer-reading would be less than the true E.M.F. in the ratio of the external resistance to the total resistance in circuit, the difference being in Mr. Shida's experiments about 2 per cent. Mr. Shida now replies, in effect, that this 2-per-cent. error was obviated in his experiments by multiplying the electrometer-reading reduced to C.G.S. units, $V-V'$, by the factor $\frac{b+\rho}{\rho}$ (where b is the internal resistance of the cell, and ρ the resistance of the rest of the circuit*). That this correction was adopted may also be gathered from Sir W. Thomson's denial of the existence, in Mr. Shida's experiments, of any such error as the omission of this factor would entail. I confess, however, that I am at a loss to understand how pointing out that the omission of the factor would introduce an error can be construed into a "misunderstanding of Ohm's Law."

After Sir W. Thomson's explicit statement that Mr. Shida's experiments were free from the source of error pointed out, I have to express my regret at having, as it appears, too hastily concluded that Mr. Shida did not apply a particular correction when, in point of fact, he only omitted to mention that he applied it. At the same time it is only due to myself to point out that the numerical values quoted by Mr. Shida as his results are not of such a nature as to lead the reader to put implicit confidence in the rest of the work, and that *à priori* they seem to suggest that when corrections and precautions were not mentioned they were probably omitted.

In order to calculate the electromagnetic value of the

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* Mr. Shida uses r instead of ρ. I have altered the symbol to avoid confusion with the other r used by Mr. Shida in his original paper, referred to later on.

E.M.F. of the cell, Mr. Shida used the following formula,

$$E = R \, . \, H \, . \tan \alpha \, . \, K,$$

where $R = b + \rho$, H is the horizontal component of terrestrial magnetism, α is the angle of deflection of the tangent-galvanometer, and K is a constant calculable from the dimensions of the instrument &c. With the object of obtaining several values of $R \, . \tan \alpha$ and averaging them, Mr. Shida caused R to vary within certain limits: unless considerably different currents were made to flow, no sensible alteration in the E.M.F. through polarization would occur; so that the product $R \, . \tan \alpha$ should be sensibly constant, barring unavoidable errors of observation. Mr. Shida states, in each of his original papers, that $R = g + b + r$, where g is the resistance of the galvanometer (measured as 30·86 ohms), b is the internal resistance of the cell (found to be 2·02 ohms), and r is a variable resistance introduced to vary the current and to bring the deflection to near 45°. Mr. Shida then gives in each paper the following table of numbers obtained—

α.	r.	R.
$45^\circ \, 15'$	80 ohms	107·88
42 45	100 "	112·88
51 39	50 "	82·88

and thence concludes that the mean value of $R \tan \alpha = 104{\cdot}73 \times 10^9$.

Now the above numerical values are irreconcilable. Whether Mr. Shida has copied down the wrong figures from his notebook, or how he came to print these numbers, I cannot say; but the fact remains that they cannot possibly be all right. That the accidental omission to correct errors of the press is the cause of the discrepancies seems improbable, not only from the nature of the errors themselves, but also from the fact that the second (Phil. Mag.) paper is not an exact replica of the first (B. A.) paper, various sentences having been altered, so that an additional revise must have been made.

First, if $r = 80$, since $R = g + b + r$, and since the galvanometer and cell were necessarily used in all the determinations, it follows that if $g = 30{\cdot}86$ and $b = 2{\cdot}02$ as stated, R must $= 112{\cdot}88$ instead of 107·88. Secondly, if $r = 100$, it similarly results that $R = 132{\cdot}88$, not 112·88. If it be supposed that in these two cases the values of R are correctly quoted and those of r wrongly given, then r must have been 75 ohms in the first case and 80 in the second, instead of 80 and 100 respectively. Thirdly, whether it be assumed that the values of R or those of r are correctly given, in neither case is the mean value of R tan α what Mr. Shida says it is. In the first case we have—

α.	tan α	r.	R.	R tan α.
45° 15′	1·0088	75	107·88	108·83
42 45	0·9244	80	112·88	104·35
51 39	1·2639	50	82·88	104·75
			Mean . . .	=105·98

whilst in the second case we have

α.	tan α.	r.	R.	R tan α.
45° 15′	1·0088	80	112·88	113·87
42 45	0·9244	100	132·88	122·84
51 39	1·2639	50	82·88	104·75
			Mean . . .	=113·82

In either case the values of R tan α are very far from being constant, whilst the mean value materially differs from 104·73 ohms.

Two other slips, presumably made either in transcribing or during proof-corrections, have been already noticed, viz. the statement (in each paper) that the electrostatic values of the electromotive forces of the battery and cell employed were 0·904187 and 0·034380 C.G.S. units, instead of one tenth of these quantities in each case respectively.

Whatever may be the explanation of these numerical aberrations, it is at least evident that when an author twice over publishes such numbers as these (and only such numbers) as samples of his work, he can hardly be surprised if readers conceive it to be possible that oversights may have been made in other portions of the research, especially when incomplete or ambiguous descriptions are given tending to convey the impression that oversights were actually made. *Humanum est errare.*

In conclusion, I must remark that Mr. Shida misrepresents me when he says that I call the difference of potential, V−V′, subsisting between the poles of an electromotor which is generating a current, the E.M.F. of that electromotor. On the contrary, I expressly define (Phil. Mag. May 1881, p. 359, footnote) the E.M.F. of such an electromotor, E, to be this difference of potential "*corrected for the effect of the current passing* (*if appreciable*)," the correction being the addition of the quantity $bC = \frac{b}{b+\rho}E$. This is obviously equivalent to defining the E.M.F. as $(V-V')\frac{b+\rho}{\rho}$.

I have the honour to be, Gentlemen,
Your most obedient Servant,
CHARLES R. ALDER WRIGHT.

Chemical Laboratory, St. Mary's Hospital, W.,
August 3, 1881.

XXIX. *Intelligence and Miscellaneous Articles.*

ON THE DETERMINATION OF THE ANGULAR DISTANCE OF THE COLOURS. BY M. A. ROSENSTIEHL.

TWO series of experiments made upon a chromatic circle called my attention to three colours appearing to fulfil special functions*. I was thus led to discuss Young's theory† on the three fundamental sensations, and to establish two categories of non-complementary colours:—1, those which by their mixture in threes at equal intensity produce the sensation of white; 2, those which produce that sensation outside of those proportions.

I have designated the first category by the name "triad;" and I remarked that from their definition it follows that, representing the table of colours by an equilateral triangle of which each vertex is occupied by one of the colours of a triad, the complementaries will be placed, in that construction, on the sides of the triangle and at the extremities of a straight line passing through the point of meeting of the median lines, while this regularity cannot exist for the second category of combinations of three colours.

The aim of the present note is to prove that the three colours above referred to, viz. orange, the third yellow-green, and the third blue, possess the characters of a triad.

By a first series of experiments I determine the pairs of complementary colours. By a second series I ascertain the proportions in which the three colours chosen must be mixed in order to obtain the sensation of white. By a third series I seek to reproduce a determined colour by the mixture of two or three colours: all these experiments are made with rotating disks; the measurement of the angles of the sectors gives the position of this colour on the sides of the triangle. In the calculations I do not take into consideration the white which is produced at the same time, but only the sensation of colour, my aim being to determine the proportion in which two different colour-sensations must be mixed to obtain a

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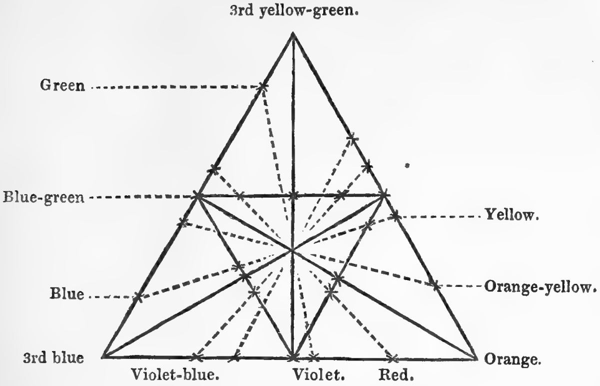
* Phil. Mag. March and April 1881, vol xi. pp. 222, 305.

† Ibid. vol. xii. p. 78.

determined sensation of colour. The results of the three series of experiments are summed up in the following figure.



It is there seen that the straight lines which join the complementary colours intersect in a single point. Consequently the three separate colours form part of a triad; here is their position in the spectrum:—

Orange at .... $\frac{3}{4}$ from C to D.
Green (3rd yellow-green) at $\frac{3}{4}$ from D to E.
Blue (3rd blue) at........ $\frac{1}{3}$ from F to G.

I repeated the last two series of experiments, methodically replacing one of the three colours by another. I did the same with the three colours which Maxwell considers fundamental. None of these systems give regular results. Some colours which to the eye are very close are in them separated by large spaces, while some distinct colours are there brought very near together and almost blended, and the lines joining two complementary colours do not intersect in only one point.

The two propositions above formulated are therefore in accordance with experiment.

To complete the demonstration of the existence of the triads, it remains for me to prove that *all the colours which occupy the vertices of an inscribed equilateral triangle possess the same properties.*

I ascertained that it really is so by recommencing the same series of experiments with the triad formed by the first violet, the blue-green, and a yellow comprised between the first and second yellows of my chromatic circle. These three colours are distant 120° in the above figure. The results of the experiment are there indicated. It will be remarked, not only that the complementary colours are situated at the extremities of a straight line passing through the point of intersection of the median lines, but also that the new figure is exactly superposable to the first in what concerns the position of the intervening colours. We may conclude from this that *the angular distance of the colours is the same, whichever may be the triad employed as starting-point.*

This angular distance is therefore determined once for all; but not so the form of the table of colours, which is independent of it, since the same result can be arrived at with an infinity of triads.

Which of the triads corresponds to the fundamental sensations? To answer this question we have no longer exact measures to guide us, but only eye-estimates. It may in reality be enunciated thus:—Are the colours for which the angular distance is the same equidistant to sight? If the answer is yes, all the triads have the same value, and the table of the colours can be represented by a circle. If, on the contrary, there are colours equidistant to sight whose complementaries appear more distant from or nearer to one another, the table of the colours presents angles.

The study which I have previously made of the distribution of the complementary colours in a chromatic circle shows incontestably that all the colours comprised between the red and the orange-yellow have complementaries very near the blue-green, and those comprised between the blue and the violet-blue have their complementaries very close to the yellow. There are therefore vertices towards the violet-blue and the orange. The difference appears to me less accentuated for the third yellow-green.

However it may be with the form of the table of colours, its projection upon the sides of an equilateral triangle has permitted the determination of the exact angular distance of the colours—a result which had not yet been obtained, and which suffices to connect the facts at present known. The above diagram represents the law of the mixture of colours, and may be substituted with advantage for Newton's rule.

It has moreover an æsthetic meaning: by the determination of the triads it gives facility for applying to arrangements of three colours the rules of harmony which I have given for the employment of complementary colours in decoration.—*Comptes Rendus de l'Académie des Sciences,* July 25, 1881, t. xciii. pp. 207–210.

ON A METHOD FOR AMPLIFYING DISPLACEMENTS OF THE PLANE OF POLARIZATION. BY M. H. BECQUEREL.

When monochromatic luminous rays pass through a *half-wave* crystal plate, the emergent rays are, as is known, rectilinearly polarized in a plane which, in regard to the axis of the crystal plate, is symmetric with the plane of polarization of the incident rays. This property of a *half-wave* plate can be utilized for doubling and trebling the measures of the displacements of the plane of polarization of light. This can be accomplished especially in the following manner:—

Having arranged an experiment for the purpose of measuring a rotation of the plane of polarization of light, the investigator commences by fixing with the greatest care, by means of the analyzer, the initial position of the plane of polarization of the incident rays. A *half-wave* plate is then interposed in front of the analyzer and rotated until the plane of polarization of the light-rays is not deflected by their passing through the plate. In this position the axis of the crystal plate either coincides with the plane of polarization of the rays studied or is perpendicular to it.

This first determination having been effected, he produces the physical phenomenon which will give rise to the rotation sought: this will be either the interposition of a substance possessing a natural rotatory power, or a magnetic influence, or some other phenomenon.

An equal rotation in the opposite direction to that which is really produced is then observed through the crystal plate. Upon the divided circle in the centre of which the analyzer is mounted, the position of the plane of polarization thus observed is determined, aud then the *half-wave* plate is withdrawn. The rays are then received upon the analyzer polarized in the direction given them by the experiment which was had in view; and this direction is determined upon the divided circle. The angle of the two planes of polarization, symmetrical with respect to their initial position, is double the angle which would have been observed directly (without making use of the *half-wave* plate).

In this second position the observer can replace the crystal plate so that it does not give rise to any new deflection of the plane of polarization; and then he stops the phenomenon he is studying. The plane of polarization of the rays which have passed through the plate, instead of returning to its initial position, is thrown symmetrically on the other side of the axis of the plate. The angle between the extreme deflections obtained by the different operations we have just indicated is treble that which would have been obtained by a direct measurement.

This method can be employed in a great number of circumstances. It is peculiarly applicable to the system of two planes of polarization forming between them a constant angle, such as are obtained with a penumbra-polarimeter, especially a cut Nicol. The determinations are then made with great facility and precision.

In an investigation in course of execution I have had occasion to make use of the experimental arrangement I have just described; and it has given me the best results.

An ordinary observation consists of two viewings, each susceptible of an error. The method above explained admits of four errors for the double deflection, and six for the triple deflection. It is therefore necessary to determine whether the measurements thus made possess greater precision than direct measurements.

If the experimenter placed the axis of the *half-wave* plate every time rigorously in coincidence with the plane of polarization of the incident rays, or with the plane that bisects the system with penumbræ, it is evident that the same precision of measurement applied to angles twice or thrice as large would diminish in the same proportion the probable errors. The precision of the method depends therefore on the perfection of the preliminary determination. Now, with the penumbra-polarimeter, for example, a small divergence between the axis of the crystal plate and the plane of symmetry of the polarizer gives rise to a variation of twice that amount in the relative position of the two planes of polarization; so that the sensitiveness of the observation is considerably augmented, and the determination is made with much greater precision than the ordinary measurements.

On discussing the perturbing influence of a small difference between the real position of the plane of symmetry of the polarizer and the initial position of the principal plane of the analyzer, one perceives that the error committed in the orientation of the axis of the crystal plate is about one half less, and that, if all the viewings are made with the same precision, the employment of the foregoing method reduces the errors of observation to about three fourths of their value for the double, and to two thirds for the triple deflections.

Besides, the observer finds in the different determinations a very precious mutual control, permitting him to fix the value of the approximation which the numbers obtained permit.

The foregoing considerations have induced me to lay before the Academy this new method, which permits one, by the mere interposition of a suitably chosen crystal plate, to augment the precision of the measurements respecting various phenomena of *rotatory* polarization, the observation of which is sometimes an extremely delicate process.—*Comptes Rendus de l'Académie des Sciences*, July 18, 1881, t. xciii. pp. 143–145.

CHANGE OF STATE: SOLID-LIQUID.

To the Editors of the Philosophical Magazine and Journal.

Mason College, Birmingham,
August 4, 1881.

Gentlemen,

In a paper on the Change from the Solid to the Liquid State, which appeared in the July number of the Philosophical Magazine, I described some experiments on the temperature of melting ice when the ice is under much greater pressure than the water in contact with it. In these experiments I thought I had obtained some confirmation of the preceding theory, that the melting-point would be lowered much more than when both ice and water are submitted to the same perssure.

I have lately repeated the experiments more fully; but I do not find my previous results confirmed. I can only suppose that they were wrong, and that the error arose from insufficient precautions as to the temperature of the thermopile-junction which was placed in melting ice. I now find that the water freezes in the pores of the sand around the junction placed under the compressed ice. As, therefore, the conditions necessary to success are not realized, there is no reason to expect any lowering beyond that which takes place when both ice and water are submitted to the same pressure.

I regret that the hasty publication of my previous experiments should render it necessary for me to ask you to publish this explanation.

I remain, Gentlemen,
Yours faithfully,
J. H. Poynting.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

OCTOBER 1881.

XXX. *The Domain of Physiology; or Nature in Thought and Language.* By T. Sterry Hunt, *LL.D., F.R.S.**

Part I.—*Historical.*

1. Etymology and significance of *Physis* and *Natura*. 2. Physical Science defined. 3. *Physicus* and *Physiologia*. 4. Physic or Natural Philosophy; Gower, Locke. 5. Physiology defined. 6. The Greek Physiologists; Humboldt. 7. General Physiology; Cudworth, Moore, Stewart, Burke, Philosophical Transactions. 8. Special Physiology, Glanvil; Mental Physiology, Brown; Reynolds. 9. Physic and Physiology in Medicine; Chaucer. 10. Physician; *Naturien* and *Naturiste.* 11. Hippocrates; Nature in Medicine. 12. Hippocrates as a Natural Philosopher. 13. Alexandria; the Greek and the Arab Physicians. 14. The terms *Médecin* and Mediciner.

§ 1. THE importance of a correct and well-defined terminology in science cannot be overestimated, since a want of precision in language leads to vagueness in thought, and often to errors in philosophy. There are few more striking examples of indefiniteness in language than can be found in the use of the words physic, physiology, and their derivatives. The material universe is designated with etymological correctness as physical (that is to say, natural)—a term which belongs alike to the organic and the mineral kingdoms; but in the use of this and of other words having a similar etymology (Gr. φύσις, Lat. *natura*) we find in modern language many restrictions, limitations, and ambiguities. It will aid us in our present inquiry if we bear in mind that both the Greek *physis* and the Latin *natura* involve the notion of a

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generation or growth, and that the adjectives physical and natural, in their origin, imply the results of a formative process or evolution. The term *physis* (which we translate by nature), as employed by Aristotle, denotes that which is at once self-producing, self-determined, and uniform in its mode of action.

§ 2. The substantive physic (*φυσικὴ*, *physica*, *physique*) has been employed by philosophers since the time of Aristotle to signify the knowledge of all material nature. "Physical science," as well defined by Clerk Maxwell at the beginning of his little treatise on Matter and Motion, "is that department of knowledge which relates to the order of nature, or, in other words, to the regular succession of events. The name of physical science, however, is often applied in a more or less restricted manner, to those branches of science in which the phenomena are of the simplest and most abstract kind, excluding the consideration of the more complex phenomena such as are observed in living beings."

§ 3. To the student of natural phenomena Aristotle gave the names of *physikos* and *physiologos*. These words were adopted in the same sense by the Romans, who made use of the substantives *physicus* and *physiologia* to designate natural philosophers and natural science. Cicero writes of the physicus or physician Anaxagoras, and employs the word physiology to denote "the science of natural things," in accordance, as he tells us, with Greek usage*.

§ 4. The earlier English writers followed the Greek and Latin usage, and employed the substantive physic (or physike) in the same sense as Aristotle. Thus, in the fourteenth century, Gower defines physic as that part of philosophy which teaches the knowledge of material things, the nature and the circumstances of man, animals, plants, stones, and every thing that has bodily substance†. Descartes, in the seventeenth

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* Cicero, *Varr.* lib. I. R. R. cap. 40: "Si sunt semina in aere, ut ait physicus Anaxagoras." Also *De Nat. Deorum*, I. 4: "Rationem naturae quam physiologiam Graeci appellant." In the *Totius Latinitatis Lexicon* of Facciolatus and Forcellinus we find the definition, "Physiologia, scientia quae de naturis rerum disserit, eadem ac Physica."

† Gower, dividing theoretical philosophy into three parts—Theologia, Physica, and Mathematica, tells us:—

"Physike is after the seconde,
Through which the philosophre hath fonde,
To teche sondrie knowlechynges
Upon the bodeliches thinges
Of man, of beast, of herbe, of stone,
Of fish, of fowl, of euerich one
That be of bodily substance,
The nature and the circumstance."

Confessio Amantis, book vii.

century, employed the word (in French *physique*) with the same signification; and it was subsequently used by Locke in a still more comprehensive sense. He writes of "The knowledge of things as they are in their own proper beings, their constitutions, properties, and operations; whereby I mean not only matter and body, but spirits also, which have their proper natures, constitutions, and operations, as well as bodies. This, in a little more enlarged sense of the word, I call φυσικὴ, or natural philosophy"[*].

§ 5. We have seen that in Latin the words physic and physiology were used synonymously. That they were thus understood by English writers is apparent from the 'Universal English Dictionary' of Edward Phillips (sixth edition, 1706), where Physiology is defined as "a discourse on natural things; physics or natural philosophy; being either general, that relates to the affections or properties of matter, or else special and particular, which considers matter as formed or distinguished into such and such species." Cotgrave, a lexicographer of the seventeenth century, in his 'French and English Dictionary,' also defines Physiologie as "a reasoning, disputing, or searching-out of the nature of things," a definition which is cited by Charles Richardson in his 'English Dictionary,' under Physiology.

§ 6. It was to those who occupied themselves with abstract or *general physiology* (as defined by Phillips) that the Greeks gave the name of physiologists, first applied to the philosophers of the Ionian school, who sought to derive all things from one or more material elements, and thus had a physical basis for their system of the universe, as distinguished from the school of Pythagoras, whose system was based on numbers and forms. Of Empedocles, the author of a didactic poem on Nature, in which we first find enunciated the doctrine of the four elements (fire, air, earth, and water), Aristotle, in his Poetics, makes the criticism that he was more of a physiologist than a poet. Humboldt repeatedly employs the word physiology and its derivatives in the same general sense. Thus, he writes of "the natural philosophy of the Ionian physiologists" (*Physiologien*), which "was devoted to the fundamental ground of origin, and the metamorphoses oi one sole element;" of the "physiological fancies of the Ionian school," and of the teachings of Anaxagoras of Clazomenæ, "in the latter period of development of the Ionian physiology"[†]. Of Anaxagoras it may be observed that his views marked a great advance over those of his predecessors, and that he merited

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[*] 'Human Understanding,' book vii. c. 21.

[†] *Cosmos*, Otte's translation, Harper's ed. ii. p. 108, and iii. p. 11.

the encomium pronounced by Aristotle that he was the first philosopher who had written soberly of nature.

§ 7. We find the word physiology and its derivatives employed in the same general sense by English writers in the seventeenth century. Thus, Cudworth speaks of "the old physiologers before Aristotle," and writes "They who first theologized did physiologize after this manner, inasmuch as they made the Ocean and Tethys to have been the original of generation"*; while Henry Moore says, "It will necessarily follow that the Mosaical philosophy, in the physiological part of it, is the same with the Cartesian"†. Coming down to later writers, we find the word physiologist used in a general sense, as equivalent to our modern term naturalist. Thus, Dugald Stewart calls Cuvier "the most eminent and original physiologist of the present age;" and Burke writes, "The national menagerie is collected by the first physiologists of the time"‡.

We may note in this connexion the two series of abridgments of the Philosophical Transactions of the Royal Society—the first, from its commencement to 1700, and the second, to 1720, both published with the *imprimatur* of Newton, as president of the Society. In these collections the classification of the papers is as follows:—(1) "*Mathematical*," including pure and applied mathematics; (2) "*Physiological*," embracing all meteorological phenomena, tides, terrestrial magnetism, mineralogy, geology, botany, zoology, and the study of the physical world in general. Subjects relating to the human body, however, such as anatomy and medicine, were excluded from part 2, and, with chemistry, made a first division of part 3, in the second and last division of which were included philological and miscellaneous papers.

§ 8. Of the "special and particular physiology," as distinguished by Phillips, we have an example in Glanvil, who, in the seventeenth century, writes of the physiology of comets§. The citation from Burke, identifying physiologists with zoologists, may also perhaps be taken as an example of a special use of the word; while in later times we have come to speak of Vegetable Physiology, Animal Physiology, Human Physiology, and even of Mental Physiology, a term employed by

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[*] 'Intellectual System,' pp. 120, 171.

[†] 'Philosophical Cabbala,' Appendix, c. 1.

[‡] Stewart, 'Philosophy of the Human Mind,' ii. c. 4; and Burke, 'Letter to a Noble Lord.'

[§] "So that we need not be appalled at blazing stars, and a comet is no more ground for astrological presages than a flaming chimney. The unparalleled Descartes hath unravelled their dark physiology, and to wonder solved their motions." (Jos. Glanvil, 'Scepsis Scientifica, ... an Essay on the Vanity of Dogmatizing,' 1665, c. xx.)

Dr. Thomas Brown of Edinburgh*, who speaks of "physiology corporeal or mental"†.

§ 9. There is an example of a special application of the words physiology and physic which requires further consideration. We have already cited Cotgrave's first definition of the word Physiologie, to which he adds, as a secondary meaning, "anatomizing physic, or that part of physic which treats of the composition or structure of man's frame." In more recent times, however, the term has come to mean, not the anatomy, composition, or structure of the human frame, but its functions, to which signification physiology is, in popular language, limited, though now by didactic writers extended to include the functions of the lower animals, of plants, and even of the human mind.

The word physic, as we have seen, was used by Gower in the general sense of a knowledge of all material things; but his contemporary, Chaucer, employed it, in a special and restricted sense, to designate the science of medicine. Thus, he calls his practitioner of the medical art "a doctor of physic," and in his description of this personage adds that "gold in physic is a cordial"‡. Subsequently, and to our own time, we find the term applied, in Chaucer's sense, alike to the art

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* The grounds upon which Brown based this extension of the term physiology may be gathered from the following passages:—"There is, in short, a science which may be called *mental physiology*, as there is a science relating to the structure and offices of our corporeal frame to which the term *physiology* is more commonly applied." He further speaks of the "*physiology of the mind*, considered as a substance capable of the various modifications or states which, as they succeed each other, constitute the phenomena of thought and feeling;" and declares that "the mind is as an object of study . . . . . to be comprehended, with every other existing substance, *in a system of general physics*." (Brown, 'The Philosophy of the Human Mind,' Lectures I., II., and V.)

† Since the writing of this essay, Prof. Osborne Reynolds, in 'Nature' for June 9, 1881 (vol. xxiv. page 123), has made a happy use of the word in question, in writing of the locomotive engine of George Stephenson, of which he says, "the physiology of the machine resembled that of the human system;" while he speaks of its inventor as "he who produced the locomotive physiologically perfect."

‡ "With us there was a doctour of phisik,
In all the world ne was there non him lyk
To speke of phisik and of surgerye,
For he was grounded in astronomye.

. . . .

He knew the cause of every maladye,
Were it of hot or cold or moyste or drye,
And where engendered and of what humoure;
He was a very parfight practisour.

. . . .

Well knew he the old Esculapius,
And Dioscorides, and eke Rufus,

of healing and to its medicaments. If we search for the origin of this peculiar use of the word physic, we shall find it employed with the same meaning in mediæval Latin*. In French also, according to Littré, the term *physique* was in the thirteenth century applied to the science of medicine, the professors of which were then called *physiciens*†, a designation which they kept till the time of Rabelais, and, as we know, still retain in English, though the term *physicien* is at present applied in French only to students of physical science in the restricted sense mentioned in § 2, including what, in didactic phrase, is now called *physique* in French and physics in English.

§ 10. It is a curious inquiry how these terms came to have this restricted use in the middle ages, and how the name of *physicus* or physician, originally applied to the student of material things—and by pre-eminence to Anaxagoras of Clazomenæ, who was called "the physician" (ὁ φυσικὸς)—came to signify in mediæval France and England the *medicus*, *médecin*, or mediciner—the master of the art of healing diseases in the human frame. Menage assigns as a reason for this, that the art "consists principally in the contemplation of nature;" and in this imperfect statement will be found the answer to our inquiry, upon which much light is thrown by the use, in mediæval times, of the words *naturien* and *naturiste*. *Naturien*‡, which is found in the fourteenth century, both in English and in French, is etymologically equivalent to *physicien*, and was applied to certain professors of the art of healing, being appa-

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Old Hippocras, Hali and Gallien,
Serapion, Rasis and Avicen,
Averrois, Damascene and Constantin,
Bernard and Gatisden and Gilbertin.

. . . .

For gold in phisik is a cordial.
Therefore he loved gold in special."
CHAUCER, *Canterbury Tales*, Prologue.

* Du Cange, *Glossarium ad Scriptores mediae et infimae Latinitatis*, ed. Herschel, sub voce Physica.

† "Nous établissons . . . . un fisicien juré et pensionnaire du couvent." (Réglement de l'Abbaye Royale de Soissons, A.D. 1282; cited by Menage, *Dictionnaire Etymologique*, sub voce Physicien.)

‡ The following satirical rhyme of the fourteenth century is cited by Littré, in his *Dictionnaire*, sub voce Naturien:—

"Où le physicien fait fin, Là commence le médecin.
Supposant pour physicien, Le très-savant naturien."

Gower, who uses the word more than once, writes:—

"And thus seyth the naturien,
Which is an astronomien."
Confessio Amantis, book vii.

rently synonymous with *naturiste*, which, as stated by the learned Littré in his *Dictionnaire*, meant "a mediciner who practised expectant medicine—that is to say, who trusted to the conservative influences of nature to heal his patient."

§ 11. For the origin of the physician or naturian in medicine, we must go back more than twenty centuries, to the great Hippocrates, justly styled the father of medicine. It was a maxim of his school that "nature is the healer of diseases"[*]; and himself it was who wrote of medicine, that "the art consists in three things—the malady, the patient, and the mediciner. The mediciner is the servant of nature, and the patient must help the mediciner to combat the disease"[†].

Nature, in the language of the time, was spoken of as a *vis medicatrix*, or healing power; but Virchow justly remarks that, from a careful perusal of the works left us by the great master, we cannot doubt that by nature he meant the whole bodily constitution of man. Hippocrates insisted upon a treatment of diseases based not upon magic nor upon supernatural agencies, but upon the belief that nature works according to a divine necessity; in other words, he taught a system of pathology founded on the recognition of physical laws, which he opposed to the superstitious notions of his caste and his age. The *iatros*, or mediciner, was henceforth no longer a magician, nor a priest, but a physiologist, physician, or naturist, seeking for healing agencies in the study of the physical organization of the patient. The pathology of the Dogmatists, who were the disciples of Hippocrates, was based upon a knowledge of the structure and functions of the human organism, and of the structural and functional modifications produced alike by disease and by the action of drugs.

§ 12. But Hippocrates had still another claim to the title of physician or physiologist, since, not content with studying the physical constitution of man, he insisted upon the importance of a knowledge of all his relations to external nature. In his celebrated treatise 'On Airs, Waters, and Localities,' Hippocrates declares that whoever would understand medicine, must study the movements of the heavenly bodies and all meteorological phenomena, together with physical geography,

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[*] Νούσων φύσιες ἰητροί, Hippocrates, *Epidem.* book vi. sec. 5. 1.

[†] *Epidem.* book i., sec. 2, 5. The received text makes the mediciner "the servant of the art;" but Galen, in his 'Commentary,' tells us that some manuscripts in his time had, instead of ὁ ἰητρὸς ὑπηρέτης τῆς τέχνης, the word φύσιος for τέχνης. This latter reading I have followed, as more consonant with the previously cited dictum; for if "nature is the healer of diseases," the mediciner must be "the servant of nature." See Adams's 'Genuine Works of Hippocrates,' vol. i. p. 360, note; also Littré's 'Hippocrates,' vol. ii. *in loco*.

including climate, soil, vegetation, rocks, minerals, and waters; to which he adds that the mediciner, if he would preserve the health of his patients and succeed in his art, must investigate "every thing else in nature"*.

§ 13. The teachings of Hippocrates and his followers were maintained in the school of Alexandria, where, we are told, the studies were arranged in four divisions or faculties—letters, mathematics, astronomy, and medicine; under which last, as we know from the history of the Museum, were included botany, geology, chemistry, optics, and mechanics. The learning of the Alexandrian school was preserved by the Jews and the Nestorians, and by them handed down to the Arabians, who brought it with them into Southern Europe. It suffices to speak of Djafar, Rhazes, Avicenna, and, later, of the schools of Salerno, Cordova, Montpellier, Narbonne, and Arles, where were gathered together men famed alike in medicine, anatomy, zoology, botany, optics, mechanics, and astronomy, who merited in the widest sense the name which they then bore, of physicians; since they were not simply iatrophysicians, but philosophers who had taken all natural science for their province. Draper, speaking of the Arabians of that age, says:—"Their physicians were their great philosophers; their medical colleges were their foci of learning." "Arab science emerged out of medicine; and in its cultivation physicians took the lead, its beginnings being in the pursuit of alchemy"†. It is to be noted that Chaucer's doctor of physic (§ 9) was not only learned in astronomy and read in the works of the Greeks, Hippocrates, Galen, Rufus, and Dioscorides, but knew well those of Ali, Avicenna, Averroës, Rhazes, and Damascenus, all of them renowned Arab mediciners and natural philosophers.

§ 14. The French language, as we have seen, soon came to distinguish between the physician and the professional healer of diseases. From *medicare* came the mediæval Latin verb *medicinare*, whence the French verb *médeciner* and the substantive *médecin*, corresponding to which we find in German and in English the substantive *mediciner*. Sir Walter Scott puts into the mouth of King Richard the words "It is unbecoming a mediciner of thine eminence to interfere with the practice of another"‡; and Jamieson gives a Scotch proverb, "Live in measure and laugh at the mediciners"§. It is to

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* Hippocrates 'On Airs, Waters, and Localities,' sections 1–8.

† Draper, 'Intellectual Development of Europe,' i. c. 13; ii. c. 4.

‡ 'The Talisman,' chap. xviii.

§ Jamieson's Scottish Dictionary has Medcinare, Medicinar, and Mediciner, meaning the practitioner of medicine, thus showing a derivation from the Latin verb *medicinare*, the second vowel being dropped in the first form.

be wished that this word were generally adopted in our speech, since the name of physician is now given to empirics who, whatever their claims to be curers, mediciners, or medicasters, have no right to be called physicians. The antagonism between the two schools is humerously shown in the old French quatrain cited in the note to § 10.

PART II.—*Philosophical.*

15. The term Physics and Physical. 16. Carpenter and Tyndall. 17. Thomson and Tait; Clifford; Dynamics and Dynamicist. 18. Chemism, theory of chemical changes. 19. The Chemical process defined. 20. The Unity of Force; universal Animation. 21. Organized Matter; Biotics. 22. Physiography and Physiology. 23. The Activity of Protoplasm. 24. Graham and Herbert Spencer on Colloids. 25. Barker on Vital Phenomena. 26. Biophysiology; scope of General Physiology. 27. Physiography; Huxley; Humboldt's Cosmos. 28. Physiophilosophy of Oken; Stallo. 29. Oken's system defined. 30. Physiographical and Physiological Botany. 31. Physiographical Mineralogy. 32. Structural Mineralogy and Mineral Physiology. Appendix: Newton.

§ 15. Having in the first part of this essay considered the words physic, physiology, and physician etymologically and historically, we proceed to notice them in their application by modern writers. We have already seen that the term physical science is often restricted to those phenomena which are common to organized and unorganized matter (§ 2). The study of these is now generally designated in didactic language as physics, or in French *physique*—the votary of such studies being called in English a physicist, and in French a *physicien*.

Physical, as an adjective, however, is used in a wider sense than the above when applied to organized beings. It then designates their organism and all pertaining thereto, as in the expression the physical life of man, or in the common tautological phrase "man's physical nature."

§ 16. While the word physic, or rather physics, is in modern English generally limited to the study of the phenomena of the inorganic world, the once synonymous term physiology has come to mean, both in English and in French, the study of the organic functions of plants and animals (and, by an extension of the term, that of the functions of the human mind), which are designated as physiological in contradistinction to the so-called physical phenomena of inorganic nature. Examples of these limitations respectively of the words physic and physiology and their derivatives are familiar to every reader. Thus, William B. Carpenter constantly distinguishes between physical, chemical, and vital forces, the consideration of the latter only, according to him, belonging to physiology[*].

On the other hand, we find well-known writers employing the word physical and its congeners indifferently in their wider and their more restricted meanings. Thus, in his address before the British Association for the Advancement of Science at Belfast, in 1874, Tyndall, in discussing the activities of the animal, speaks successively of "the work of the physicist, . . . the comparative anatomist, and the physiologist." Following this, the influence of the nervous system "over the whole organism, physical and mental," is spoken of; and a few lines further on, "the physical life dealt with by Mr. Darwin" is distinguished from "a psychical life;" while in the next paragraph we read of "organisms whose vital actions are almost as purely physical" as the coalescence of drops of oil suspended in a watery medium of the same density in the classic experiments of Plateau[†]. In the first citation the investigations by the dynamo-physicist of the nervous and muscular activities of the animal are distinguished from those of the biologist. In the second and third citations the physical organism and the physical life are distinguished, not as in the preceding, from the chemical and vital (which they evidently include), but from the mental organization and the psychical life; while in the fourth the antithesis is between physical, in the sense of dynamical, on the one hand, and chemical and vital processes on the other.

§ 17. Thomson and Tait, in their treatise on Natural Philosophy, wherein are considered only those simpler phenomena of matter which are neither chemical nor vital, employ the term *Dynamics* for the forces thus manifested, and divide the study of them into *Kinetics* and *Statics*, or the phenomena of actual motion and of rest. Some writers have used static as the antithesis of dynamic (see further, § 24); but statics, as implying simply equilibrium, are, as W. K. Clifford has well remarked, "but a particular case of kinetics," and hence are to be included with the latter under the common title of dynamics. Thomson and Tait consider under this head, besides the phenomena of ordinary motion, the vibrations which produce sound, and those motions by which we seek to explain the phenomena of temperature, radiant energy, and electricity and magnetism. The whole of the phenomena to which, in the modern and restricted sense, the name of Physics is generally applied, are thereby included

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[*] "Relation of the Vital to the Physical Forces," Philosophical Transactions, 1850, p. 727.

[†] Tyndall's Belfast Address, Appleton's ed., pp. 50 & 51.

under the head of dynamics—a term which is thus employed not only by the authors just cited, but by Clerk Maxwell, Helmholtz, and Clifford[*], and will be so used in the following pages, while the term *dynamicist* will replace physicist.

§ 18. Dynamics in the abstract regard matter in general, without relation to species, the genesis of which is the office of the chemical process or chemism. This gives rise to mineralogical, or so-called chemical, species, which, theoretically, may be supposed to be formed from a single element or *materia prima* by the chemical process.

"It is necessary to distinguish between the production of new species differing in physical characters[†] and that reproduction which belongs to organic existences. The distinction arises from that individuation which marks the results of organic life, and is eminently characteristic of its higher forms. The individuality, not only of the organism, but of its several parts, is more evident as we ascend the scale of organic life, while inorganic bodies have a specific existence, but no individuality; division does not destroy them. Crystallization is a commencement of individuation."

"That mode of generation which produces individuals like the parent can present no analogy to the phenomena under consideration; metagenesis, or alternate generation, and metamorphosis are, however, to a certain extent, prefigured in the chemical changes of bodies. Their metagenesis is effected in two ways—by condensation and union, on the one hand, and by expansion and division, on the other. In the first case, two or more bodies unite and merge their specific characters in those of a new species. In the second case, this process is reversed, and a body breaks up into two or more new species. Metamorphosis is, in like manner, of two kinds: in metamorphosis by condensation only one species is concerned; and in metamorphosis by expansion the result is homogeneous and without specific difference. The chemical history of bodies is a record of these changes; it is, in fact, their genealogy."

"The processes of union and division embrace by far the greater number of chemical changes, in which metamorphosis sustains a less important part. By union, we rise to indefinitely higher species; but in division a limit is met with in the production of species which seem incapable of further

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[*] W. K. Clifford, 'Essays,' ii. p. 17. This author, following the French usage, employed the substantive Dynamic in a treatise on the subject thus entitled; but the plural form, Dynamics, is preferable, as serving to distinguish it from dynamic used adjectively.

[†] That is to say, differing in dynamic relations.

division; and these, being regarded as primary or original species, are called chemical elements. These two processes continually alternate with each other; and a species produced by the first may yield, by division, species unlike its parents. From this succession results double decomposition or equivalent substitution, which always involves a union followed by division, although, under the ordinary conditions, the process cannot be arrested at the intermediate stage."

§ 19. I have quoted the three preceding paragraphs from an essay published by myself in 1853, on the Theory of Chemical Changes. Therein I also wrote, "Chemical combination is interpenetration, as Kant has taught. When bodies unite, their bulks, like their specific characters, are lost in that of the new species." In 1854, however, in an essay entitled 'Thoughts on Solution'*, I declared, with regard to Kant's view, that "The conception is mechanical, and therefore fails to give an adequate idea. The definition of Hegel, that the chemical process is an identification of the different and a differentiation of the identical, is, however, completely adequate. Chemical union involves an identification not only of the volumes (interpenetration mechanically considered), but of the specific characters of the combining bodies, which are lost in those of the new species. . . . We may say that all chemical union is nothing else than solution; the uniting species, are, as it were, dissolved in each other, for solution is mutual."

The above considerations will serve to show the essential nature of chemism, a process resulting in the genesis of chemical species, which are mineral or inorganic.

§ 20. The force involved in the chemical process manifests itself as radiant energy and electricity; and there is apparently a tendency among modern dynamicists to confound these activities with chemism itself, and thus to lose sight of the essential significance of the chemical process as already defined. Thus Clifford wrote of molecular motion, "which makes itself known as light, or radiant heat, or chemical action" †; while Faraday was wont "to express his conviction that the forces termed chemical affinity and electricity are one

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* Of the two essays above quoted, the first appeared in 1853, in the American Journal of Science for March, and also in the Philosophical Magazine [4] v. p. 526, and also, translated into German, in the *Chemisches Centralblatt* for 1853, p. 849. The second was published in the American Journal of Science for January 1854, and also in the Chemical Gazette for 1855, page 90. Both will be found in the author's volume of 'Chemical and Geological Essays,' in which, for the extracts here given, see pages 427, 428, and 450.

† W. K. Clifford, 'Essays,' ii. p. 17.

and the same." Helmholtz, from whom I here quote, adds:—"I think the facts leave no doubt that the very mightiest among the chemical forces are of electrical origin; . . . but I do not suppose that other molecular forces are excluded, working directly from atom to atom"*.

The activities which appear in dynamic and in chemic phenomena are one in essence; for force is one. The same is true of the activities manifested in organic growth, and even in thought; but the unity and mutual convertibility of different manifestations of force afford no ground for confounding, as some would do, dynamics with chemics, or with vital or mental processes. All of these phenomena are but the evidences of universal animation, or, in other words, of an energy which is inherent in matter, the manifestations of which, as matter rises to higher stages of development, become more complex, as organic individuals are themselves more complex than mineral forms.

§ 21. From the process which generates chemical species we pass to that which gives rise to organized individuals, in which appear a new class of phenomena, distinguished alike from those of dynamics and those of chemism. These new manifestations, which are called vital, involve dynamical and chemical activities, but display, in addition to these, still higher ones. Matter on this more elevated plane not only becomes individualized, but adapts itself to external conditions by organization, and exhibits in the resulting forms the power of growth by assimilation and of reproduction. The study of these forms in all their relations is the object of Biology. Organogeny, or the process of morphological growth and development, distinguishes the biological from the mineralogical individual. The activities of the crystal are purely dynamic; and its crystalline individuality must be destroyed before it can become the subject even of chemism; while the plant and the animal exhibit not only dynamical and chemical, but organogenic activities, which last are designated as vital phenomena. The study of these constitutes a third division of physics, which may be conveniently designated as Biotics (from *βιοτικὸς*, pertaining to life), and have to do with organic growth, development, and reproduction—activities which do not appear in the mineral kingdom.

Mineralogy is the science of inorganic matter, and studies its dynamical and chemical relations; while Biology, which is the science of organic matter, adds to these the study of biotic relations. The dynamic and chemic activities which

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* Helmholtz, The Faraday Lecture, April 5, 1881; abstract prepared by its author, 'Nature,' vol. xxiii. p. 539.

in the mineral kingdom give rise to the crystalline individual are therein in static equilibrium. The organic individual, on the contrary, is kinetic, and maintains its equilibrium only by perpetual adjustment with the outer world.

§ 22. General physic, or the study of nature, presents itself under a twofold aspect, the historical and the philosophical; the former gives rise to physiography, while to the latter the name of physiology more properly belongs. Physiography describes specific and individual forms and their external relations; while physiology investigates the processes by which these forms are produced, and gives us the logic of nature. The physiology of matter in the abstract is dynamic; that of mineral forms is both dynamic and chemic; while that of organic forms is at once dynamic, chemic, and biotic.

Nature in all its manifestations constitutes a unity; and it is the object of general physiology to study the process of creation in the material world from primal matter upward through its various forms until it attains to organization, and at length, in man, to self-consciousness, where the domain of physiology ends and that of psychology begins.

§ 23. In accordance with the views here enunciated, all matter is in a sense living, "all movement is radically vital"[*], though we, in common language, refuse the designation of vital to those lower forms of material activity which appear in dynamic and chemic phenomena, reserving it for such as are supposed to be peculiar to organized forms, which, to prevent misconception, I have called biotic. When matter, through chemism, attains the condition of protoplasm, which may be chemically described as a colloidal albuminoid united with more or less water, it begins to exhibit that form of activity which we term vital or biotic. "The mobility and the spontaneous movements of this substance," says Allman, "result from its proper irritability. From the facts, there is but one legitimate conclusion, that life is a property of protoplasm"[†].

§ 24. Many of the peculiar characters of protoplasmic matter appear to be common to chemical species in the colloidal condition. The remarkable properties exhibited by colloids led their discoverer, Graham, twenty years since, to declare, "The colloidal is, in fact, a dynamical [kinetic] state of matter, the crystalloidal being the statical condition. The colloid possesses *Energia*; it may be looked upon as the pro-

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[*] Stallo, 'Philosophy of Nature,' p. 66.

[†] Allman, Presidential Address before the British Association for the Advancement of Science, in 1879.

bable primary source of the force appearing in the phenomena of vitality. To the gradual manner in which the colloidal changes take place (for they always require time as an element) may the characteristic protraction of chemico-organic changes also be referred "[*].

Following Graham, Herbert Spencer has noted that pliability, elasticity, the power of absorbing water with change of bulk, and the phenomenon of osmosis, the whole of which are well designated by him as showing sensitiveness to external agencies which are mechanical or quasi mechanical—are possessed in common by mineral colloids and by organized substances. These phenomena are examples of that " continuous adjustment of internal relations to external relations " which characterizes organic life[†]. When the chemist shall have succeeded by his synthesis in producing a colloidal albuminoid having the same chemical constitution as protoplasm, there is, as Barker has well said, reason to expect that it will exhibit all the phenomena of life which appear in the protoplasmic matter common to plants and animals.

§ 25. Barker has, in this connexion, asked the important question, What are we to understand by organic life, and what is the true meaning of vital as applied to a function?[‡] If, with him, we answer, following Küss, " Life is all that cannot be explained by dynamics and chemism," we shall find, restricting our inquiries to the animal economy, that a large part of the phenomena commonly called vital, and, as such, included under the head of animal physiology, are dynamic or chemic. The law of the conservation of energy applies as rigidly to a living animal as to a thermic engine; and the amount of work done or of heat evolved is measured by food consumed in the former as it is by the fuel burned in the latter, the energy manifested in both cases being dependent on the oxidation of carbon and hydrogen. Recent inquiries go far to confirm the view that muscular contraction is electrical, and that electrical manifestation in the muscles is, as in our ordinary batteries, dependent on chemism. The tendency of late investigations is to bring nervous activity into the same category; and the electrical nature of capillarity has been shown by Draper and by Lippmann. The animal circulation is a

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[*] Thomas Graham, 'Chemical and Physical Researches,' p. 554, from Philosophical Transactions for 1861, p. 183.

[†] Herbert Spencer, 'Principles of Biology,' vol. i. part 1, chapters 1 and 2.

[‡] George F. Barker, Address as President of the American Association for the Advancement of Science, Boston, August 1880. I have in this paragraph closely followed Professor Barker's argument.

mechanical result of muscular contraction; the aëration and the coagulation of the blood, and the process of digestion, are chemical; while absorption finds an explanation in the phenomena of diffusion and osmosis.

When the energy which is in matter is manifested without reference to species, we call it simply dynamics; when it results in the production of mineral species, we call it chemics, or chemism; and when it gives rise to organisms, which may be defined as kinetic individuals, we distinguish it as vital, or biotic. In matter we must recognize with Tyndall, "the promise and the potency of all terrestrial life"*.

§ 26. It follows, from what has been said, that the word physiology, as popularly limited to the functions of living beings, is made to include many phenomena which are not biotic, but are common to the organic and mineral kingdoms, and that we need some further definition to distinguish those which are characteristic of organic life. I therefore venture to designate the study of these by the distinctive name of Biophysiology; while those phenomena which are recognized as simply dynamic, or dynamic and chemic, whether manifested in organisms or in mineral species, may be included under the name of Abiophysiology.

General physiology comprehending these two divisions, will thus be restored to its original and proper signification, as an inquiry into the reason of all things in the material universe, and as distinguished from physiography, whose province is the description of universal nature. Scientific precision demands a reform in our terminology, and requires us to extend the name of physiology once more to the processes and the activities of the three kingdoms of nature. The inorganic, not less than the organic, world has its physiology. On the other hand, the study of mind and spirit and the phenomena of consciousness, which Locke and Thomas Brown included under the head of physic and physiology, should be relegated to the domain of psychology.

§ 27. The kindred term physiography is now correctly employed in a general sense, with a meaning coextensive with that which we claim for physiology. A great living teacher, Prof. Huxley, has given us, under the title of 'Physiography, an Introduction to the Study of Nature,' an elementary treatise wherein, after describing the rocks, the waters, and the atmosphere, which make up the inorganic portions of the earth, he proceeds to consider the growth and development of

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* Address as President of the British Association, Belfast, 1874, (Appleton's ed.) p. 59.

plants and animals, and their relations to each other and to the mineral kingdom, and concludes with an account of the astronomical relations of our planet as a part of the solar system.

It was the conception of the essential unity of nature, without which a true science is impossible, which inspired Humboldt to attempt, in his *Cosmos*, a complete physiography, which was to be "a physical description of the universe, embracing all created things in the regions of space and in the earth." Humboldt elsewhere speaks of "the idea of vitality . . . . . . so intimately associated with that of the existence of the active, ever blending natural forces which animate the terrestrial sphere," and, recalling the fact that the inorganic crust of the earth includes the same chemical elements that enter into the structure of animal and vegetable organisms, adds :—"A physical cosmography would therefore be incomplete if it were to omit a consideration of these forces (and of the substances that enter into solid and liquid combinations in organic tissues under certain conditions), which, from our ignorance of their actual nature, we designate by the vague term of *vital forces*. The natural tendency of the human mind involuntarily prompts us to follow the physical phenomena of the earth through all their varied series, until we reach the final stage of the morphological evolution of vegetable forms, and the self-determining powers of motion in animal organisms"*.

§ 28. The necessary complement to a scientific physiography is thus, as Humboldt has here pointed out, a philosophy of the material universe, or, in other words, a general physiology. The most complete attempt at thus systematizing nature is that of Lorenz Oken, who divided all philosophy into Pneumatophilosophy and Physiophilosophy, corresponding respectively to Spirit and to Nature. Physiophilosophy, as defined by him, is the science of the conversion of spirit into nature, and has for its object to show how, and in accordance with what laws, the material universe has been formed ; to portray the first periods of the world's development from naught ; to show how the heavenly bodies and the chemical elements originated ; in what manner, by self-evolution into higher and manifold forms, these generated mineral species became at length organic, and in man attained to self-consciousness.

Physiophilosophy is therefore the generative history of the world, or, in other words, the history of the process of crea-

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* Humboldt's *Cosmos*, Otte's translation, Harper's edit. 1851, author's preface, p. viii, and vol. i. pp. 339–341.

tion. It aims, in the language of Stallo, to describe "the genetic evolution of the material world—therefore also its first origin in naught, and its subsequent development up to its limit, man, who is a complex of all preceding forms, includes all particular developments, and is, as it were, the focus where all the various tendencies of nature converge. . . . . In man all external activities, all divine ideas are gathered;" and thus it is that, in the words of the poet, he is enabled "to think again the great thought of the creation"*.

§ 29. The origin of matter itself, Hylogeny, belongs to Pneumatophilosophy. The genetic process in the primal undifferentiated matter, with which Physiophilosophy first concerns itself, is by Oken considered under the two heads of Ontology and Biology. The successive steps in the ontological process are:—first, Cosmogony, or the fashioning of the heavenly bodies from the previously formed matter; followed by the genesis therefrom of the chemical elements—Stoichiogeny. These elements give rise to mineral species, which together make up the earth—Geogeny. Biology, which has for its object the study of the organic world, is by Oken divided into Organogeny with its subdivisions, and Phytosophy and Zoosophy, treating respectively of the development of plants and animals. In the organism we have a "combination of all the activities of the universe in a single individual body." The inorganic and the organic worlds are not only in harmony with each other, but are one in kind. Man, in whom self-consciousness or spirit manifests itself, represents the whole universe in miniature †.

§ 30. The Physiophilosophy of Oken, of which we have given an outline, is thus identical in its aim and its plan with the earlier attempts of the Greek philosophers to which the

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* "Schön ist, Mutter Natur, deiner Erfindung Pracht
Auf die Fluren verstreut; schöner ein froh Gesicht
Das den grossen Gedanken
Deiner Schöpfung noch einmal denkt."
KLOPSTOCK, *Ode*; *Der Zürchersee.*

Compare this with the language of Schelling, cited by Hegel: "Ueber die Natur philosophiren heisst die Natur schaffen."

† Lorenz Oken, 'Physiophilosophy,' Introduction, pp. 1–3, of Tulk's translation, published by the Ray Society, London, 1847. See also an excellent analysis of the system by J. B. Stallo, in his 'Philosophy of Nature' (Boston, 1848), pp. 221–330, from which we have quoted above. Errors in detail, and defects and obscurities, are to be found in the system of Oken, which even novices in science can today point out and criticise; but it must not be forgotten that his Physiophilosophy has been a most potent influence in shaping and directing the scientific thought of the last two generations. Oken has been the inspirer and the teacher of the teachers of science.

name of physiology was given; and the two terms are in fact synonymous. The study of nature, as has been shown, divides itself into physiography and physiology; and this division applies equally to each one of the three great kingdoms of nature. Thus, for example, Physiographical Botany studies the relations of plants to each other as members of the vegetable kingdom, and investigates their external forms and relationships, by which we arrive at Systematic and Descriptive Botany with its classification and terminology. These together give us Botany as a great division of Natural History. Physiological Botany, on the other hand, considers the individual plant in itself, as seen in its structure, growth, and development, and in its relations to the other kingdoms of nature. It is properly divided into Structural Botany (which investigates the anatomy, organography, and morphology of the plant), and Vegetable Physiology (which studies the functions of the vegetable organism, its growth, nutrition, and decay, and the interdependence of the vegetable, animal, and mineral kingdoms)*. The same distinctions and definitions will apply, *mutatis mutandis*, to Physiographical and Physiological Zoology.

§ 31. The vastness and the complexity of the inorganic as compared with the organic world of nature, makes it difficult to grasp at once a conception of the true relations of Mineralogy, which comprehends the study of all forms of unorganized matter†. Physiographical Mineralogy, in its widest sense, has thus for its object not only this earth, but all other matter in space, and includes, so far as our planet is concerned, Geognosy and Petrography, besides Systematic and Descriptive Mineralogy as generally understood.

§ 32. In the study of Mineralogy in its physiological aspect, we have to consider the various conditions of mineral matter, distinguished as gaseous, liquid, or solid, as amorphous, crystallized in different geometric forms, or colloidal. These unlike conditions of matter, and their different relations to gravity, pressure, temperature, sound, radiant energy, electricity, and magnetism, the phenomena of capillarity, and of the occlusion, diffusion, and transpiration of gases and liquids, indicate structural, or, as we sometimes term them, molecular differences in mineral species, which make up what we must include under the title of Structural Mineralogy.

The changes of mineral species from one condition to another, and their transformations under the influences of

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* See Asa Gray, 'Structural and Systematic Botany,' Introduction.

† See the author, on "the Objects and Methods of Mineralogy," Chemical and Geological Essays, p. 453.

the agencies already noticed, including the phenomena of chemism which give rise to new species, make up together the dynamic and chemic activities of matter, which constitute the secular life of the planet. They are the geogenic agencies which, in the course of ages, have moulded the mineral mass of the earth, and from primeval chaos have evolved its present order, formed its various rocks, filled the veins in its crust with metals, ores, gems, and spars, and determined the composition of its waters and its atmosphere. They still regulate alike the terrestrial, the oceanic, and the aerial circulation, and preside over the constant change and decay by which the face of the earth is incessantly renewed, and the conditions necessary to organic life are maintained. To the study of these processes we may, with propriety, apply the name of Mineral Physiology *.

APPENDIX.

The doctrine of universal animation, or of an energy inhering in all matter, and that of the essential unity of all manifestations of material energy, whether in dynamic, chemic, or biotic phenomena, both of which we have maintained in the preceding pages, and especially in § 20, were held by Sir Isaac Newton, who ascribed such phenomena to the force of an immanent spirit. In the General Scholium which closes the third volume of the *Principia*, following his magnificent profession of Theism, he asserts the existence of a "most subtile spirit, pervading and latent in gross bodies," which spirit, by its force and activity, is the cause of gravitation, of cohesion (and consequently of chemism, which he elsewhere refers to peculiar aggregations of particles), of electrical attraction and repulsion, of the phenomena of light and of heat, and, finally, of all sensation, and of the power of motion in animal bodies†.

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* I have elsewhere made use of this term in speaking of the phenomena connected with the decay and transformations of silicated rocks, as belonging to "the domain of what I venture to call mineral physiology" (Canadian Naturalist, 1880, new series, vol. ix. p. 435).

† "Adjicere jam liceret nonnulla de spiritu quodam subtilissimo corpora crassa pervadente, et in iisdem latente; cujus vi et actionibus particulæ corporum ad minimas distantias se mutuò attrahunt, et contiguæ factæ cohærent: et corpora electrica agunt ad distantias majores, tam repellendo quàm attrahendo corpuscula vicina; et lux emittitur, reflectitur, refringitur, inflectitur, et corpora calefacit; et sensatio omnis excitatur, et membra animalium ad voluntatem moventur, vibrationibus scilicet hujus spiritûs per solida nervorum capillamenta ab externis sensuum organis ad cerebrum et a cerebro in musculos propagatis."—*Princip. Math.* lib. iii. Scholium Generale.

The same doctrine is enunciated, though in a less precise form, in 1675,

XXXI. *Molecular Attraction.* *By* FREDERICK D. BROWN, *B.Sc., Demonstrator of Chemistry in the University Museum, Oxford*[*].

AMONG those who are interested in chemical science, and especially those who are engaged in an attempt to advance that science by experimental work, there must be many who feel that, notwithstanding the immense number of new facts brought forward during recent years, comparatively little real progress has been made. Since 1857, when Kekulé first published his views concerning the internal construction of the molecules, chemists have been mainly occupied in applying his ideas to a multitude of carbon compounds. This work, although of the greatest importance, has led us perhaps too much into questions of detail, and has turned away our attention from the other great problems which were of so much interest to our predecessors. The chemical literature of the past decade is replete with information concerning the reactions undergone by complicated organic substances; but it rarely tries to tell us how and why these reactions take place. We diligently gather stores of stones for our building; and, this done, we are too often content to stand listlessly round the heaps waiting for the builder, and leaving even the work of the stonemason to the chances of the future. If, under these circumstances, any one tries, in however feeble a fashion, to arrange a few of the stones together, the work will

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in Newton's famous Hypothesis touching his Theory of Light and Colour, in his subsequent letter to Boyle in 1679, and also in the Queries 18–24 appended to Book III. of his 'Optics,' in all of which the various phenomena are ascribed to a supposed ætherial medium. From various considerations it appears probable that these Queries, though first published in 1717, were indited before the *Principia*, which was written in 1685–86, and published in 1687; while the General Scholium itself did not appear till the second edition of the *Principia*, in 1713.

In this connexion it may be noticed that the uncertain and obscure utterances found both in Newton's essay of 1675 and in the Queries to the 'Optics,' with regard to exhalations from the sun and other celestial bodies, were, after his studies of the comet of 1680, exchanged for the clearly-defined opinions in Propositions 41 and 42 of the third book of the *Principia*. In these it is maintained that the exhalations alike from the sun, the fixed stars, and the tails of comets, are not only diffused throughout all space, and finally reach the atmospheres of the planets, but that the matters thus conveyed are necessary for the maintenance of vegetable life, and contribute to the solid mass of the earth. These views were a remarkable anticipation of some of the conclusions announced by the present writer in 1878, and later in 1880, in an essay "on the Chemical and Geological Relations of the Atmosphere," for which see the 'American Journal of Science' for May 1880.

[*] Communicated by the Author.

at any rate be valuable in so far as the attention of others will be drawn to the possibility of the task; though the structure erected be soon thrown down, it will have set an example which may lead to other and more durable buildings.

Animated by these ideas, I venture to bring forward a small attempt to link together some of the properties of chemical substances, and to bring them under the general theories which we owe to Dalton, to Avogadro, and to Kekulé. All chemical hypotheses founded on the atomic theory must ultimately refer either to the form and structure of the atoms and molecules, or to the movements executed and the forces exerted by them. It is with the latter that we are now more immediately concerned; but we must first define exactly our notions of the former.

We all recognize that chemical substances are composed of molecules, and that these, in their turn, are built up of atoms; but if we step beyond this general statement, the same unanimity no longer prevails. With regard to the structure of the molecules, there seems to be an unwillingness to believe that the atoms are really arranged in the molecule in the manner indicated by our ordinary formulæ; yet it seems impossible to form any other conception concerning them than this purely concrete one. If an atom is combined directly with another, it must surely be contiguous to that other; and if several atoms are combined with one, they must, I imagine, be ranged round that one. Again, if a group of atoms passes unaltered through several reactions while the other portions of the molecule are replaced or broken up, it seems but reasonable to suppose that the atoms forming such a group are actually placed together in space. When, further, we learn from the researches of Captain Abney and of Dr. Russell that the existence of such groups of atoms is registered in the absorption-spectra of the compounds, that we can, as it were, see them in the molecule, we shall not take up an unsafe position if we assume that the ordinary formulæ indicate real place in the molecule. While making this assumption, I do not wish for a moment to imply that the atoms are arranged in that particular form which we are accustomed to give to our graphic formulæ, but merely to assert that the connecting-link of the formula indicates juxtaposition of the corresponding atoms.

It is now known with certainty that the molecular volumes of substances are but slightly altered by combination—that is to say, that under comparable conditions an atom of any substance generally occupies about the same space, with whatever atoms, similar or dissimilar, it may be combined.

This fact seems to me to point to the conclusion that the atoms which make up a molecule are as close together as their periodic motions will permit, and are not merely held in certain positions of equilibrium by various opposing forces; for if the latter supposition were true, I fail to see how it would be possible for the same atom, together with its proportion of space, to have always the same volume, surrounded, as it is, sometimes by atoms by which it is strongly attracted, and by which therefore its volume would be diminished, sometimes by atoms with which it has but little affinity, and which, therefore, would leave it plenty of room in which to execute its vibrations. The proximity of the several molecules in the liquid and solid states must also be assumed, in order to account for the invariability of molecular volumes.

The assumptions contained in the preceding paragraphs are in no way opposed to the views generally held concerning molecular and atomic motion which we owe to the development of the science of heat. They merely state that there is no force of repulsion exerted between contiguous atoms, and that the vibratory or other movements alone prevent their absolute contact.

Having thus fixed our ideas concerning the structure of molecules, we may pass to the forces exerted between them, neglecting the repulsion sometimes assumed, but which seems to be wholly unnecessary.

The theory of universal gravitation, as I understand it, asserts that the mutual attraction exerted by any two bodies A and B is dependent only on their respective masses and on the distance between them, being entirely uninfluenced by the presence of other bodies even in the immediate neighbourhood of A or B. Thus at a given moment the Earth and Venus, being in certain definite positions, exert upon each other a certain force of attraction; the attraction thus taking place between the masses of the two planets would be unaltered by the removal of the moon from the sphere of action; the gravitation of the earth and the moon does not therefore tie up any portion of the attractive energy of the earth, and so diminish the force with which other bodies gravitate towards it.

A totally different assumption is usually made with regard to that form of attraction which gives rise to chemical phenomena. Here it is supposed that two or more atoms, having combined together, have thereby become incapable, at any rate in the majority of cases, of attracting others to any appreciable extent. Thus I imagine that most chemists hold the view that when hydrogen and oxygen combine together to form water, they thereby exhaust, or nearly exhaust, their combining-power, that the power of attraction residing in the oxygen atoms is all concentrated upon the hydrogen atoms, just as we might conceive all the attractive power of the earth concentrated on the moon, thus leaving all other bodies in its neighbourhood free from the influence of gravity. We thus invest matter with two separate forms of attraction, differing entirely in their mode of action, and having indeed nothing in common.

Although there is no inconsistency in this, yet it would seem to be simpler to assimilate to a certain extent chemical attraction and gravitation. Let us suppose, then, that the act of chemical combination in no wise alters the power of attraction which the combining atoms exert upon surrounding bodies, and let us see what effect this hypothesis has upon the explanation of various phenomena.

That this assumption is not purely arbitrary is proved by the mere occurrence of any and every chemical reaction; for it is evident that no such reaction could take place unless the force of chemical affinity were exerted between atoms which are still held in combination in different molecules. Take, for example, any simple reaction, such as the neutralization of caustic soda by hydrochloric acid: when the molecules of sodium hydrate are brought into proximity with those of the acid, some of the atoms are interchanged, and molecules of water and of sodium chloride result. Now this can only take place in two ways: in the one we may imagine a constant spontaneous decomposition and recomposition of the several molecules to take place, so that there are in the alkaline liquid a considerable number of free atoms ready to act on other free atoms in the acid liquid; in the other we must allow that the force of affinity inherent in the chlorine atoms stretches across, as it were, to the sodium atoms in the sodium hydrate. Of the constant decomposition and recomposition necessitated by the first process of reaction, we have, at any rate in the majority of cases, no evidence whatever; on the contrary, the agreement of the observed vapour-densities of substances with those required by Avogadro's law renders such molecular instability extremely improbable; we are therefore driven to allow that affinity does act from molecule to molecule.

The hypothesis above indicated enables us to explain to a certain extent the relative volatility of substances, and goes far to render a satisfactory account of molecular compounds. We all, I presume, look upon the maximum vapour-tension of a substance at a given temperature as affording, to a certain extent, a means of estimating the attraction which its molecules exert among themselves: if there is considerable attraction, there will be a low vapour-tension; and with little attraction there will be a low boiling-point. It follows from this that the attraction between the molecules of hydrogen is relatively extremely small; that in the case of oxygen and nitrogen it is also very small, though probably much larger than in the former case; the attraction mutually exerted by molecules of chlorine will be more considerable; while with bromine, iodine, and other liquid and solid elements it will be greater still. We must not, however, confound the attraction exerted between *atoms* of a substance with that between the *molecules*; for each atom attracts separately those of the contiguous molecule, so that the attraction between two molecules of bromine, for example, will be four times as great as between two atoms; and generally when the molecule of a substance contains n atoms, the attraction between two molecules will be approximately n^2 times that between two atoms. This is of course even approximately true only when the distance between the two molecules is great relatively to their size; when the two molecules are close together, the several interatomic attractions will be exercised over very different distances, and will therefore be very unequal in amount. Nevertheless the above remark enables us to see that in some cases the apparent attraction, as estimated by the boiling-point, may be very misleading. In sulphur, for example, of which the molecule in the solid and liquid states is probably somewhat complex, we have a substance of high boiling-point, though the mutual attraction of the atoms may be comparatively small. The same is the case with carbon and many other substances.

Applying now the above considerations to a few actual cases, we shall see that the relative volatility of different substances is generally satisfactorily explained. Let us designate by $(h\,h)$ the attraction at unit-distance between two atoms of hydrogen, by $(o\,o)$ the attraction between two atoms of oxygen, and generally by $(r\,s)$ the attraction at unit-distance between any two atoms R and S. Then, in the case of water, the molecular attraction will be represented by

$$4\mathrm{A}(h\,h)+4\mathrm{B}(h\,o)+\mathrm{C}(o\,o),$$

where A, B, and C are factors dependent on the distances which separate the atoms. Now we have seen that $(h\,h)$ and $(o\,o)$ probably have small values; but $(h\,o)$ is not small: hence the attraction between molecules of water should be far greater than that between molecules of oxygen, and the boiling-point much higher, a result which is in accord with fact. The boiling-point of water would probably be much higher than it is were it not that the attractions between H and O are exerted over comparatively large distances, owing to the hydrogen of one molecule shielding its companion oxygen from the approach of other hydrogen. In the similarly constituted body, H_2S, the value of the molecular attraction will be

$$4A_1(h\,h)+4B_1(h\,s)+C_1(s\,s),$$

in which expression A_1, B_1, and C_1 may be supposed to have values not differing excessively from those which hold good in the case of water (the sulphuretted hydrogen being supposed liquid).

We have seen that the value $(s\,s)$ may possibly be small; and as the attraction is exerted between atoms which cannot approach each other very closely, C_1 is certainly small: hence this term will not probably much increase the molecular attraction. The first term will remain much the same as in the case of water; but the second term will have a much diminished value on account of the feeble affinity of hydrogen for sulphur: the whole value of the molecular attraction will therefore be much smaller than in the case of water; and we should expect sulphuretted hydrogen to be much more volatile than water, which is actually the case. With carbon dioxide it would seem, at first sight, that we have a substance which should be less volatile than water; for the heat evolved in burning carbon is greater than that given out by the combustion of hydrogen; $(c\,o)$ therefore is probably greater than $(h\,o)$; and as $(c\,c)$ is greater than $(o\,o)$, and $(o\,o)$ than $(h\,h)$, the whole value

$$4A_2(o\,o)+4B_2(c\,o)+C_2(c\,c)$$

would appear to be greater than the corresponding value for water. The researches of Kopp and, more recently, of Prof. Thorpe have, however, shown that the atom of hydrogen always occupies about the same volume, and that, if we take this volume as the unit, the atom of carbon occupies a volume 2, while one of oxygen, when combined with only one atom of carbon, occupies a volume 2·22. From these numbers we learn that two contiguous molecules of CO_2 must have their respective atoms of carbon more distant than the atoms of oxygen in two contiguous molecules of water; for the oxygen in the one case occupies a far larger volume than the hydrogen in the other: hence also the important attraction $(c\,o)$ is exerted over a much greater distance, a distance which is increased by the larger volume of the atom of carbon itself. The second and third terms of the above quantity are therefore greatly diminished, and the whole value may well be less than in the case of water.

In applying these arguments we must remember that the molecules are in constant motion about their centres of gravity. In the case of CO_2, for example, the direction of the line joining the centres of the carbon atom and one of the oxygen atoms varies continuously; hence the molecule of CO_2, as regards the action on other molecules, may be regarded as practically in the condition of a centre of carbon surrounded by a shell of oxygen.

Among inorganic substances there are but few which take the form of volatile liquids, and these are mostly isolated instances ill-suited for comparisons of this kind. It is therefore to the homologues and isomers of organic chemistry that we must chiefly look for a confirmation of these views. In the alcohols of the ethyl series, for example, the larger the molecules the greater must be the attraction between them, and consequently the higher the boiling-point; this, as is well known, is in accordance with fact. In the case of isomeric alcohols, the influence of the position of the atoms comes conspicuously to the fore. It is clear that if the atoms of carbon of two different molecules cannot approach each other so nearly in the case of one isomer as in another, the attraction between the molecules will be less, and the boiling-point consequently lower. Now in secondary and tertiary alcohols the carbon atoms are more sheltered by each other, are, as it were, more removed from the exterior of the molecule than in primary alcohols; at the same time the boiling-points are lower, which is in accordance with our hypothesis.

If we replace two atoms of hydrogen in an alcohol by one of oxygen, we increase the attraction of the molecules, since we substitute a certain number of attractions $(h\,o)$ and $(c\,o)$ for the relatively small attractions $(h\,h)$ and $(c\,h)$: the increase of boiling-point which we should expect is confirmed by experiment.

If, on the other hand, we replace the hydroxylic hydrogen of an acid by methyl or ethyl, we lower the boiling-point, although we increase the number of attractions exercised between the molecules. But we must remember that by this substitution we combine a considerable number of atoms with that carbon to which is annexed the oxygen of the molecule, thus causing a more even balance of the molecule about that carbon, and throwing it therefore more into the centre of the system. The attraction $(h\,o)$ to which the total molecular attraction is so largely due will therefore be exerted over greatly increased distances, and the boiling-point will be to a like extent diminished. That this argument is founded on fact is, I think, shown by a host of other instances: thus, ethyl-methyl ether, CH_3—O—C_2H_5, boils at 11°; but ethyl alcohol, H—O—C_2H_5, boils at 78°; in this latter substance the oxygen is partly exposed by the substitution of H for CH_3. Again, in ethyl butyrate, C_2H_5—O—CO—CH_2—C_2H_5, and in propyl propionate, C_2H_5—CH_2—O—CO—C_2H_5, it is evident that the oxygen atoms occupy in each case an equally central position, while the total number of atoms is the same. We might therefore expect that the boiling-points of the two substances would not be far apart. Experiment shows that the boiling-point of the butyrate is 121°, that of the propionate 122°. In the isomeric substance butyl acetate the oxygen atoms are no longer so central, and the boiling-point rises to 124°·3; while in valeric acid, in which the oxygen atoms may be viewed as occupying an outside place in the molecule, the boiling-point rises above 180°. As another example of the reduction of the total molecular attraction when the oxygen occupies a central and sheltered position, we may take ether, C_2H_5—O—C_2H_5, boiling at 35°. If it be true that in this case the attractions ($h\ o$) are greatly diminished by the distance over which they are exerted, it is clear that for O we may, without much general effect, substitute CH_2 or some other group of atoms of similar weight: this substitution produces pentane; and we find that this is a body having a boiling-point differing but little from that of ether. Many other examples might be brought forward, were it not that their discussion would transcend the limits of this paper.

The use of the above hypothesis renders it difficult, at first sight, to account for the formation of definite chemical compounds. It seems that if any number of atoms of hydrogen are equally attracted by one of chlorine, the combination of one of them with that atom would not prevent the adherence of a second and a third forming H_2Cl, H_3Cl, &c. This difficulty is avoided by supposing that the chlorine atom is of such a form that only one atom of hydrogen can approach sufficiently closely to adhere permanently; such forms are difficult to imagine, though it may be remarked that an atom in the form of a ring offers in a certain sense a unique position to another which instals itself inside it. The existence of molecular compounds proves that the permanent adherence of other atoms is sometimes possible, and thus affords material support to the notion that the chemical affinity of an atom is not only exerted upon those atoms with which it is combined, but upon all others in its vicinity.

XXXII. *On the Dynamical Theory of Radiation.* *By* ARTHUR SCHUSTER, *Ph.D., F.R.S.*[*]

IF we attempt to draw up a summary of our knowledge on the radiation of heat and light, we shall find that it has been derived from two distinct lines of investigation. The first of these, originated by Prevost of Geneva, has been followed up by Balfour Stewart and Kirchhoff. It is founded on the experimental fact that bodies placed in an enclosure of uniform temperature will ultimately assume that temperature, and on a law called, after its inventor, Prevost's law. The characteristic distinction of this line of investigation, as opposed to the second one, presently to be noticed, consists in the fact that it makes no assumption whatever on the nature of radiation. Whether the constitution of the body be molecular or not, it claims to found some important laws, laws which have a wide practical application, on experimental facts simply, and one or two assumptions against which no serious objections have as yet been urged. The second line of investigation to which I have referred starts from the assumption of the existence of atoms and molecules. It tries to form ideas, more or less hypothetical as they are more or less distinct, on the nature of these molecules, and endeavours to fit the observed phenomena of nature into the results of an almost pure deductive reasoning. The first method may be considered more satisfactory and philosophical; but the second is more productive, and any further advances which the science of radiation may be expected to make must be derived from the dynamical considerations on which it is founded. At present it may be useful to compare the results already obtained with observed facts, and to point out where a further examination of the assumptions which had to be made seems necessary.

In the first place, it is to be remarked that nearly all the investigations confine themselves to the consideration of an enclosure of uniform temperature—that is, to the state of the body in which it neither gains nor loses heat. It is hardly necessary to remark that no observations can be carried on in such an enclosure. If, at the ordinary atmospheric temperature, we shut all windows and doors so as to keep out all radiations which might directly or indirectly reach us from our luminary, observations on radiation and absorption will be prevented by our inability to see. But even if we should raise the temperature of the room and every thing it contains

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[*] Communicated by the Author, having been read at the York Meeting of the British Association.

to the point at which bodies become white-hot, we should not be better off. For our retinal and crystalline lenses would be white-hot too; and supposing they could sustain such a temperature, all parts of the retina would radiate alike; consequently they would also receive the same amount of radiation: the whole internal radiation being constant, there would be nothing to distinguish one part of the retina from the other, and we should be as effectively (perhaps more effectively) blind as at the ordinary temperature.

No observations being possible in an enclosure of uniform temperature, we must try to adapt our theoretical conclusions to cases in which we can subject them to the test of experiment. We do this by making an assumption contained in Prevost's law, and adopted by all subsequent writers. The first part of Prevost's law says, that bodies are always both radiating and receiving heat; if their temperature remains constant, as it does if they are placed in an enclosure of constant and uniform temperature, it is because they radiate just as much heat as they receive. The second part of Prevost's law, which is always implied though not always distinctly stated, says that the radiation of a body is a function of its temperature only. Assuming, with Prevost, that a body of given temperature radiates in the same way whether placed in an enclosure of uniform temperature or not, we can apply the results obtained by theoretical reasoning to the phenomena which we observe every day. But if some of these conclusions seem to be contradicted by experience, we are led to inquire a little more closely again whether really the radiation of a body is completely defined by its thermometric temperature, without regard to the fact whether, on the whole, it is gaining or losing heat. In Prevost's time men of science had nothing to guide them, and Prevost's assumption was the simplest, as it was the only natural one. But we may well discuss whether the dynamical considerations on which the molecular hypothesis is founded tend, or do not tend, to support the view that the radiation of a body is only a function of its temperature.

In the first place, let us consider a case, not connected with radiation, where the temperature alone certainly does not completely define the state of a gas at a given point. Imagine at first, again, an enclosure of uniform temperature, and fix your attention on a certain element of space in that enclosure. The molecules of a gas are moving about within that space with varied velocities; but, on the whole, as much energy passes the element in one direction as in another, and the average *vis viva* of the molecules is therefore independent of direction. But supposing now some parts of the enclosure to be hotter than others, conduction of heat takes place; and although the thermometric temperature of the element we are considering may be the same as before, yet the molecules will move more quickly in one direction than in another, and the temperature of the element, without regard to the temperature of the surrounding elements, will not indicate the direction in which the molecules move fastest. The temperature at a given point is only an average indication, and only completely defines the state of the gas if the temperature is uniform. We should be wrong to assume that, because a certain distribution of velocities holds when the temperature is uniform, it must also hold when conduction of heat is allowed to take place. What holds in the case of conduction of heat may also hold in the case of radiation; and we cannot assume, without further proof, that the radiation of a body is independent of the temperature of the surrounding bodies. It admits, on the contrary, I believe, of proof that the radiation of a body into a cooler medium, being a phenomenon of dissipation of energy, involves altogether different considerations from those we are accustomed to apply when the temperature does not change. In order to fix the attention, imagine a Bunsen burner with a soda bead in it. The spectroscope will show us the well-known yellow sodium-lines; but we know that the sodium-molecule is also capable of sending out certain rays in the green part of the spectrum*; and these rays are so weak in the case under consideration, that they escape observation. Following backwards the rays which enter the spectroscope, we finally reach the internal radiation of the Bunsen flame; and we may therefore say that the internal radiation is strong for a certain set of yellow vibrations, and weak for a certain set of green radiations. About the vibrations of the molecule the spectroscope does not directly reveal any thing; but there must be some relation between them and the intensity of radiations of the surrounding medium. If there is more internal radiation for the yellow light, it can only be because the vibrations of the molecules for the yellow light are stronger. Supposing, now, the flame to be suddenly placed in an enclosure of constant temperature, which temperature shall be the same as that of the flame itself. The internal radiation is constant within that enclosure; or rather it only depends on the refractive index of the medium.

For the yellow light we should find that the internal radia-

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* I do not wish to imply any definite opinion on the sodium-spectrum and its changes with temperature, but only take a certain view of it as an illustration.

tion in the enclosure is very nearly the same as that of the flame radiating into space; and consequently a removal of the flame into the enclosure will not materially affect the average energy of vibration for yellow light. But the internal radiation for the green light being much stronger in the enclosure of uniform temperature, the energy of vibration of the medium within the flame will be much increased by the removal of the flame; and this cannot take place without a corresponding increase in the energy of vibration of the molecules themselves. It follows from this that the difference in the intensity of the yellow and green vibration of sodium must be much smaller in the enclosure of uniform temperature than when the sodium-flame is allowed to radiate into space, though the temperature in both cases may be the same.

The following suggestion as to the genesis of radiation is founded on the ideas of the molecular theory of gases, and may serve to make the difference between the molecular vibrations in the case of a radiation into a colder space as distinct from a radiation into an enclosure of equal temperature a little more clear. In a gas which is neither gaining nor losing heat we assume a constant interchange of motion between different molecules.

Let us assume that the molecules we are considering are like the sodium molecules, capable of vibrating in two distinct periods, one corresponding to the yellow and one to the green rays. The energy of vibration for each molecule is constantly changing ; the translatory energy of motion is, by means of the encounters, transformed into vibratory energy corresponding to one or to the other period; and if, on the whole, the average energy remains constant, it is because just as much vibratory energy is transformed into energy of translatory motion. We may imagine that this energy of translatory motion is more easily transformed into yellow vibrations than into green vibrations; but in that case the energy of yellow vibrations must also more easily be transformed into translatory motion. The mathematical calculations of Maxwell and Boltzmann have shown that when the final equilibrium has been reached the final energy of vibration for the two periods must be the same, though each encounter may more easily produce vibrations of one period than of the other. This theoretical conclusion is not altogether confirmed by experience; but we may, for the sake of argument, assume it to hold in the imaginary case which we are at present considering. Imagine now the gas to be taken out of the enclosure and allowed to radiate into space. During the first instant the yellow and the green vibrations will be of the same intensity; but very soon the molecular encounters will show their influence. We have assumed that yellow vibrations are more easily produced than green vibrations. While, therefore, the green vibrations are allowed to decrease in intensity, the intensity of the yellow rays is constantly renewed by the encounters. The radiation of a body is a phenomenon of dissipation of energy; and the relative intensity of the different radiations merely show through what channels the energy is more quickly dissipated. The rate at which the energy of translation is transformed into energy of periodic motion (which has, according to Boltzmann, nothing to do with the establishment of final equilibrium in the enclosure of uniform temperature) is the most important quantity in determining the intensity of vibration when a body is losing heat. The mere fact, therefore, that we observe lines of different intensities in the spectra of bodies is not in itself sufficient to invalidate Boltzmann's theoretical conclusion that in an enclosure of uniform temperature the vibrations ought to be equally strong.

It will be seen from what precedes that I have adopted a view on the origin of the different intensities of spectral lines which is different from that usually given; but it is, as far as I can see, the only one consistent with dynamical principles. The statement that the spectrum of a body is not independent of the vibrations of the surrounding bodies may sound improbable; but it will, I think, be found to follow from any dynamical explanation of radiation. But though the mere fact of different intensities of spectral lines need not be conclusive against Boltzmann's conclusions, another argument is much more so. If his reasoning is correct we can, from the measurement of the two specific heats, deduce the number of degrees of freedom of a molecule. This number is very small, while we know, by spectroscopic observation, that it must, in fact, be infinite. Those who have observed the complicated spectrum of mercury, for instance, will find it hard to believe that a molecule of mercury has only three degrees of freedom, that is, that it can only move about in space; that it does not vibrate nor rotate. No suggestions which have yet been made seem to me to get over this serious difficulty; and as the reasoning by means of which the result in question has been obtained does not seem open to serious criticism, some of the fundamental assumptions on which the reasoning is based require to be carefully scrutinized. Now it seems to me that in the course of the whole investigation the vibrations of the molecules themselves, as far as their mutual influence (which must take place in consequence of the transmission of these vibrations through the medium) is concerned, have been overlooked. It is therefore perhaps not astonishing that the result is not, on this very point, in accordance with experimental fact. While molecules are in close proximity, forces must act which depend on their vibrations; and the relative phase of vibrations will be an important factor in the determination of the strength and sign of these forces. As the molecules approach each other they try to place themselves in unison; that is to say, the vibrations of those already in unison will be strengthened, while the vibrations of those the phases of which are in disagreement will be weakened. At the moment of the shock a sudden alteration in phase may take place ; and observations on natural light seem to show that it does take place. If this alteration takes place according to fixed laws, the forces acting between the molecules during the second part of the shock will be altogether different from those acting while the molecules are approaching. Boltzmann assumes that all the phenomena of an encounter take place as often in a reverse order ; but if forces such as those suggested act, this need not be true.

XXXIII. *On Logical Diagrams for* n *terms. By* ALLAN MARQUAND, *Ph.D., late Fellow of the Johns Hopkins University*[*].

IN the Philosophical Magazine for July 1880 Mr. Venn has offered diagrams for the solution of logical problems involving three, four, and five terms. From the fact that he makes use of circles, ellipses, and other curvilinear figures, the construction of diagrams becomes more and more difficult as new terms are added. Mr. Venn stops with the five-term diagram, and suggests that for six terms "the best plan would be to take two five-term figures."

It is the object of this paper to suggest a mode of constructing logical diagrams, by which they may be indefinitely extended to any number of terms, without losing so rapidly their special function, viz. that of affording visual aid in the solution of problems.

Conceiving the logical universe as always more or less limited, it may be represented by any closed figure. For convenience we take a square. If then we drop a perpendicular from the middle point of the upper to the lower side of the square, the universe is prepared for a classification of its contents by means of a single logical term.

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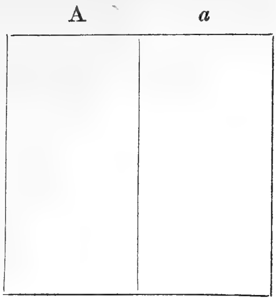
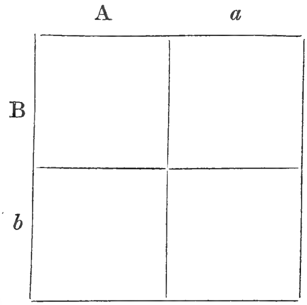
[*] Communicated by the Author.



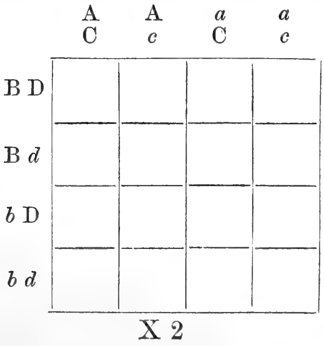
This represents a universe with its A and not-A "compartments." The quantitative relation of the compartments being insignificant, they may for convenience be represented as equal.

The introduction of a second term divides each of the existing compartments. This may be done by a line drawn at right angles to our perpendicular and through its centre, thus :—



The four compartments represent the sub-classes A B, A *b*, *a* B, *a b*.

A diagram for four terms would require two more perpendicular and two more horizontal dividing lines, thus :—



The sixteen compartments here represent the formal division of a universe into the classes A B C D, A B C *d*, A *b* C D, &c.

Thus by continued dichotomy we may reach a diagram for any number of terms. A diagram for *n* terms, if *n* be any even number greater than 2, requires $2+2^2+2^3+\dots+2^{\frac{n}{2}}$ dividing lines; diagrams for $n-1$ terms require $2+2^2+2^3+\dots 2^{\frac{n}{2}}-2^{\frac{n}{2}-1}$ such lines.

As the number of terms increases, the labour of writing out a quantity of letters may be considerably lessened by the use of brackets. This will appear in the solution of the following problem.

The are eight arguments, A, B, C, D, E, F, G, H, thus related to each other:—When E is true, F is true; and when F is true, either E is true or B and C are both false. When either G is true or E and F are both false, D is true. If B is false when either F or G (but not both) are true, then H is true and either C is false or D true. It is true only when an even number of the remaining arguments are true; it is false only when an odd number of the remaining arguments are false.

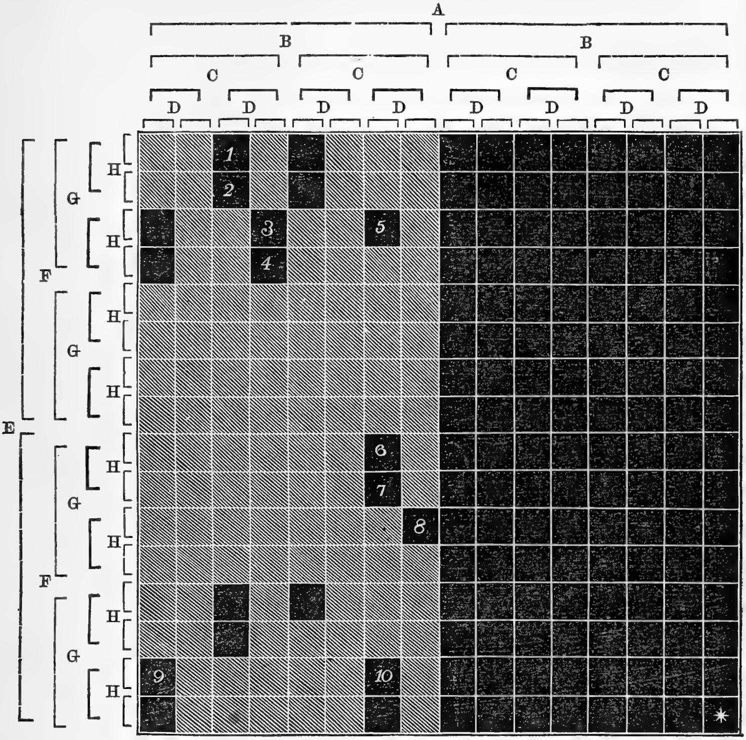
Supposing any combination not inconsistent with the premises to exist, (1) What follows from A being true either when B is true and D false or C false and F true? and (2) From what combination of arguments may we conclude that A and H are both true when E and G are both false?

Representing truth by a capital and falsity by a small letter, all possible combinations of the truth or falsity of the eight arguments are indicated by the small squares in the following diagram.

The shaded squares indicate the A combinations which are inconsistent with one or more of the premises; the non-A combinations, not being required in the conclusion, may be neglected. The first part of the conclusion calls for the eight combinations numbered 1 to 8 on the diagram. These are:—

A B *c* D E F G H
A B *c* D E F G *h*
A B *c* *d* E F *g* H
A B *c* *d* E F *g* *h*
A *b* *c* D E F *g* H
A *b* *c* D *e* F G H
A *b* *c* D *e* F G *h*
A *b* *c* *d* *e* F *g* H

and one or other of these is true. It will be observed that in all the combinations C is false and F true. Many subordinate conclusions may be drawn, such as A, B, and D being all true, E, F, and G are also true; A being true when B and G are both false, H is true, &c.



The second part of the conclusion calls for the three combinations marked 8 to 10 on the diagram. These are

A *b c d e* F *g* H
A B C D *e f g* H
A *b c* D *e f g* H.

Hence from either B C D *f* or *b c* D *e* or *b c d e* we may conclude A *e g* H.

Attention may be drawn to the fact that these diagrams differ from those suggested by Mr. Venn in having a compartment for the *absence of all* the characters or objects. Thus in the 8-term diagram the compartment marked with a star stands for the combination *a b c d e f g h*. This compartment may need to be shaded out, and hence should be indicated on a complete logical diagram. This is parenthetically acknowledged by Mr. Venn ('Symbolic Logic,' p. 271), and practically in the construction of his logical-diagram machine ('Symbolic Logic,' p. 122).

I have constructed a logical-diagram machine on the basis of these diagrams. Blocks, corresponding to the squares or rectangles on the diagrams and resting on two sets of slides, the one set below and at right angles to the other, may be made to drop when the combinations which they indicate are inconsistent with the premises. 32 such slides are required to operate the 256 blocks of an 8-term machine.

But this machine, though simple enough, is practically inferior to the diagrams which I have had printed at a trifling expense for problems of 7, 8, 9, and 10 terms.

XXXIV. *On the History of the Theory of the Beats of Mistuned Consonances.* By R. H. M. BOSANQUET, *Fellow of St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN a paper recently published in the Philosophical Magazine, I have given an account of my recent experiments on an important branch of this subject. I wish to contribute further to the appreciation of the bearing of my results, by means of a critical notice of a few of the most important points connected with the history of the subject; to which I propose to add some remarks on König's recent paper (*Annalen der Physik und Chemie*, 1880, p. 857).

Passing over the early history of the subject, we come to Smith's 'Harmonics' (1759), a treatise always regarded as important, but difficult. Smith was mainly concerned with the reduction of the phenomena to rule, for the purpose of his studies of musical temperament. His fundamental principles do not really reach beyond the numerical or geometrical appearances or patterns which arise from the superposition of certain sequences of forms at regular intervals. Causal explanation, reference to laws of sensation, analysis of the functions of the ear, were not attempted by Smith. But his work is of great importance. Is is still much appreciated as the first and, in one sense, almost the most powerful exposition of principles still largely held, which are entirely incompatible with the point of view originated by Helmholtz, or its developments.

Freed from the singular phraseology in which Smith's propositions are enveloped, they are not difficult in themselves. I will shortly state in an example the effect of his reasoning, so far as it purports to deal with the causes of beats.

Smith's 'Harmonics,' prop. x. p. 81, 2nd edit.

We will call a note having 100 vibrations in a certain time a "unison;" and a note having 101 vibrations in the same time will be said to make with the first an "imperfect unison." Then the time in question, in which the notes make respectively 100 and 101 vibrations, is what Smith calls a "simple cycle," or "the period of the imperfect unisons."

Consider notes making 2 and 3 vibrations respectively in the time of 6 of the first note, or "unison." Then the time in which these notes make 2, 3, and 6 vibrations respectively is what Smith calls the "short cycle" of the consonance.

Suppose the consonance to be mistuned, so that 2 vibrations of the one note = 6 of the unison (100), but three of the other = 6 of the imperfect unison (101), then the 2 of the one note still very nearly = 3 of the other, and each of these periods or short cycles is still nearly 6 vibrations both of the unison and the imperfect unison. Then Smith says:—

"Take away the greatest equal numbers of short cycles (of 6) that can be taken from both ends of the simple cycle, or period of the imperfect unisons."

(Take away two sets of 8 short cycles of 6 from 100 and 101 respectively.)

"Then some part of another short cycle or two, as consisting of unequal numbers of the quicker and slower vibrations of the imperfect unisons, will always remain in the middle of the cycle or period."

($100-2\times8\times6=4 . 101-2.8.6=5$. So that 4 and 5 vibrations respectively are the parts of the short cycles that remain over.)

"And this part, by interrupting the succession of the short cycles" (of 6) "wherein the harmony of the consonance consists, interrupts its harmony, and causes the noise which is called a beat; especially as the interruption is made where the short cycles on each side of it are the most imperfect and inharmonious. Therefore the time between the successive beats, made in the middle of each period or simple cycle of the pulses of the imperfect unisons, *or of the least imperfections of the consonance*, is equal to the time of the period."

The term *period of least imperfections* is explained by noticing that there would not be a real coincidence of the two notes at the end of the period; but there would very nearly be one.

This reasoning only points out the mode of calculating the number of times that a certain form of grouping is repeated. So far it is quite right; and as forms of grouping will in this subject always necessarily be a matter of interest and importance, the modern student cannot afford to dispense with Smith's most powerful exposition. But it does not account for our modern conception of beats, regarded as variations in the intensity of certain notes. We have still to find out in what way such variations of intensity arise out of the system of recurrences Smith describes.

With respect to the supposed difficulty of Smith's propositions, I am confident that any one who has mastered the above example will find the difficulties gone, in all that part of the work which is concerned with the doctrine of beats.

Young's criticism on Smith's work is, that he achieved nothing. De Morgan thinks that Young, and others whom he quotes, did not understand Smith. The passage of Young which De Morgan cites as a proof of this, however, is very good sense, and undoubtedly represents the fact correctly on Young's own theory.

Whether Young understood Smith or not, and whether or no Young's theory will now bear strict criticism, there can be no doubt that Young's work had a most beneficial influence on the progress of the subject. He is remarkably clear about a number of important points ; and on some his exposition is not surpassed at the present day.

Young clearly pointed out the mode in which the ordinary beats (which become Smith's short cycles in or near consonances) are numerically related to the Tartini tones (combination-tones). He uses a phraseology which is now seen to be objectionable, when he says that the beats *become* the Tartini tones. This would involve, in our quantitative language, the statement that the whole of the energy of the beats is transformed into combination-tones, to which statement it is impossible to assent. So that I differ from Young's exposition as I differ from that of König, where he assumes the passage of beats of great frequency into a note, and compares the beats with impulses, as if they contained nothing else but the impulse. But this difference of view will only touch the mode in which the notes arise out of the beats. One might almost say that it is a mere question of language ; but it is not so. The element of quantitative transformation is essentially involved.

The facts, however, are of the first importance ; and it is not too much to say that Young's statements fully anticipate the principal conclusions of König's great paper, and give the facts completely and compendiously. It is no use attempting a detailed analysis of Young's position, as it is substantially the same as that of König.

Of the other writers who occupy a similar position we need only mention De Morgan, from whose analysis many have formed their idea of Smith's work. From our present point of view we admire his painstaking appreciation of Smith, while we think he overestimates the value of Smith's analysis of beats. He does not seem to see that it amounted only to a numerical or geometrical analysis, and left the reader a long way off even the idea of the existence of a deeper-seated explanation. The obstacle opposed to Smith's explanation by the resolution in the ear of the resultant or complex forms into their elements according to Ohm's law, escaped De Morgan entirely.

De Morgan is behind Young in thinking that Tartini (combination) tones only exist in the case of consonances; this vitiates fundamentally all his notions of the phenomena, and causes him to give defective rules for them. Further, he condemns Young for the statement that other resultant tones (as we now call them) arise out of the beats of consonances besides the Tartini tone (difference-tone of Helmholtz, first combination-tone). In fact Young here clearly describes the phenomena subsequently described by König as beat-notes, and was right in the main as to the facts.

I think we may fairly summarize the position taken up by Helmholtz on this question as amounting to the admission of the principle—*all beats consist of variations of intensity of musical notes.* I am not quite sure whether Helmholtz was the first to enunciate principles which amount to this, as the history of these doctrines is voluminous and rather obscure. And I do not think that even Helmholtz enunciates this as a new dogma in so many words, though it is assumed throughout as the basis of his treatment. Some of the older writers, particularly Smith and Young, so guard their exposition that it is quite capable of adaptation to this principle. On the other hand, certain modern writers, professing or appearing to follow in the footsteps of Smith and Young, so modify their exposition as to make it inconsistent with the above principle, and to require the admission that beats consist of some form of perception other than that of the variation of musical notes.

In treating of the position of Helmholtz, it will be desirable to discuss somewhat minutely a treatise on Beats, consisting of two papers by W. Pole, F.R.S., Mus. Doc., in 'Nature,' Jan. 13 and 20, 1876. I regard these papers as the most important manifesto of the school last alluded to, before the paper of König to be subsequently considered. The clearness and knowledge of the subject make them a good text for discussion; yet I consider the exposition they contain to be erroneous.

Pole describes three kinds of beat—the unison-beat, the consonance-beat or Smith's beat, and the beats of Helmholtz, which are taken to include the beats arising from overtones and combination-tones.

The account of the unison-beat is in most respects that common to all authors. But there is a point in which it differs from the carefully-guarded exposition of Smith, which gives rise to a fallacy running throughout the whole paper, though distinct from my principal ground of objection. The whole exposition is founded on effects supposed to be produced by the blows of a hammer. Now this makes a very complicated effect the basis of operations. In the first place, we have in the primary impulse a sound rather than an impulse of the air. This example then involves the question of the production of notes by intermittent sounds, which belongs to a higher and more difficult part of the subject. Secondly, the impulse selected, regarded as an impulse, is of a very complicated character. It can be shown without difficulty that a series of discontinuous impulses is represented by the sum of an infinite series of impulses following the pendulum-law, the amplitudes of all the terms being ultimately equal.

Consider more closely the example given by Pole. Suppose A to make eleven strokes of the hammer to ten of B. Then, if at one point the strokes of A and B coincide, after five strokes of B, A will be half a stroke in advance, and the strokes will succeed each other with intervals of half a stroke for a short time; that is to say, the pitch of the resulting sound goes up an octave. Now if two pure tones near each other in pitch produce the unison-beat, the beat consists of alternations of sound and silence very nearly—at all events not of alternations of fundamental and octave. Notes are easily procured of sufficient purity for this demonstration. Consequently Pole's illustration fails to indicate the real nature of the phenomenon.

Smith is careful to avoid this difficulty; and his demonstrations are perfectly applicable to pendulum-vibrations, although he makes no assumption as to the forms of the impulses employed.

The explanation given by Pole of the effect of imperfect coincidences is quite correct; but it is attributed to Young. Now this explanation was fully attained by Smith, and is the origin of the term "period of least imperfections," which is one of the most perplexing elements of his phraseology.

In alluding to the disappearance of unison-beats in intervals greater than a third, Pole notices Young's theory that the beats pass over (? are entirely transformed) into the Tartini tone. But in speaking of the doubt cast by Helmholtz on Young's theory, Pole fails to notice the real nature of the discovery of Helmholtz, which is that the two notes are separately received by the ear; so that the disappearance of the beats is to be regarded as arising, in great measure at least, from the removal of the two disturbances from each other's influence, and not entirely or even principally from their rapidity. I have dealt with this point in my recent paper (Phil. Mag. June 1881, p. 422).

Pole's second kind of beat is Smith's beat (variation of maximum displacement in resultant forms). It is assumed that the theory of Smith constitutes a complete explanation of the existence of certain beats in mistuned consonances, as to the nature of which nothing further is said. Smith's investigation has been sufficiently given for its nature to be fully realized. It will be seen that Smith does not say any thing as to what the ultimate nature of the beat is; he only examines the superposition, and says, "wherever there is a certain irregularity there must be a beat." The classification of beats given by Pole appears therefore to import into Smith's investigation, *sùb silentio*, something not really contained in it—namely, the implication that the beat thus demonstrated is something *sui generis*, and different in kind from the variation of a musical note. For all beats formed by the variations of musical notes appear to be referred to Pole's third class, viz. the beats of Helmholtz. But before we can admit the existence of beats not consisting of variation of musical notes, we must learn what their sounds really consist of, and have these sounds strictly deduced from the theory; which, to say the least, has not been accomplished so far.

The third kind of beat described by Pole is the overtone-beat of Helmholtz. It is too well understood for any discussion to be needed here.

The combination-tone beats, which were shown in my recent paper to be the actual principal element in the beats of mistuned consonances produced by pure notes, are brought in by Pole at the end, whether regarded as part of the third class or not is not clearly stated.

The next contribution of importance is the famous paper of König on beats and beat-notes, which has been regarded by some authorities as establishing results inconsistent with the views originated by Helmholtz. This paper was to a great extent the origin of my work in connexion with this subject. The question which suggests itself continually in reading König's paper is, "What did all these beats consist of?"—a question which I have endeavoured to answer in my recent paper, and hope to answer more fully on some future occasion.

König only answers this question once, where he says that, in beats of the mistuned octave, the octave and fundamental appear alternately. I regard this as a proof that König's notes were not free from octave. As to this see further Helmholtz, *Tonempfindung*, 4th ed. pp. 263, 264; also Preyer, *Akustische Untersuchungen* (Jena, 1879), p. 15.

On account of the importance of König's paper, I have prepared the following list of the rather numerous errata in the copy in the Philosophical Magazine, as also of those in the copy in Poggendorff. It is not possible to read the copy in the Philosophical Magazine with entire comprehension unless the errata have been first eliminated.

Pogg. *Ann.* vol. clvii.	Phil. Mag. fifth series, vol. i.
Page	Page
191, last line, for $m'=e$ read $m'=c$	427, 10 lines from end, for $m=e'$ read $m'=c$
192, line 4, for $c':d'''$ read $c'':d'''$	427, 7 lines from end, $c'':d'''$
192, line 10, $c'':g''$	427, last line but one, for $e'':g''$ read $c'':g''$
192, eleven lines from end, for $c''\ e''$ read $c''\ c''$	428, line 15, for $c''\ e''$ read $c''\ c''$
193, line 5, for $c''\ c'$ read $c''\ c''$	428, 13 lines from end, for $c''\ c'$ read $c''\ c''$
193, 8 lines from end, c	429, line 13, for c' read c
193, 7 lines from end, C	429, line 14, for c read C
194, line 2, C : H	429, line 22, for C : b read C : B
194, line 8, $c'''\ h^{\mathrm{IV}}$	429, line 26, for $c''':d^{\mathrm{IV}}$ read $c''':b^{\mathrm{IV}}$
208, line 1, for e read c	439, line 17 from end, c

Errata (*continued*).

Pogg. *Ann.* vol. clvii.	Phil. Mag. fifth series, vol. i.
Page	Page
208, line 2, for *e* : *c* read *c* : *c*	439, line 16 from end, *c* : *c*
213, line 9 from end, c'	444, line 8 from end, for *c* read c'
213, line 5, Einklang Stösse	444, line 6, *for* Unison beats *read* Unison. Beats.
213, Rauhigkeit u. C ganz schwach Stösse	444, lines 21, 22, 23, *for* Roughness and C very weak beats *read* Roughness and C very weak. Beats.
213, Octave Stösse	444, beginning of second period, *for* octave beats *read* octave. Beats.
213, Duodecime Stösse	444, beginning of third period, *for* Twelfth beats *read* Twelfth. Beats.
214, Einklang Stösse	445, beginning of first period, *for* Unison beats *read* Unison. Beats.
214, *c* deutlich Stösse	445, last line but one of first period, for *c* distinct beats read *c* distinct. Beats.
214, for $c^{\text{IV}} : c^{\text{V}}$ read $c''' : c^{\text{V}}$	445, heading of third period, for $c^{\text{IV}} : c^{\text{V}}$ read $c''' : c^{\text{V}}$
215, Einklang Stösse	446, first line of table, *for* Unison beats *read* Unison. Beats.
215, C vernehmbar Stösse	446, last line but one of table, *for* C perceptible beats *read* C perceptible. Beats.

Errata (*continued*).

Pogg. *Ann.* vol. clvii.	Phil. Mag. fifth series, vol. i.
Page	Page
216, line 6, mit den Stosstönen zusammenfallen, und sich also bei diesen nicht nachweisen lassen	511, line 13, *for* they coincide with the original note, and therefore cannot be proved by it. *read* they coincide with the beat-notes, and therefore cannot be demonstrated beside the latter.
216, line 11, , $=m$, $=n-m$	511, line 17, *for* $m'=m$ *read* The beat-note $=m$ and for $m'=n-m$ *read* The beat-note $=n-m$
225, last line of table, *for* E *read* C	517, last line, C
232, beim Zusammenklange zwar periodisch an Intensität zu- und abnehmende Schwingungen von nahe zu $\frac{a+b}{2}$ erzeugen.	522, line 21, *for* periodically exhibit when sounded together an increase and decrease of vibrations of about $\frac{a+b}{2}$ *read* when sounded together give rise to vibrations of frequency about $\frac{a+b}{2}$, which periodically increase and decrease in intensity.

The experimental results of König are very completely stated. So far as they affect the questions in dispute they may be shortly put as follows:—

When any consonance formed by notes having the relation of harmonic and fundamental is mistuned, beats occur.

All such beats* *may be regarded as* forming beat-notes of corresponding frequency, which become audible when their number and the intensity of the primaries are sufficient.

Two such beat-notes, when nearly forming unison, octave, or twelfth, produce with each other secondary beats. These in turn may be regarded as forming secondary beat-notes.

Difference- and summation-notes are *regarded* as independent of the beat-notes, and as being much weaker.

Beat-notes *are regarded as* not susceptible of explanation

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* I keep the experimental results clear from König's hypothesis as to their cause.

by difference- and summation-notes, since the frequencies do not correspond*.

The audibility of beats *is regarded as* depending solely on their number, and on the intensity of the primaries, and as being independent of the magnitude of the interval. (This is certainly not true; Helmholtz, *Tonemp.* 4th ed. p. 286.)

In several other respects beats are identified with primary impulses of the same frequency.

According to my results, we should correct these statements by saying that beat-notes are developed from beats by the transformation of a small definite portion of the energy of the beats into the beat-notes, in the transmitting mechanism of the ear.

We now come to a paper of considerable interest, "On Beats of Imperfect Harmonies," by Sir W. Thomson (Proc. Roy. Soc. Edinb. 1877–78, p. 602.) The facts stated in this paper are in accordance with the details of König's experiments. The phase-differences corresponding with the different portions of the beat are exhibited by figures of harmonic curves. The correspondence of the number of beats with that deducible from the forms of the curves is demonstrated experimentally; and the existence of beats is demonstrated in the case of the simpler binary harmonies, when mistuned.

The following is an example of the nature of the description of the sounds heard:—"a *revolving character*† which I perceive in the beat seems distinct enough to prove that the ear does distinguish between these configurations." . . . . It will be seen that the author has here to some extent proposed to himself the question—What are the sounds of which the beats consist? But if my work is right, he has not attained the complete answer to it.

When I first took up the consideration of König's experiments, it seemed to me that the most probable solution was that the notes he used were not pure from overtones. In fact, if we admit the existence of overtones in his notes, phenomena would be produced which would correspond to those he describes. I now know that the beats thus accounted for would not contain some of the elements which do appear. But, since König gives no analysis of the constituent elements of his beats, it was impossible to foresee this. I consequently spent some time in examining the question of the production

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* This supposed difficulty arises out of the restriction to first difference-tones. In my recent paper the identity of the beat-notes with the various difference-tones is sufficiently established.

† Italics in original, foot of p. 610.

of overtones under similar circumstances, and showed that they do exist. Helmholtz (*Tonemp.* 4th ed. pp. 263, 264) comes to the same conclusion; also Preyer (*Akustische Untersuchungen*). And it was only by the application of the new methods recently placed at my disposal that I was able to recognize the insufficiency of this explanation, and to place the matter on the footing developed in my recent paper.

It only remains to notice König's paper in Wiedemann's *Annalen*, 1880, p. 857; and I cannot refrain from expressing my admiration for the clearness and thoroughness with which every point is examined experimentally. I cannot pretend to explain all the phenomena recorded; but I will endeavour to make clear the general point of view from which I regard them.

The proposition König sets himself to maintain is—that a (pure) tone can excite also all the tones of its harmonic series. This proposition was maintained by Wheatstone; and there is a not inconsiderable number of well-informed persons who are disposed to admit it. Its complete discussion is a matter of very great importance. As a matter of mathematics it is quite inadmissible, unless we admit either the impurity of the "tone" employed or the existence of transformation.

It is generally understood and admitted that a very small excitation, operating on a vibrating body of the same period, is capable of exciting large vibrations in the vibrating body. Suppose, for instance, that in König's pendulum with the spring arrangement at the top (p. 867), the pendulum and weighted spring were arranged to swing in exactly equal periods; then, how large would the movements of the pendulum have to be to set the spring in vibration with large amplitudes? There can be no doubt that an extremely minute movement of the pendulum, so small as to be scarcely perceptible, would be sufficient for the purpose.

Now, suppose the pendulum arranged as König had it, so that the pendulum swung as a fundamental, and the spring in the period of a harmonic. Then, if the movement of the pendulum contained ever so little of the harmonic in question, the spring would certainly be set in vibration with large amplitudes, just as in the former case; only that, the minute amount of the higher harmonic motion of the pendulum being masked by the fundamental motion, it would appear as if the fundamental itself was exciting the spring in the period of the harmonic. We have then only to inquire, Is it possible that the fundamental vibration of the pendulum can have been accompanied by a small amount of harmonic? And we answer, it is not only possible, but certain that it must be so. When we speak of pendulum-vibrations we are apt to forget our history, and to think of the circular pendulum as if it really executed perfect harmonic motions. But this is only true of the ideal cycloidal pendulum, which never has been realized, and is never likely to be. And the difference between the two is not negligible: we need only think of the correction for arc which the periodic time requires in pendulum-observations of accuracy, to see that higher harmonic terms must enter into the motion; and in all cases in which the fundamental vibration is not very small indeed, these higher terms will certainly not be negligible.

With any other experimenter than König I should be disposed to point to a number of influences which lead to transformation. One may possibly have escaped him : it is necessary for his purposes to be sure that the bending of the spring calls into action a force strictly proportional to the displacement. For large displacements it is improbable that this condition is satisfied. Further, any unsteadiness in the stand of the instrument leads to transformations of very considerable extent ; but this is not likely to have misled König.

The remaining investigations will now be more easily dealt with.

On pages 860 & 861 the argument rests on the idea that the harmonic in the source of excitation must be developed to the same actual magnitude as the excited harmonic vibration. But this is not at all the case. According to both theory and practice, the excited harmonic vibration can be developed by an extremely small corresponding vibration in the exciter; and it seems to me quite probable that a large fundamental may contain sufficient harmonic for the purpose of excitation without showing any trace of it in its curves, as is apparently the case on page 863.

I do not say that this is the sole explanation of the results; but it is sufficient to prevent me from accepting König's argument as a proof.

There is much difficulty in the question of transmission through the air. When we think of the complicated series of currents and vortices that must surround the prongs of a tuning-fork, it seems very difficult to be sure that there is no transformation there. But certainly König's experiment with the phonautograph (p. 864) seems to refer us back to the explanation above stated.

With respect to the transmission of the vibrations through threads (p. 865), I examined this point some years ago by means of a little apparatus shown to the British Association in 1876. If the threads are not so arranged that the pull on the excited vibrator is strictly proportional to the displacement of the exciter, it is quite certain, both from theory and experiment, that transformation must take place. My apparatus consisted of a number of pendulums arranged as harmonics to a large and heavy metronome. The connexions were made with elastic threads. When the connexions were so made as to satisfy the above requirement as nearly as possible, I could reduce the excitation of the harmonic pendulums to a very small amount; I never succeeded in entirely stopping it. But it was obviously impossible to fulfil the above condition with any approach to real perfection. When, however, the approximate fulfilment of the condition was purposely avoided, as by letting the thread just go slack at one point of the vibration, transformation set in at once, as it should do, and the small pendulums were set in violent vibration. At that time I pointed out the defect of Mayer's arrangement of transmission by threads (note, p. 865), as it did not appear from the account that any means were taken to render the pull strictly proportional to the displacement of the exciter, and consequently transformation was to be expected.

On the whole, no doubt, the truth of the matter is best stated in a form that combines much of what both parties to this discussion have maintained. It is quite true that, in a hypothetical system in which the forces called into action are strictly proportional to the displacements, the fundamental harmonic vibration cannot permanently excite its multiples. This is unassailable as matter of mathematics; and as to experiment, we can only say that, the nearer we approach to the construction of such a system, the less are the multiples excited by the fundamental. But I think that the actual construction of such a system is impossible. And so far as our actual systems depart from the above condition, more or less, transformation does and must take place. Wheatstone's law is therefore generally true of actual systems; and it is only incomplete because it omits the question of quantity. How much of the multiple vibration is excited in systems whose forces depart to known extents from the simple law of proportionality? So far as we are able to answer by our general knowledge of the facts, they are entirely in accordance with the theory. The more minute comparison of the different classes of systems should be an experimental study of great interest but no theoretical difficulty, which would materially assist to throw light on the general comprehension of the subject.

XXXV. *On the best Arrangement of the Wheatstone's Bridge for the Measurement of a particular Resistance.* *By* THOMAS GRAY, *B.Sc., F.R.S.E.*[*]

[Plates IV., V., & VI.]

NO experimental method is in more general use than is that of Wheatstone's bridge for the comparison of electrical resistances. It appears, however, that erroneous ideas are prevalent among practical electricians, and have found their way into works on electrical testing, as to the proper resistances to be used in the different branches of the Bridge in order that the most sensitive arrangement may be obtained in any particular case. The following investigation, together with some graphical illustrations of the effect of using different resistances, was originally prepared for the use of my students; but the fact just referred to has led me to believe that it may be found generally useful by those engaged in electrical work.

The most general case (that in which, for the measurement of a particular resistance, all the other resistances, including those of the galvanometer and battery, may be varied) has been excellently treated by Prof. Clerk Maxwell, and more recently by Prof. Chrystal in his article "Electricity" in the *Encyclopedia Britannica*; but, so far as I am aware, no one has considered in detail all the different cases[†].

The subject is divided into three parts, each of which may be found useful in particular cases. The first part, however, is that which will be of the greatest service as a guide for practical purposes. In this part the galvanometer and battery are supposed to be constants, while the bridge-resistances are changed to suit particular cases. It is seldom convenient to change either the battery or the galvanometer much during a series of tests; and I therefore think this part the most interesting and useful.

PART I.

The problem is this:—With a given battery and a given galvanometer, and a certain resistance to be measured, what must the other three resistances be in order to give the best effect?

Let a, b, R, c (Pl. IV. fig. 1) be the resistances in the bridge, g and f the galvanometer- and battery-resistances, and G the current passing through the galvanometer when balance

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[*] Communicated by the Author.

[†] This paper was prepared for press before the author knew of the existence of Mr. Oliver Heaviside's paper in the Philosophical Magazine, vol. xlv. (1873) p. 114, in which the case treated in Part I. below is considered, and concordant results arrived at.

is approximated to to a degree determined by the equation

$$C\frac{a}{c}=\frac{b}{R},$$

where C is a constant very near unity.

By the aid of Kirchhoff's corollaries to Ohm's law we readily obtain the following equation to the current passing through the galvanometer when E is the electromotive force of the battery.

$$G=\frac{E(aR-bc)}{ab(c+R)+cR(a+b)+g(a+c)(b+R)+f\{(a+b)(c+R)+g(a+b+c+R)\}}. \quad . \quad (1)$$

Along with this equation we have the condition

$$C\frac{a}{c}=\frac{b}{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Substituting $C\frac{aR}{c}$ for b in equation (1) and putting $1-C=i$, we get

$$G=\frac{ERaci}{CaR\{(c+R)a+cR+g(a+c)+f(c+R)+fg\}+ac\{cR+f(c+R)+fg\}+cR\{g(a+c)+fg\}+c^2fg}.$$

We have to find when this quantity is a maximum, R being supposed constant. It will be found more convenient to take the reciprocal of G and solve for a minimum. We have

$$\frac{Ei}{G}=R+2(g+f)+\frac{fg}{R}+\frac{(R+g)(a+f)}{c}+\frac{R+f}{R}c+\frac{g(c+f)}{a}+\frac{cfg}{aR}+a, \quad . \quad . \quad (3)$$

neglecting C and making i very small.

Differentiating this quantity with respect to a and c respectively, we obtain

$$\frac{d}{da}\frac{Ei}{G}=\frac{R+g+c}{c}-\frac{g\{R(c+f)+cf\}}{Ra^2}, \quad . \quad . \quad (4)$$

$$\frac{d^2}{da^2}\frac{Ei}{G}=2\frac{g\{R(c+f)+cf\}}{Ra^3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{d}{dc}\frac{Ei}{G}=-\frac{(R+g)(a+f)}{c^2}+\frac{(a+g)(R+f)}{Ra}, \quad . \quad (6)$$

$$\frac{d^2}{dc^2}\frac{Ei}{G}=2\frac{(R+g)(a+f)}{c^3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\frac{d^2}{da\,.\,dc}\frac{Ei}{G}=-\left\{\frac{R+g}{c^2}+\frac{g(R+f)}{Ra^2}\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Equating (4) to zero, we get

$$a^2=\frac{gc\{\mathrm{R}(c+f)+cf\}}{\mathrm{R}(\mathrm{R}+g+c)}. \quad \ldots\ldots\ldots\ldots \quad (9)$$

Equating (6) to zero, we get

$$c^2=\frac{\mathrm{R}a(\mathrm{R}+g)(a+f)}{(a+g)(\mathrm{R}+f)}. \quad \ldots\ldots\ldots\ldots \quad (10)$$

Substituting for c in (9), we find

$$a^2=\frac{\dfrac{ga(\mathrm{R}+g)(a+f)}{a+g}+\left\{\dfrac{\mathrm{R}a(\mathrm{R}+g)(a+f)}{(a+g)(\mathrm{R}+f)}\right\}^{\frac{1}{2}}gf}{\left\{\dfrac{\mathrm{R}a(\mathrm{R}+g)(a+f)}{(a+g)(\mathrm{R}+f)}\right\}^{\frac{1}{2}}+\mathrm{R}+g}, \quad .\ . \quad (11)$$

the positive signs before the radicals only being taken, as a and c are essentially positive.

After reduction, all the factors being retained, this expression takes the form

$$(a^2-gf)^2\frac{a(\mathrm{R}+g)}{a+g}\{\mathrm{R}(a+f)(a+g)-a(\mathrm{R}+f)(\mathrm{R}+g)\}=0.$$

Hence we have

$$\frac{a(\mathrm{R}+g)}{a+g}=0, \text{ and } \therefore a=0;$$

or

$$a^2-gf=0, \text{ and } \therefore a=\sqrt{gf}; \quad \ldots\ldots\ldots \quad (12)$$

or

$$(\mathrm{R}-a)(fg-\mathrm{R}a)=0, \text{ and } \therefore a=\mathrm{R} \text{ or } \frac{fg}{\mathrm{R}}.$$

On examining equation (11), it will be found that the latter two roots refer to the equation when the negative signs are taken before the radicals.

From equations (9) or (10) we see that, when $a=0$, $c=0$; and, again, when $a=\sqrt{fg}$,

$$c=\sqrt{\frac{\mathrm{R}f(\mathrm{R}+g)}{\mathrm{R}+f}}. \quad \ldots\ldots\ldots \quad (13)$$

Substituting these values in equations (5), (7), and (8), it appears that $a=0$ and $c=0$ neither give a minimum nor a maximum, but that $a=\sqrt{fg}$ and $c=\sqrt{\dfrac{\mathrm{R}f(\mathrm{R}+g)}{\mathrm{R}+f}}$ always give a maximum value for G.

This leads to a very simple rule for the adjustment of the bridge, because, no matter what the resistance to be measured may be, the resistance a should have a perfectly definite value, and then the subsequent adjustment of c is easy after a first rough test for R.

The effect of using the resistances given by this rule is very striking, and is illustrated by means of the subjoined curves.

The first, second, and third sets of curves (Plates IV. & V.) show the effect of using a value of a differing more and more from $\sqrt{fg}$, the value of c being in each case determined so as to give the best effect for that particular value of a. An examination of the curves will show that, for any particular battery and galvanometer, the resistance which is best measured is that which is equal either to the resistance of the galvanometer or the battery, the arrangement being equally sensitive for both. Care must be taken, however, not to confound this with the best arrangement for measuring the resistance here referred to; for it does not follow that no better arrangement can be made for the measurement of a resistance because it is the one which gives the best result in a particular arrangement. For instance, if we have a battery-resistance 100 and galvanometer 10, we find the greatest sensibility will be obtained when $\mathrm{R}=10$ or 100; but, as we shall see below, if the battery be rearranged so that f shall also be 10, a still greater degree of sensibility will be obtained for the measurement of the resistance 10.

The fourth set of curves (Pl. VI.) illustrates the effect of varying the ratio of a to c from that which gives the best effect. In these curves, then, a is supposed constant, while c varies—the abscissæ being proportional to c and the ordinates to $\frac{\mathrm{G}\sqrt{g}}{i.e}$, where e denotes the electromotive force per unit of battery-resistance. In the first three sets of curves the abscissæ are proportional to a and the ordinates to $\frac{\mathrm{G}\sqrt{g}}{i.e}$. It must be observed, however, that in the third set the ordinates are ten times greater than they would be if plotted to the same scale as the first and second sets. Curves II. and III. of the third set are shown on the right-hand side, with their abscissæ also multiplied by ten.

Part II.

The second case investigated is that in which the galvanometer-resistance is also supposed to vary, the battery-resistance being supposed constant in this case. This assumption is made with regard to the battery, because for any given value of E the maximum effect is obtained when $f=0$. It is to be remembered, then, that diminishing f always increases the sensibility of the bridge.

In this case the number of turns of wire in the coil of the galvanometer has a considerable influence on the result, and hence comparison between the sensibility in one case and that in another when the galvanometer has been changed is difficult. I have assumed in the present investigation that the galvanometers are all such that they will give deflections $=\mathrm{AG}\sqrt{g}$, where A is a constant, which must be the same for each instrument.

From equation (3) we may express $\dfrac{\mathrm{F}i}{\mathrm{G}\sqrt{g}}$ in the form

$\dfrac{\mathrm{E}i}{\mathrm{G}\sqrt{g}}=\dfrac{c_1+c_2g}{c_3\sqrt{g}}$, where c_1, c_2, c_3 are constants;

∴ for a minimum

$$g=\frac{c_1}{c_2}$$

$$=\frac{\mathrm{R}+2f+\dfrac{\mathrm{R}(a+f)}{c}+\dfrac{\mathrm{R}+f}{\mathrm{R}}c+a}{2+\dfrac{f}{\mathrm{R}}+\dfrac{a+f}{c}+\dfrac{c+f}{a}+\dfrac{cf}{a\mathrm{R}}}$$

$$=\frac{(\mathrm{R}+2f)\mathrm{R}ac+\mathrm{R}a^2(a+f)+ac^2(\mathrm{R}+f)+a^2c\mathrm{R}}{2ac\mathrm{R}+acf+a\mathrm{R}(a+f)+c\mathrm{R}(c+f)+c^2f}$$

$$=a\frac{(\mathrm{R}+c)\{f(\mathrm{R}+c)+\mathrm{R}(a+c)\}}{(a+c)\{f(\mathrm{R}+c)+\mathrm{R}(a+c)\}}$$

$$=a\frac{\mathrm{R}+c}{a+c}. \qquad (14)$$

We have now to combine this value of g with the values of a and c previously found, namely

$$a^2=fg, \qquad (12)$$

$$c^2=\frac{\mathrm{R}f(\mathrm{R}+g)}{\mathrm{R}+f}, \qquad (13)$$

$$g=a\frac{c+\mathrm{R}}{a+c}. \qquad (14)$$

From (12) and (14) we get

$$a=f\frac{c+\mathrm{R}}{a+c};$$

and

$$\therefore\ c=\frac{f\mathrm{R}-a^2}{a-f}.$$

Substituting this value in (13), we obtain

$$(R+f)(fR-a^2)^2=(a-f)^2R(Rf+a^2);$$

$$\therefore\ (R^3+2R^2a-3Ra^2)f-a^2(3R^2-2Ra-a^3)=0;$$

$$\therefore\ (R-a)\{R+(R+3a)-a^2(a+3R)\}=0.$$

And $R=a$ is one root. There remains the cubic equation

$$a^3+3a^2R-3aRf-R^2f=0, \quad . \quad . \quad . \quad . \quad . \qquad (15)$$

which, when solved in the ordinary way, assumes an irreducible form. It is more convenient to retain the equation as it stands, and solve by approximation for the numerical root in particular cases.

It will be noticed that $R=a$ is not a root which generally satisfies the equation, because, by equations (12) and (14), $f=R$ in that case, which is contrary to the hypothesis of f being a constant. Equation (15), however, provides a maximum value of $G\sqrt{g}$; and hence this is the root required.

The following table shows the calculated values of a, c, and g, corresponding to the battery-resistances (f) and the resistances to be measured (R), given in the first and second columns respectively. The advantage obtained by increasing f is plain from the increased value of $\frac{G\sqrt{g}}{i.e}$, as shown in the last column. The same thing may be readily observed by examining the fifth set of curves. The abscissæ of these curves represent battery-resistance, and the ordinates the value of $\frac{G\sqrt{g}}{i.e}$.

f.	R.	a.	c.	g.	$\frac{G\sqrt{g}}{i.e}$.
1	0·01	0·42	0·065	0·176	0·0123
,,	0·10	0·44	0·20	0·194	0·0417
,,	1·00	1·00	1·00	1·000	0·0625
,,	10·00	2·30	3·73	5·290	0·0424
,,	100·00	6·23	11·70	38·810	0·0140
,,	1000·00	18·76	36·12	352·000	0·0052
10	0·10	1·60	0·188	0·256	0·054
,,	1·00	4·38	1·630	1·918	0·131
,,	10·00	10·00	10·000	10·000	0·197
,,	100·00	22·70	37·06	51·53	0·142
,,	1000·00	62·00	121·00	384·40	0·054
50	10·00	28·6	14·8	16·36	0·343
,,	100·00	63·0	77·0	80·00	0·427
,,	1000·00	151·0	262·0	459·00	0·469

Part III.

The third case is that in which all the resistances, except that to be measured, may be varied.

If we consider the battery only to vary, we obtain the best arrangement when the greatest total current flows through the bridge; and this leads at once to the equation

$$f=\mathrm{R}\frac{a+c}{c+\mathrm{R}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Now, equations (12) and (13) taken together give the best arrangement when the battery and galvanometer are constants, while (14) gives the best arrangement when the galvanometer is the only variable. By treating these four equations as simultaneous we evidently obtain the best arrangement when every thing may be varied.

From (14) and (16) we get $gf=a\mathrm{R}$; hence from (12) we have $a=\mathrm{R}$; and consequently by (14) $g=a$.

Substituting in (13), we find $c=\mathrm{R}$; and we are led to conclude that, for the best effect,

$$a=b=c=\mathrm{R}=g=f.$$

When the resistance of the electrodes in the battery circuit is taken into account, the equations become (r being the resistance of the electrodes)

$$a=\sqrt{g(f+r)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$$c=\frac{\sqrt{\mathrm{R}(f+r)(\mathrm{R}+g)}}{\mathrm{R}+f+r}, \quad . \quad . \quad . \quad . \quad . \quad (18)$$

$$g=a\frac{c+\mathrm{R}}{a+c}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

$$f=c\frac{a+c}{c+\mathrm{R}}+r. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

From (17) and (19) we get

$$f=a\frac{a+c}{c+\mathrm{R}}-r. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Again, from (17) and (18) we get

$$f=\mathrm{R}\frac{a^2-c^2}{c^2-\mathrm{R}^2}-r. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Combining (21) and (22),

$$a=\mathrm{R}\frac{a-c}{c-\mathrm{R}} \text{ or } a=\frac{\mathrm{R}c}{2\mathrm{R}-c}. \quad . \quad . \quad . \quad (23)$$

Subtracting (20) from (21), we get

$$0=(a-c)\frac{a+c}{c+\mathrm{R}}-2r. \quad . \quad . \quad . \quad . \quad (24)$$

Substituting from (23) in (24), we obtain

$$c^4-4\mathrm{R}c^3+3\mathrm{R}^2c^2+2r(\mathrm{R}+c)=0.$$

Similarly we may obtain

$$a^4+2\mathrm{R}a^3-\{6\mathrm{R}r+3\mathrm{R}^2\}a^2-8\mathrm{R}ra-2\mathrm{R}^3r=0.$$

By solving either of these equations for any particular case the value of a or c may be obtained; and then the values of the other quantities may be easily got from equations (19), (20), and (23).

This case can only have interest when it is not practicable to reduce the electrode resistance considerably below that of the battery.

XXXVI. *Liquefaction and Cold produced by the mutual Reaction of Solid Substances.* *By* Miss EVELYN M. WALTON[*].

THE mixing of two dry, finely-powdered salts, one or both containing water of crystallization, is often attended by liquefaction with decrease of temperature, which in many instances is very marked; and sometimes there is also a decided change in colour.

A transparent homogeneous liquid is sometimes, though rarely, obtained; but generally the liquid holds in suspension an insoluble compound or an undissolved salt either in the hydrous or anhydrous state; and sometimes the consistency is that of a stiff paste.

History.—It has long been known that freezing-mixtures may be made by mixing some salt with ice or snow; and in 1875–6 Guthrie[†] determined the lowest attainable temperature of quite a large number of such mixtures. He found that the lowest temperature obtained with any given salt was the same whatever its initial temperature; also that, within certain wide limits, this was independent of the proportions used.

The earliest allusion I find made to freezing-mixtures formed by the use of salts only, is in the ninth volume[‡] of Silliman's Journal, where Ordway, in a paper on Nitrates mentions experiments in which the mixture of ammonium bicarbonate with hydrated iron nitrate and with hydrated aluminum nitrate was followed by a reduction of temperature

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[*] From the 'American Journal of Science' for September 1881.

[†] Phil. Mag. [4] xlix. p. 210.

[‡] II., ix. pp. 30, 31, 33.

from 58° to −5° F., and from 51° to −10° respectively. Subsequently* he mixed nitrate of iron with Glauber's salt, and obtained a reduction of 32° F.

Berthelot, in his recent work on Thermo-Chemistry, devotes a brief space to the subject; and the *Comptes Rendus*, vol. xc. pp. 1163, 1282, contains a communication from Ditte calling attention to this wonderful phenomenon. He considers the use of concentrated acids with hydrated salts, also mixtures composed solely of salts. An example is given of ammonium nitrate and hydrated sodium sulphate mixed together in a mortar, the loss of heat being about 20° C.

Liquefaction of Salts.—As far as we know, when any salt soluble in water is mixed with ice, liquefaction is sure to follow, and the minimum temperature is below 0° C. But when salts only are taken, the case is different.

In some instances liquefaction is very evident, in others there is none at all, and in still others it is doubtful; while the loss of heat is sometimes great, sometimes very slight, according to the amount of liquefaction. Whether moistening will take place or not must be decided in nearly every case by actual trial; and in the preliminary experiments made with reference to this point, I have mixed the substances in a wedgwood mortar.

From a large number of trials, the following conclusions have been drawn:—

1. As a rule, it is necessary to liquefaction that one of the solid substances used should be hydrated.

2. It is not necessary that each solid should be a salt. Moistening sometimes follows the mixing of a salt with an acid, a salt with a base, or a base with an acid.

Ex.—Calcium chloride ($CaCl_2 . 6 H_2 O$) with tartaric acid ($C_4 H_6 O_6$).

Sodium sulphate ($Na_2 SO_4 . 10 H_2 O$) with potassium hydrate (KOH).

Potassium hydrate (KOH) with tartaric acid ($C_4H_6O_6$).

3. As, in the case of liquids, metathesis will take place if a compound insoluble in the menstruum can be formed, so with solids, if such a compound can result, metathesis is probable with liquefaction.

4. If, by mixing two salts, an insoluble compound is produced, a mixture of two others like the new ones formed will not, as a general thing, be attended by liquefaction.

5. When no insoluble compound is formed, four bodies are probably contained in the product, metathesis being partial;

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* II., xxvii. p. 15.

for it is sometimes observed that liquefaction seems equally marked, whether the two original salts are mixed or the two bodies formed by their interchange.

6. The rule among liquids in regard to weak and strong acids and bases seems to prevail with solids also, their action tending to promote or impede liquefaction.

7. When, by the admixture of two salts, oxidation or reduction can take place, there is again probability of liquefaction.

Ex.—$SnCl_2 . 2H_2O$	with	$HgCl_2$	liquefied.
,,	,,	$Fe_2Cl_6 . 12H_2O$	,,
,,	,,	$CuCl_2 . 2H_2O$	,,
,,	,,	$PbCl_2$	no liquefaction.

In the last no change by reduction is possible.

A new Substance.—A notable exception to the rule mentioned above (that one salt at least should be hydrated), is that of $AgNO_3$ mixed with $HgCl_2$. When these are rubbed together there is decided moistening, which would seem to prove that there is such a body as anhydrous nitrate of mercury liquid at ordinary temperatures. On adding water a large residue of silver chloride is observed.

Evidences of Chemical Change.—When salts capable of metathesis are mixed, in addition to liquefaction, change of colour, formation of an insoluble compound, and escape of a gas are proofs of chemical reaction.

An important difference sometimes noticed between mixtures of salts in the solid and the liquid form is the escape, in the former case, of some gas, as $C_2H_4O_2$, CO_2, HCl, or NH_3. The gas is dissolved by a liquid solution and eludes observation.

Classification.—Cases of liquefaction may be divided into two classes—the first including those in which there is mutual exchange of base or acid, the second those in which there is no interchange.

The mixture of lead nitrate with sodium carbonate is an example of the first class. There is metathesis, and we obtain lead carbonate, sodium nitrate, and ten equivalents of free water.

$$Pb(NO_3)_2 + Na_2CO_3 . 10H_2O$$
$$= PbCO_3 + 2NaNO_3 + 10H_2O.$$

Hydrated Product.—When iron nitrate is mixed with calcium chloride, thirty-six equivalents of water in some form are obtained.

$$Fe_2(NO_3)_6 . 18H_2O + 3[CaCl_2 . 6H_2O]$$
$$= Fe_2Cl_6 + 3Ca(NO_3)_2 + 36H_2O.$$

Having mixed equivalent weights, the product was dried on the smooth surface of a plate of plaster of Paris, which absorbed the moisture; and an analysis showed the two new salts obtained to be hydrated.

Therefore

$$Fe_2(NO_3)_6 . 18H_2O + 3[CaCl_2 . 6H_2O]$$
$$= Fe_2Cl_6 . 12H_2O^* + 3[Ca(NO_3)_2 . 4H_2O] + 12H_2O.$$

This experiment was repeated with a mixture of $Fe_2(NO_3)_6 . 18H_2O$ with NaCl, also of $Ca(NO_3)_2 . 4H_2O$ with $MgSO_4 . 7H_2O$; and the first product was found to contain iron chloride, the second nitrate of magnesium, both in the hydrated form.

At the more or less low temperature due to liquefaction, there is naturally a tendency for salts to crystallize out from the saturated solution.

The crystalline character is sometimes perceptible to the senses; for the product often contains grains much coarser than did the finely-powdered salts first taken.

Effect of Temperature.—Experiments show that sometimes liquefaction takes place readily at a temperature somewhat elevated, but not at all at a low temperature. A mortar and pestle which had been warmed by hot water were occasionally used, care being taken that the heat should not be great enough to cause either of the original salts to melt in their water of crystallization.

When two salts capable of metathesis are mixed, chemical action apparently begins immediately at every point of contact. But there is a limit to the fineness of division which may be effected by mechanical means; and the substance consists of minute grains coated on the outside with the new product, while remaining unchanged at the interior.

When liquefaction ensues, the interchange is continued, either because by removing the particles of the product new surfaces are presented, or because the liquid, penetrating the granules, separates them into their molecules.

If the salts taken furnish little or no water in excess of that required to combine with the new ones formed, the process of interchange apparently soon ceases, unless sufficient heat is supplied to prevent the constituents of the product from assuming the solid form.

Difficultly Soluble Salts.—When salts difficultly soluble are used, moistening follows but slowly, if at all. The molecules

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* It was found that $Fe_2Cl_8 . 12H_2O$, and not $Fe_2Cl_6 . 6H_2O$, was formed.

of such substances are not easily separated with a limited supply of water, especially at a reduced temperature.

Liquefaction without Chemical Reaction.—The second class referred to above includes mixtures of salts of the same base, or having the same acid; and although it seems to be the exception rather than the rule that there should be liquefaction in such cases, yet this sometimes occurs.

Ex.—$Fe_2Cl_6 . 6H_2O$	with	$Fe_2(NO_3)_6 . 18H_2O$,	liquefied
$Fe_2Cl_6 . 12H_2O$	,,	,,	,,
$FeCl_2 . 4H_2O$*	,,	,,	,,
$FeSO_4 . 7H_2O$	,,	,,	,,
,,	,,	$Fe_2Cl_6 . 6H_2O$	,,
$Na_2C_2H_2O_2 . 6H_2O$	,,	$PbC_2H_2O_2 . 3H_2O$	,,
,,	,,	$K_2C_2H_2O_2$	,,
,,	,,	$ZnC_2H_2O_2 . 3H_2O$	,,
$Na_2SO_4 . 10H_2O$	,,	$ZnSO_4 . 7H_2O$	,,
,,	,,	$Li_2SO_4 . H_2O$	,,
$CaCl_2 . 6H_2O$	,,	$Fe_2Cl_6 . 12H_2O$	,,
,,	,,	$CuCl_2 . 2H_2O$	,,

Some interesting experiments with caustic soda ($NaOH$) showed that when it was used with any hydrated sodium salt the combined water was liberated, evidently to satisfy the affinity of $NaOH$ for water.

KOH was also used with various hydrated salts; and in every instance liquefaction ensued. Apparently the hydrated salt was attacked for the sake of its water; and the first reaction seems to be appropriation of water by KOH, which is doubtless followed by metathesis in most cases.

Liquefaction in the examples given above, however, cannot be explained in this way. Neither is there metathesis; and evidently double salts are not formed.

Having mixed equivalent weights of $ZnSO_4 . 7H_2O$ and $Na_2SO_4 . 10H_2O$, the composition of the resulting solid part was found not to be that of a double sulphate, there being an excess of Na_2SO_4.

Equivalent weights of $CaCl_2 . 6H_2O$ and $Ca(NO_3)_2 . 4H_2O$ were mixed, also of $Fe_2Cl_6 . 12H_2O$ and $Fe_2(NO_3)_6 . 18H_2O$, with a view to analysis; but in each case the thin liquid disappeared entirely into the plaster plate used for absorption, leaving only a stain visible.

Theory.—These examples must be similar in nature to mixtures of salts with ice, which result in liquefaction and solution of the salts.

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* Of course a ferric and a ferrous base are not strictly the same; but ferrous nitrate is too unstable a body with which to work except in the coldest weather.

That the cold produced when ice and a salt are mixed is due to rapid liquefaction of the ice is plain enough; but I have seen no attempt made to explain the cause of the liquefaction, until Ordway last year announced his theory of the "diffusion of solids" in an address * before the American Association for the Advancement of Science.

We know that the molecules of a body are in a state of constant oscillation, and that if a salt solution be placed in contact with pure water, diffusion takes place until the molecules of salt are equally distributed throughout the mass.

So, too, when the solid is placed in water, solution follows, or, in other words, diffusion. Now, when a salt and water, both in the solid form, are in contact, there is probably the same tendency to interpenetration. But a mixture of water and salt molecules cannot remain in the solid form except at a low temperature, and the rigidity of the solid state is overcome, because oscillations of the water and the salt molecules cooperate to produce a greater motion.

Graham found that although sodium chloride is not at all deliquescent, yet the saturated solution has a great affinity for water. Therefore when the smallest quantity of the salt is once in solution the first step is taken, and the melting of the ice continues rapidly. If this is the true explanation of the action of sodium chloride on ice, the problem is solved.

When salts capable of metathesis are used, this physical phenomenon is complicated by chemical reaction. Liquefaction probably results when $CaCl_2 . 6\,H_2O$ and $Ca(NO_3)_2 . 4H_2O$ are mixed, and in similar cases, because the crystallizing-point of these two bodies together is lower than for each alone—just as the freezing-point of salt water is lower than that of fresh water, and as the fusing-point of an alloy is sometimes below that of either of its constituents.

Calorimeter.—For further experiments in which the reduction of temperature might be measured with some degree of accuracy, it was desirable to secure a closed space in which radiation and convection should be reduced to a minimum, and the heat of the surroundings should be constant. A calorimeter was therefore constructed somewhat like that used by Berthelot in some of his investigations.

It consists of a covered circular tank of fourteen-ounce tinned copper, of about twelve gallons capacity, placed in a much larger wooden case, the space between the walls of the tank and case being filled with loose cotton.

The upper surface of the tank has four wells, each to receive a cylindrical vessel of polished german silver resting on

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* Proceedings Amer. Assoc. Adv. Science, vol. xxix. p. 293.

cork supports and having an air-space around and under it. Each of these vessels, containing a glass beaker of smaller diameter (also on cork rests), is furnished with a closely-fitting cork cover, perforated to admit a thermometer and a slender wooden stirrer consisting of an upright rod with cross arms at bottom, like a pug-mill.

The thermometers have each a long stem with the scale on the upper part, so that readings even to −40° C. can be taken without raising the bulb from the mixture.

The tank is kept filled with water*; and this is frequently agitated by a stirrer moved with a crank. The stirrer revolves horizontally in the bottom of the tank; and having two blades like a propeller, it agitates the water thoroughly from bottom to top, the moistened part being always immersed. Over the whole is a closely-fitting wooden cover, also perforated for the thermometers and stirrers.

The salts to be mixed, after finely pulverizing, were placed in separate beakers within the calorimeter, and left for a time to acquire a uniform temperature. The contents of one beaker were then added to those of the other, the cover replaced as quickly as possible, and the whole mixed vigorously by twirling the stirrer. Liquefaction generally took place in five to ten minutes; and observations of time and temperature were then taken, slight agitation being still continued. There being four beakers, two experiments can be carried on at the same time; and as the cover is not in a single piece, one portion can be removed without uncovering the other pair of beakers.

Equivalent weights were taken, seventy grams being used at first; but this was afterward increased to one hundred grams.

From the following observations it will be seen that the amount of radiation and convection is so small that it may be disregarded.

Mixture of $Mn(NO_3)_2 . 6H_2O$ with $Na_2CO_3 . 10H_2O$.
Temperature of water of calorimeter, 18° C.

Time.	Temperature.
0 min.	19° C.
4 ,,	− 9
5 ,,	−10
6 ,,	−10·5
10 ,,	−10·5
11 ,,	−10
21 ,,	− 7
25 ,,	− 5

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* Water of any decided temperature may be used.

Six minutes were required to reach the lowest point; and during the next five minutes there was a gain of but 0°·5. Stirring was stopped at the end of eleven minutes.

Lowest attainable Temperature.—In addition to Guthrie's discoveries already mentioned, he found that when two salts were used with ice the minimum temperature was unlike that of either alone, each exercising an influence over the other.

Most of my experiments with the calorimeter were made for the purpose of discovering whether or not the lowest attainable temperature of a given salt when mixed with ice is the same if that salt is produced in a freezing-mixture of two salts, also if it is independent of the initial temperature and the proportions used.

The hydrated sulphate and carbonate of sodium were each mixed with various nitrates, whereby nitrate of sodium was produced and a sulphate or carbonate, usually an insoluble compound, which I thought could not influence the result.

The lowest attainable temperature of sodium nitrate with ice is −17° C.

The following results were obtained with metals whose carbonates are without doubt anhydrous insoluble compounds:—

		Initial temp.	Lowest temp.	Loss.
$Pb(NO_3)_2$	with $Na_2CO_3 . 10H_2O$	19° C.	−17°	36°
„	„ „	0	−17	17
$Ba(NO_3)_2$	„ „	21·3	−13·7 *	35
„	„ „	− 1	−17	16
$Al_2(NO_3)_6 . 18H_2O$	„ „	14	−18	32
„	„ „	− 4	−18	14
$Cu(NO_3)_2 . 6H_2O$	„ „	16·5	−18	34·5
„	„ „	− 2	−15 †	13

With the nitrates of zinc, manganese, iron, and chromium the results were not so free from modifying influences as I had anticipated, basic carbonates being formed not wholly insoluble at low temperatures.

The interesting fact was thus revealed that ferric carbonate or basic carbonate exists in the liquid form at a low temperature, say −20° C. The colour is a deep red; and, as the mixture gradually warms, CO_2 is rapidly given off, causing the contents of the beaker, which was not at first more than half-filled, to overflow and insoluble Fe_2O_3 to be deposited.

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* An insufficient quantity was taken.

† Liquefaction proceeded very slowly.

			Initial temp.	Lowest temp.	Loss.
$Mn(NO_3)_2.6H_2O$	with	$Na_2CO_3.10H_2O$	18°	−14°	32°
”	”	”	− 2	−26	24
$Zn(NO_3)_2.6H_2O$	”	”	20	−16·7	36·7
”	”	”	− 1	−21·5	20·5
$Cr_2(NO_3)_6.18H_2O$	”	”	− 3	−22	19
$Fe_2(NO_3)_6.18H_2O$	”	”	13·5	−17	30·5
”	”	”	10·5	−17	27·5
”	”	”	− 3	−24	21

It will be seen from these and the following results that the minimum temperature is not independent of the initial temperature; it was also found that the lowest point varies with the proportions taken:

			Initial temp. 1st.	Initial temp. 2nd.	Lowest temp.
$Pb(NO_3)_2$	with	$Na_2CO_3.10H_2O$	45°	22°*	−12°5
$Fe_2(NO_3)_6.18H_2O$	”	”	39	32	− 4
$Al_2(NO_3)_6.18H_2O$	”	”	37	32	− 2

With the nitrates of magnesium and calcium the tendency to metathesis is so slight that the liquefaction is not rapid enough to produce any great degree of cold; and with an initial temperature of −2° there is no liquefaction whatever.

The time allotted for the completion of my graduation thesis, of which this paper gives the substance, rendered it necessary to suspend for the present the continuation of these experiments. This work was undertaken at the suggestion of Professor Ordway, to whom the subject has been one of interest for some years, but whom the pressure of other duties has prevented from pursuing an investigation. He has, however, given considerable thought to the matter, one of the results of which is his theory of the “diffusion of solids.” His predictions that there may be liquefaction without chemical reaction, and that the product obtained from the mixture of salts is sometimes hydrated, were both confirmed by the results of my work. He devised the calorimeter which was used; and I am indebted to him also for valuable suggestions and advice.

Mass. Inst. Technology, June 3, 1881.

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* The temperature of $Na_2CO_3.10H_2O$ could not be raised so high as that of the other salts, without melting.

XXXVII. *Note on a new Method of Measuring certain Chemical Affinities.* *By* ALFRED TRIBE*.

WHEN a metal is immersed in an electrolytic field (*i. e.* in an electrolyte in the act of electrolysis), and the electromotive force set up on any part of its surface suffices to overcome the affinities of the radicals of the medium, the positive ion separates on that part of the surface which may be supposed to have received − electrification, and the negative ion on that part which receives + electrification.

When the metal is in the form of a rectangular plate and placed so that the lines of force are perpendicular to its surface, the maximum electromotive force set up is on the central parts of such a plate, becoming less towards the edges, where, and for some little distance from which, it is insufficient to initiate electrochemical action.

When the rectangular plate is placed in the electrolytic field so that the lines of force are parallel with one of its edges, the maximum electromotive force is on the end of the plate, becoming less and less towards the central parts until it no longer suffices to bring about electrochemical action. This is denoted by the boundaries of the deposits, which in many cases are very sharply defined. A plate in fact, in the position just named, may be regarded, in so far as electrical power is concerned, as a series of pairs of electrodes, the limits of the electro-deposits representing a pair, the electromotive force of which is just incapable of resolving the electrolyte into its constituent ions (Proc. Roy. Soc. nos. 209, 214, 1881; Phil. Mag. vol. xi. p. 446, 1881).

The very intimate relation between electromotive force and chemical affinity was a long time ago pointed out by Sir W. Thomson, and more recently in an elaborate research by Dr. Wright. It was therefore anticipated that if, in a series of trials, the chemical affinities were altered while other circumstances remained the same, the magnitude of the intermedial space between the boundaries of the electro-deposits would increase along with the force required to overcome the affinities of the ions of the electrolyte.

A series of experiments with molecular solutions of the chloride, bromide, and iodide of zinc with plates (*analyzers*) of silver, copper, iron, and zinc showed that this is the case; *i. e.* the intermedial space with the chloride was greatest, the bromide less, and the iodide least. Another series with silver

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* Communicated by the Author, having been read at the Meeting of the British Association, York, September 1881.

analyzers, but with molecular solutions of the sulphates of zinc and copper, showed that the intermedial space in the case of the zinc sulphate was much the greater. Again, another series with zinc sulphate showed that a zinc analyzer gave the least intermedial space, iron greater, copper greater still, and silver most of all.

This method affords a simple means of demonstrating differences of chemical affinity; but whether it is capable of giving more than approximative measurements will require further investigation to determine. In some cases secondary actions are, of course, set up, so that the intermedial space would represent the initial electromotive force + or − any interfering electromotive forces.

Dulwich College,
August 1881.

XXXVIII. *On the Number of Electrostatic Units in the Electromagnetic Unit.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG to ask you very respectfully to allow me to publish a few lines regarding Dr. C. R. A. Wright's letter (the August number of this Magazine) in connexion with my paper (Phil. Mag. Dec. 1880). It is with the greatest regret that I am again compelled to trouble you with the subject, simply because of the mistakes (for which I feel extremely sorry) in the *simplest part of my paper,* namely *putting down the numerical figures* in the table (p. 434, Phil. Mag. Dec. 1880). It should stand thus:—

α.	r.	R.	R tan α.
44° 15′	75 ohms	107·88 ohms	105·09
42 45	80 "	112·88 "	104·34
51 39	50 "	82·88 "	104·76

$\therefore$ the mean value of R tan $\alpha = 104{\cdot}73 \times 10^9$ (C.G.S.).

The truth was that I was trying the experiment first with $\left.\begin{matrix}80\\100\end{matrix}\right\}$ for r, but changed them to $\left.\begin{matrix}75\\80\end{matrix}\right\}$ ohms to get more convenient deflections, and copied down wrong numbers from my notebook. As the values of R tan α were several times worked out and examined carefully before the paper was written (a close agreement between themselves having been observed), I was so certain of their accuracy that every time I read the proof I passed over the table. As to the mistake in the numbers 0·904187 and 0·03438 (p. 433, Phil. Mag. Dec. 1880), in which the decimal point should stand on the left of the first zero, I trust you will take it for granted that it is a misprint.

I have the honour to be, Gentlemen,
Your most obedient Servant,
R. Shida.

Japanese Legation, London, W.
September 4, 1881.

XXXIX. *Answer to* Dr. Wright's *Remarks on* Mr. Shida's *Measurement of the Number of Electrostatic Units in the Electro-magnetic Unit of Electromotive Force. By* Andrew Gray, *M.A., Assistant to the Professor of Natural Philosophy in the University of Glasgow.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

PERHAPS I, as one to some extent cognizant of the experiments the accuracy of which is called in question by Dr. Wright, may be allowed to say a few words in reply to his letter printed in your last number.

The discussion originated (as Dr. Wright states in that letter) in his remark, in the Philosophical Magazine for May 1881, that the assumption of the equality of the E.M.F. of a Daniell's cell when generating a current, with that of the same cell when its poles are insulated, would introduce an error in the final result of Mr. Shida's research. That this assumption was not made has been already explicitly stated by Mr. Shida; but perhaps, in view of Dr. Wright's account of the matter above, it may not be amiss to sketch shortly the method of investigation which Mr. Shida employed.

For a reason which will presently appear, he proceeded in the following manner:—(1) He compared by means of a quadrant-electrometer the E.M.F. of a battery of tray Daniells with that of the cell chosen; (2) measured by means of a Thomson's absolute electrometer the electrostatic value of the E.M.F. of the same tray battery; (3) repeated the quadrant-electrometer comparison. (Dr. Wright is therefore in error in stating in his July Note, and again in his letter of last month, that the quadrant-electrometer comparison was made before and after the *electromagnetic* valuation. Yet Mr. Shida's statement on this point is perfectly free from ambiguity.) These experiments were several times repeated, and afforded plainly a means of measuring accurately, more accurately than if the cell had been applied directly to the absolute electrometer, in absolute electrostatic units a difference of potential equal, or nearly equal, to the E.M.F. of a Daniell's cell, by a quadrant-electrometer. Thus the quadrant-electrometer, a much more convenient instrument than the absolute electrometer for rapid experimenting, and for the purpose to which it was to be applied, could be used in the remainder of the investigation, which, as appears clearly from Mr. Shida's paper, supplemented by his note in the June number, consisted in the measurement, both electrostatically and electromagnetically, of *one and the same electromotive force.*

For the electromagnetic measurement, the cell was placed in circuit with a resistance-coil and a tangent-galvanometer, and the current measured as described by Mr. Shida. But Mr. Shida has since stated that, while the current was thus flowing through the galvanometer, he measured the difference of potentials at the two ends of the external resistance by the quadrant-electrometer, and calculated from this, by a simple application of Ohm's law, the E.M.F. of the cell while generating a current. There could therefore be no question here of polarization-error; for the method is plainly equivalent to determining simultaneously, in both kinds of units, the difference of potentials between the two ends of the external resistance. These measurements, Mr. Shida has told me, he has since carefully repeated with different resistances in circuit, obtaining results which one and all differed only very slightly from the E.M.F. of the cell with its poles insulated. Hence, even if the assumption deprecated by Dr. Wright had been made, no appreciable error in the result would have been introduced. Of course I do not overlook the fact that the determination of the resistance of the cell by the ordinary method of measuring the difference of potentials between the poles of the cell when insulated, and again when connected by a known resistance, will be in error if there is a diminution of E.M.F. due to polarization in the latter case; but even a considerable percentage of error from this cause will not, in the calculation by Ohm's law of the E.M.F. of the cell when generating a current through a large resistance (such as that used by Mr. Shida), mask the diminution, if sensible, of E.M.F. produced by the current through that resistance.

These results of Mr. Shida's have been confirmed by experiments which I have myself made to determine the diminution, if any, of E.M.F. caused by polarization in a Thomson's gravity Daniell* similar to that used by Mr. Shida. By

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* This cell consists of a copper plate about 720 square centim. in area and a zinc grating of about the same area separated by a diaphragm of

an application of Poggendorff's method of compensation, in which, by applying the poles of a gravity Daniell to two such points in the circuit of two cells of the same kind that no current was caused to flow through the derived circuit in which the first cell was thus placed, the E.M.F. of the two compensating cells was determined in terms of that of the compensated cell, the resistance between its poles, and the total resistance in the circuit of the former. By using two values of these resistances, and assuming that the electromotive forces were the same in the two cases, two equations were obtained, from which the E.M.F. and resistance of the compensating-battery could be calculated. The result showed that, with a total resistance of about 10 ohms in circuit, the E.M.F. of each of the compensating-cells was diminished by only about ·6 or ·7 per cent. after the current had flowed for several minutes. The internal resistance of the two cells deduced from these experiments agreed within about 1 per cent. with that calculated in the ordinary way from the readings of a quadrant-electrometer when the battery-poles were applied to it (1) insulated, (2) connected by a resistance of 1·96 ohm.

It should be stated that Mr. Shida's later determinations of v were made exclusively with Thomson's tray Daniells. The plates of these cells are about 2 feet square, and the resistance of each from ·04 to ·1 ohm. Mr. Shida found no trace of polarization at all in these cells with the resistances he used in circuit.

How Dr. Wright could suppose for a moment that any person who had a knowledge of Ohm's law could confound the difference of potentials at the two ends of the external resistance with the electromotive force of the cell I cannot imagine; yet it is on this supposition that his long note in the July number of the Philosophical Magazine, with its allegation of a 2-per-cent. error in Mr. Shida's result, is founded. Surely no "explicit statement" on the part of any one was required that one of the commonest rules of electrodynamics was observed in the calculations!

The discrepancies, however, which Dr. Wright points out in the numbers which Mr. Shida has given in his paper are real, but admit of easy explanation. I find, on referring to a report of work done last summer furnished by Mr. Shida to Mr. J. T. Bottomley, and dated 25th July 1880, which includes an account

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vegetable parchment, contained in a large shallow glass vessel filled up with a solution of zinc sulphate. Crystals of copper sulphate are placed in a glazed earthenware vessel resting on the zinc, and communicating by a glazed earthenware tube with the liquid immediately above the copper plate.

of the experiments on which Mr. Shida's paper was founded, and which has been in Mr. Bottomley's possession ever since, the following statement of results:—

“Deflections, α. Resistance, R” (in ohms).

$$\left.\begin{array}{ll}“(1) & 44^\circ\ 15' \\ (2) & 42\ \ 45 \\ (3) & 51\ \ 39\end{array}\right\}\text{ corresponding to }\left\{\begin{array}{r}107\cdot88\\112\cdot88\\82\cdot88\end{array}\right.$$

“Therefore we get,” in C.G.S. units

$$\begin{array}{ll}“(1) & \mathrm{R}\tan\alpha=105\cdot09,\\ (2) & \mathrm{R}\tan\alpha=104\cdot35,\\ (3) & \mathrm{R}\tan\alpha=104\cdot785^*,\end{array}$$

“and the mean value of

$$“\mathrm{R}\tan\alpha=104\cdot73.”$$

It is plain therefore that Mr. Shida, in copying down from his notebook specimens of his numbers, inadvertently wrote 45° 15′ in the first column of the table for the calculation of R tan α, and in the second column gave 80 and 100 instead of 75 and 80 respectively. The table ought accordingly to stand:—

α.	r.	R.	R tan α.
44° 15′	75	107·88	105·09
42 45	80	112·88	104·34
51 39	50	82·88	104·76

which gives Mr. Shida's average value of R tan α, viz. 104·73.

I do not think that Dr. Wright's letter calls for further remark from me, except that, whatever may be the influence of these “peculiar numerical aberrations” on the minds of readers of Mr. Shida's paper, we may take it for granted that they had had no effect in leading Dr. Wright to believe that “oversights had been made in other portions of the research,” or that precautions not mentioned “were probably omitted;” for if these aberrations had been observed by Dr. Wright before he wrote his note in the July number of the Philosophical Magazine, he would probably not have forgotten them when pointing out the trifling slip in misplacing the decimal point in the statement of the electrostatic values of the cell and battery.

I have the honour to be, Gentlemen,
Your obedient Servant,
ANDREW GRAY.

Physical Laboratory, the University,
Glasgow, September 1881.

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* This value of R tan α, 104·785, has obviously been written by a slip of the pen instead of 104·755.

XL. *Intelligence and Miscellaneous Articles.*

EXPLANATION OF AN ANOMALY IN DOUBLE CIRCULAR REFRACTION. BY M. CROULLEBOIS.

THE two inverse circular rays have a separate existence outside the quartz; to be convinced of this it is sufficent to repeat the following experiment:—Taking a *biquartz* of 60 millim. thickness, we pass interfering pencils, polarized or natural, through each half; we then receive these pencils, superposed, upon the horizontal slit of a direct-vision spectroscope. Under these conditions we observe two distinct systems of curved longitudinal fringes presenting the characters of inverse circular polarization. If we explore the common field directly with the lens, we get two lateral groups of vertical fringes, resulting from the interference of rays of the same gyration unequally retarded in the biquartz.

For a long time it was possible to believe that the distance, estimated in fringes, of the two central bands gave the measure of the circular birefringence for the middle rays of the spectrum. It is far from being so; *the observed is always greater than the theoretic displacement.* For example, with a quartz of 42 millim. M. Billet* ascertained that this distance amounts to 24 fringes and is more than double the theoretic value, which would be 10·5. My present purpose is to explain this disagreement between theory and observation. Even in white light, with the lens only, the position of the central fringe is often doubtful; with the spectroscope, employed as stated above, there is no longer any uncertainty. The central fringe is always delimited by the two black lines equally inclined in both directions from the vertical. Having made this remark, let us return to the experiment, and suppose that the observer has the group of circular fringes to the left; the central band answering to the different colours will be thrown back unequally. The displacement will be given by the formula

$$f = \mathrm{K}.e(n'' - n'),$$

and will be proportional to the numbers 58, 62, 70, 80, 87, and 100 respectively for the lines B, C, D, E, F, G. Thus the central fringe of the violet is much more displaced than that of the red, and the black line (place of those central fringes) is strongly inclined upon the vertical. To the right of this singular line BG the attendant bands become more and more oblique; to the left, on the contrary, they reerect themselves; and as the distance between two successive bands is greater in the red than in the violet, on advancing towards the left we shall find two black lines inclined symmetrically: it is there that the central fringe IK of the displaced system must be formed. But how many fringes must we go forward to meet with that central fringe? Let us observe, 1st,

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* *Optique physique*, t. ii. p. 243.

that we must have IB=KB$_1$, B$_1$ being the projection of the point B upon the horizontal KG; 2ndly, that there are as many fringes of the rays G as of the rays B, from the curve BG to the vertical IK. Consequently we have the evident relation

$$42(f_G-f_B)+m\lambda_G=m\lambda_B,$$

whence

$$m=6{\cdot}81.$$

Accordingly, besides the theoretic displacement, we must advance 6·81, which gives 12·06 fringes, and consequently, in conformity with the experiment, 24 fringes for the displacement of the two groups.

With my biquartz of 60 millim. I have met with the same exaggeration. The thickness capable of producing the retardation of one wave-length upon the middle ray of the spectrum is usually estimated at 8 millim.; the distance of the centres of the two lateral systems ought therefore to be 15 fringes. Now it is found that this distance reaches the figure 34; and this exaggeration of the displacement is explained as above.

According to the formula we have

$$m=9{\cdot}7,$$

which number added to 7·5 gives 17·2, and consequently (very approximately) 34 fringes for the total displacement.

It results from the above that the abnormal displacement of the two lateral groups receives a natural explanation, and that it cannot be taken advantage of as an argument against Arago's hypothesis. —*Comptes Rendus de l'Académie des Sciences*, September 12, 1881, t. xciii. pp. 459–461.

ON THE LIGHT OF COMETS. BY M. RESPIGHI.

The appearance of the fine comets *b* and *c* 1881 has presented to astronomers the opportunity of making new researches upon the light of those stars; and the results obtained by spectroscopy and photography have appeared to confirm the idea that their light is due in part to reflection of that of the sun, and in part to a light which is the cometary matter's own.

There is no doubt as to the first part of this conclusion, since to the proofs already acquired Mr. Huggins has succeeded in adding the photograph of Fraunhofer's lines in the spectrum of comet *b* 1881; but on the second point—that is to say, on the question of knowing whether the comet emits a light of its own, due to a real incandescence, I think there is no ground yet for giving an affirmative decision. Indeed it seems to me that the discontinuity of its spectrum, as well as the bright lines or bands, may proceed from the same cause, namely reflected light, if regard be had to the modifications which that light must have undergone in passing through the gases and vapours which constitute the entire mass of a comet.

It is certain that the greater portion of the light transmitted by comets comes from the interior of their mass, and that it has traversed deep strata of gas or vapours; it must have undergone there the elective absorption proper to those vapours and to the compounds which result from them. It is therefore natural that dark lines or bands should be produced, different from Fraunhofer's lines, which are peculiar to the light of the sun. Thus, besides the faint but complete spectrum of that light, produced by reflection from the external parts where the absorption is insensible, another spectrum must be produced, coming from the deeper parts and greatly modified by more powerful absorption.

The limits of a mere Note do not permit me to enter into the details of my numerous spectroscopic observations of the comet *b* 1881; but I can affirm that they appear to confirm this view; that is to say, the phenomena do not seem to require the intervention of a light due to the incandescence of the cometary matter. Hence it would follow that the discontinuity of this spectrum comes from the same cause which gives rise to wide dark bands in the spectrum of the sun when he is near the horizon, or in the spectra of the atmospheres of the planets, with the sole difference that in the comets the phenomenon is exaggerated in consequence of the enormous thickness of the absorbing strata, their richness in chemical compounds, and the faintness of the light which they reflect toward us. It would therefore be necessary, for the spectral analysis of the comets, to proceed as we do for that of those atmospheres—that is to say, to fix our attention not so much upon the bright bands as upon those which owe their darkness to absorption.—*Comptes Rendus de l'Académie des Sciences*, 5th September 1881, t. xciii. pp. 439, 440.

ON THE MAGNETIC METALS. BY M. GAIFFE.

With the view of seeking out the best conditions for the preparation of magnets of cobalt and nickel, of which I have the honour to submit some specimens to the Academy, I tried those metals in different states.

The samples which were used for the trials were obtained by means of the electrochemical processes communicated to the Academy by Mr. Adams and me on January 7, 1870, and July 15, 1878, by employing currents of suitable intensity for rendering the metals very hard.

After dividing the samples into bars of nearly equal dimensions, I left some bars of each metal in the hard state; others were annealed; and others were annealed and forged. All were afterwards magnetized in the same manner, and the magnetism measured directly with the magnetometer. After thirty-six and seventy-two hours of rest, it was measured again.

Although only taken very approximately, these measurements (recorded in the annexed Table) will nevertheless, I hope, suffice to show what relatively great coercive force these metals, especially cobalt, can acquire in a state of purity, when it is known that pure iron, obtained by the same means, gave under the same circumstances only inappreciable deflections.

For completeness I add that some examples of cobalt which took only a weak magnetization two years since, when I made these experiments, became strongly magnetized now, without having been annealed, and having lost nothing of their original hardness.

It would *a priori* seem that the feeble coercive force of these metals when they issue from the galvanoplastic baths is due to the presence of hydrogen in combination with them, and that as soon as it has disappeared, either by the action of heat or by spontaneous liberation, nickel and cobalt resume their real coercive force. In that case the action of hydrogen would be analogous to that of the metals which are combined with nickel to constitute argentan: it would paralyze their magnetic power.

Comparative Table of the Coercive Forces of Nickel and Cobalt in different States.

Conditions of the experiments.	Deflection in the magnetometer.	
	Nickel.	Cobalt.
Immediately after magnetization:		
Bar not annealed	2° 15′	5° 30′
,, annealed	5 20	11 0
,, annealed and forged	7 0	14 45
Thirty-six hours after magnetization:		
Bar not annealed	1 45	5 0
,, annealed	3 30	9 30
,, annealed and forged	6 0	14 0
Seventy-two hours after magnetization:		
Bar not annealed	1 30	4 45
,, annealed	3 5	9 0
,, annealed and forged	5 30	13 30
,, annealed, magnetized, and again annealed	0 5	1 15
Bar annealed, magnetized, and afterwards forged	0 25	6 0

Comptes Rendus de l'Académie des Sciences, 12th Sept. 1881, t. xciii. pp. 461, 462.

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XLI. *Experimental Investigations on Magnetic Rotatory Polarization in Gases.* *By* M. Henri Becquerel*.

[Plates VII. & VIII.]

Introduction.

UNTIL within the last few years all attempts to demonstrate the electromagnetic rotation of the plane of polarization of light in passing through gaseous media have been futile, and the phenomenon discovered by Faraday in solid and in liquid bodies seemed in gases to escape all measurement. The analogy presented in the case of dilute solutions of certain substances had led to the supposition that the magnetic rotatory power of the same body remained nearly proportional to its density at the moment of its passage from the liquid to the gaseous state. Experiment has shown that this is not the case; and even whilst operating under conditions of magnetic intensity apparently sufficiently powerful, several experimentalists have been unable to observe any appreciable rotation of the plane of polarization in light.

My investigations, pursued now for several years, on magnetic rotatory polarization, have led to various conclusions with regard to gases, which it was of great interest to verify, and which enabled me to foresee the character of the rotations to be observed, and the existence of a remarkable relation between the index of refraction of bodies and their magnetic rotatory power. Based upon these considerations, an appa-

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* From the *Annales de Physique et Chimie*, [5] xxi. p. 289.

ratus was constructed sufficiently powerful to demonstrate the feeble rotations which the hypothetical calculations had led me to expect.

The results of the preliminary observations were published at the end of March 1879. The numbers obtained at that time were the first that had been given on the measurement of these phenomena, or which had established the relative magnitude of the magnetic rotatory powers in liquid and solid bodies and those in a gaseous condition.

Since that time, Bichat and Kundt and Röntgen have published various communications relative to the magnetic rotatory power of certain bodies in a gaseous state.

About the same time Lippich obtained a magnetic rotation of several seconds of arc with a column of 0·50 metre of air subjected to a strong magnetic action. Bichat more particularly confined himself to the determination of the relation between the magnetic rotatory power of carbon disulphide at various temperatures in the liquid and gaseous condition. His results agree with my own.

Kundt and Röntgen have studied the magnetic rotatory power of certain gases from a point of view quite different from mine. They compressed the gases to 250 atmospheres, while I more particularly studied them under the ordinary temperature and pressure. The German authors have obtained under considerable pressure magnetic rotations of several degrees; but the imperfection of their apparatus did not allow them to take advantage of the extent of the rotations in order to obtain very accurate measurements. They limited themselves to publishing numbers relative to five gases—hydrogen, oxygen, air, oxide of carbon, and marsh-gas; and they were unable to discover any relation between the index of refraction of the gases and the observed rotations.

In the present memoir I attempt to show that the existence of a relation between the index of refraction of bodies and their magnetic rotatory power in a solid, liquid, and gaseous state cannot be called in question. I would further record the fact that I have demonstrated the existence of a perturbation in the phenomena of atmospheric polarization, due, apparently, to a slight rotation of the plane of polarization of the luminous rays passing through the atmosphere, under the influence of terrestrial magnetism.

METHOD OF EXPERIMENT.

The study of the magnetic rotatory power of gases implies an accurate knowledge of their temperature and pressure at the moment of the experiment. The same volume of the different gases has been invariably taken. My wish to operate at the ordinary temperature and pressure very greatly complicated the apparatus, which would have been extremely simple if the experiment had been made only under very strong pressure; and I have been obliged to have recourse, therefore, to means of amplification which I shall describe more closely hereafter. The apparatus employed is composed of two distinct parts:—first, the electromagnetic apparatus; and, secondly, the optical system, the arrangement of which is of the most delicate character.

1. *Electromagnetic Apparatus.*

The apparatus is composed of a copper tube, 0·122 metre in external diameter and 3·27 metres long, closed by parallel mirrors, and around which are six large electromagnetic bobbins. The whole of this arrangement is mounted horizontally on a wooden bench formed of a large beam 3·82 metres long. This serves also to sustain the optical system (see Plate VIII. fig. 1). The tube is held by special supports in the middle of the bobbins so as not to touch them. Each of these is 0·48 metre long, 0·13 metre internal diameter, and contains 15 kilogr. of copper wire 0·03 millim. in diameter, thoroughly insulated and rolled upon a copper tube which forms the body of the bobbin. The total weight of the copper wire employed was about 90 kilogr., which corresponds to 1380 metres in length for the six bobbins.

They are associated in series, so as to form a single solenoid round the tube which contains the gas.

Into these bobbins was passed the electric current produced by 80 elements (nitric acid) of large size. The battery was formed of two groups of 40 elements arranged in series, which were united together in multiple arc.

Under the influence of an electric current so intense, the conductors and the tube itself soon become considerably heated, and the temperature rises to 30° or 40° C. It is necessary, on the one hand, to allow for this elevated temperature, and, on the other, to guard against the variations in intensity of the electric current. These variations are slow and regular, and are due to the progressive weakening of the acids of the couples, and to the heating of the conductors by the passage of the electric current. In order to allow for them, it is sufficient to measure at convenient intervals of time the intensity of the electric current and to trace the curve of the intensities. For this purpose there was arranged, in connexion wth the circuit, a sine-compass, which measured at every moment the intensity of the current in the apparatus, and which thus allowed the observed results to be reduced to what they would have been if the electromagnetic intensity had been perfectly constant. A mercurial commutator conveniently placed within reach of the observer, allowed the direction of the electric current in the bobbins to be reversed at pleasure, and the bodies studied to be subjected to inverse electromagnetic action.

The tube which contains the gases was originally 3 metres long. It was constructed to maintain a vacuum and to support the pressure of several atmospheres ; for this purpose it was terminated by very thick pieces of copper, which were screened and soldered, and in which was drilled a rectangular opening of 0·10 metre by 0·04 metre.

These pieces presented a plane surface perpendicular to the axis of the tube, and on which were fixed by means of screws flat glasses enclosed in a frame of copper. The tube was provided with taps and a manometer; and the whole was so arranged as to slip inside the bobbins.

Various inconveniences, and amongst others a slight accident, which caused the simultaneous rupture of the two glasses, led to the following modification of the apparatus.

On the two plane surfaces which form the end of the 3-metre tube (Pl. VII. fig. 1) are screwed and cemented cylindrical copper tips 0·135 metre long, furnished at their ends internally with a thread for a screw. The tube can be closed by two large copper stoppers, which are screwed into the pieces just described, and in which are cemented two flat glasses of about 0·005 metre thickness, leaving for the light a rectangular opening of 0·09 metre by 0·06 metre. Washers of greased leather enable the tube to be hermetically closed. On each side of these pieces are arranged taps, which answer the purpose of either introducing the gas or putting the tube into communication with a manometer. Before entering the apparatus, the gases are passed through tubes containing substances necessary to purify and dry them.

The tube extends beyond the bobbins by about 0·135 metre on each side, so that the glasses are subjected to only a very feeble magnetic influence : we shall see afterwards how this influence must be taken into account.

The distance between the internal surfaces of the glasses is 3·27 metres ; the volume occupied by the gas submitted to the experiment is about 37 litres. When the tube was hermetically closed it acted as a gas-thermometer, and the indication of the internal pressure allowed the average temperature of the gaseous mass to be deduced from it. The axis of the tube made an angle of about 52° with the magnetic meridian. The arrangement of the apparatus is shown in the accompanying figure (Plate VIII. fig. 1).

2. *Optical Apparatus.*

Under the conditions of magnetic intensity just described, the rotation of the plane of polarization of a luminous ray passing through the tube filled with gas is very feeble. With the various gases studied, the double rotation obtained by reversing the direction of the electric current in the bobbins did not exceed an angle of 5′. On this account it became necessary to amplify the phenomenon. Two methods might be employed. The first, contrived by Faraday, consists of passing several times through the substance to be studied luminous polarized rays which are reflected from two mirrors conveniently placed. It is well known that, under these conditions, the magnetic rotation is proportional to the distance traversed through the substance influenced, contrary to what would be observed in a body endowed with a natural molecular rotatory power. This method is the one exclusively adopted by me in the experiments about to be described.

The second method is a general method of amplification of the displacements of the plane of polarization of a luminous ray, and was proposed and applied by M. Fizeau. It consists in passing the polarized luminous ray through a pile of glasses conveniently diverged. I have tried to apply this method; and I shall state hereafter why, in the present instance, I have preferred to use the former.

Description of the Optical System.

The apparatus was specially constructed with a view to applying Faraday's method of amplification.

Source of Light.—The source of light was incandescent lime produced by an oxyhydrogen flame, and seen either directly or through different-coloured screens. I employed as screens coloured glasses—red, yellow, and copper-green—and also a liquid screen containing ammoniacal nitrate of copper, which chiefly permitted light-blue rays to pass. We shall see later how it was possible to estimate the length of the average wave of luminous rays which in each case reached the eye of the observer.

The luminous point L (see Pl. VIII. fig. 2) was placed at about 0·487 metre in front of the extremity of the tube—that is to say, at 0·627 metre from the face of the last bobbin—at the focus of a collimating-lens C, having a focus of 0·14 metre, which transmitted through a polariscope P parallel rays. The coloured screens were always placed in front of the collimating-lens.

Polariscope.—The polariscope was formed by a very fine Nicol's prism, 45·5 millim. in thickness by 45 millim. in width. The prism was cut in half lengthwise, and the two halves brought together so that their principal planes made an angle of about 5°. The field of this prism was a circle of about 0·022 metre in diameter. Such a large opening was considered to be necessary—first of all to allow a sufficient quantity of light to pass through, and secondly that the images should retain, after the successive reflections, an apparent diameter suitable for observation.

The polariscope, the collimator, and the source of light were firmly mounted on the same piece of copper, which was capable of receiving a horizontal movement, and which could also turn through a slight angle round a vertical axis, so as to cause the axis of the optical system to assume a slight inclination to the axis of the tube (Pl. VII. fig. 2).

Mirrors.— Suppose, for the sake of simplifying the description, that the glasses G, G_1 which close the tube had been removed (see Pl. VIII. fig. 2). The luminous rays, rendered parallel by the collimator, after having passed through the polariscope, graze the edge of a vertical mirror M_1, pass through the tube, and encounter at the other extremity a vertical mirror M, from which they are reflected ; they then fall upon the mirror M_1, and in this way are reflected successively on the two mirrors until the last ray reflected grazes the edge of the mirror M. The number of the successive reflections depends on the mutual inclination of the mirrors and the direction of the incident ray. We have seen how it is possible to determine the latter. As to the mirrors, they are mounted in copper frames, which are fixed by three spring screws to vertical wooden supports firmly fastened to the wooden block which sustains the apparatus. By means of the three screws the mirrors can be moved in any required direction (Plate VII. figs. 2 & 4).

If the light were polarized only in a vertical plane, the plane of the successive reflections being horizontal, it would follow that the plane of polarization of the reflected images would remain absolutely vertical.

With this apparatus the light of each half of the image is polarized in two planes equally inclined to the vertical (that is to say, to the plane of the reflections) ; each half will thus experience at each reflection an equal slight rotation, and the bisecting plane of the planes of polarization should remain about the same for all the images. Consequently, the position of the analyzer which corresponds to equality of tint should remain nearly the same for all the images. This conclusion is verified by experiment; besides, it is easy to see that, the reflections on the mirrors being nearly normal, the displacement of the planes of polarization must be inappreciable. The mirrors are made of St.-Gobain glass wrought with the greatest care and silvered on the surface. As it is necessary that they should be absolutely plane in order to avoid any deformation of the images, we have employed rather large surfaces of glass, of which we have used only the central part. The mirrors are rectangular, and are about 0·16 metre long by 0·06 metre wide and 7·9 millim. thick; they are only silvered on 0·08 metre of the middle portion (Pl. VII. fig. 3), so that the luminous rays, on coming out of the polariscope and before being received on the analyzer, pass once through the thickness of the glass of the mirrors M_1 and M. There is thus introduced into the magnetic rotations to be measured a slight correction due to the influence of magnetism on this mass of glass. The mirrors are placed at 0·195 metre from the bobbins. It is easy to see that under these conditions the correction which corresponds to the passage of the luminous ray through the substance of the mirrors is very slight. It would have been possible to silver the mirrors to the ends and cause the luminous rays to pass over their edges without passing through the glass; but I have preferred to allow this slight correction, which was easily estimated, to remain, in order to profit by the greater perfection of the workmanship and silvering of the central portion.

Analyzer.—The luminous rays, after their successive reflections, are received on an analyzer formed of a Foucault's prism set on a graduated circle, the vernier of which reads to a minute. The diaphragm of this prism is 0·017 metre. Behind the analyzer is a microscope of a rather strong magnifying-power (about 19 diameters), and which is independent of the movement of the analyzer on the graduated circle, so as not to increase the mass to be carried round in taking the measurements, and to leave to the analyzer all the freedom of movement possible (Pl. VII. fig. 4). This precaution is far from being useless in ensuring accuracy of observation. The analyzer and microscope, however, are associated in the other movements, forming a system which can be displaced laterally and inclined at a slight angle by turning on a vertical axis, so as to bring to the centre of the microscope the reflected image to be studied, and to cause the optical axis of the system to coincide with the more or less oblique direction of the reflected ray.

The various parts of this apparatus can be regulated with great nicety; and we thus see that the two mirrors present a series of images which are reflected without any deformation. Including the direct image from the polariscope, they correspond to columns of air represented by one, three, five, seven, nine times the distance of the mirrors. A tolerably large number of reflected images can be obtained; but, in order to preserve a sufficient luminous intensity, distinctness, and an apparent diameter of the image suitable for the polariscope, I have been obliged to stop at the fourth reflected image, which corresponds to nine passages of the luminous ray through the bobbins. With the magnifying-power just indicated, this fourth image is as clear as the direct image of ordinary polarimeters or saccharimeters.

Suppose, now, that the electric current is caused to pass in one direction: one of the images, the fourth for instance, must be reduced to uniformity of tint, as in ordinary polarimeters; then, if the electric current is reversed, one half of this image will be seen darker than the other. If the analyzer is then turned so as again to obtain uniformity of tint in the two halves of the image, the angle by which it has been turned will measure twice the magnetic rotation sought.

The method supposes, in theory, that the light studied is monochromatic; but in the experiment itself, as the rotations are very small, it is not necessary that the incident rays should have the same wave-length. However, we shall return later to this important question.

Under the most favourable conditions the rotations could be measured to nearly 30″.

Glasses which close the Tube.—In the experiments that we have just described the luminous rays only pass through air, and the rotation obtained represents, with the exception of a slight correction, the magnetic rotation of that gas. When the experiment is tried with other gases, it becomes necessary to close the tube by glasses, as we have above indicated.

The luminous rays pass through the glasses G, G_1 as many times as through the tube itself; and as at each passage the latter presents four surfaces, it is clear that the luminous rays corresponding to the fourth reflected image will have traversed thirty-six surfaces of glass. If to them are added the four surfaces of the mirrors, it will make forty surfaces of glass during the passage of the light. From this it is seen what must be the perfection of the workmanship of these surfaces in order that the images should not experience any appreciable distortion. In addition to this, if it happened that the glasses were even very slightly unannealed, the depolarization of the light, which would have been almost imperceptible during a single passage, would become considerable.

The glasses first used were very good; but an accident having caused their simultaneous breakage, I had much difficulty in replacing them, and was obliged to try several pairs before obtaining those that I have employed throughout the work, and which are at least as good as the first ones. These glasses are of St.-Gobain glass, about 0·005 metre in thickness, and are finished with the greatest care, so that they do not distort the images in the least degree, at any rate up to the fifth passage. Even after nine passages they do not depolarize the light to an appreciable extent, and the images can still be reduced to equality of tint with remarkable precision.

When the apparatus is not perfectly regulated, it is found that the positions of the equality of tint of the various images are no longer exactly the same as they ought to be. This arises from the fact that the rays which reach the eye for each image pass through the polariscope and the analyzer more or less obliquely. This inconvenience would be obviated by adjustment; but as the angle of the two deviations obtained by reversing the direction of the electric current is always measured, the absolute direction of the plane of polarization of the incident ray does not enter into the determinations. It is equally easy to prove that the luminous rays which pass through the glasses almost normally cannot undergo any appreciable amplification or diminution in their magnetic rotation in consequence of the phenomena of refraction of the polarized light.

However, it was proved by a special experiment that a slight rotation of the plane of polarization of the luminous rays experienced by them on issuing from the polariscope was still the same at the fourth image reflected; so that the passage through the glasses and the successive reflections did not, with the exception of the magnetic effect, introduce any appreciable correction into the measurements. The only important correction occasioned by the glasses has relation to the magnetic rotation which they cause the plane of polarization of the light to undergo. This correction was studied with the greatest care, and it will be treated of hereafter.

It would have been preferable to avoid this difficulty by placing the glasses further away from the bobbins; but this arrangement would have tended to make the size of the apparatus too large. It would also have been possible to use only the four centre bobbins; but the diminution of the distance traversed by the light under magnetic influence was not compensated for by the increased intensity of the current of the pile circulating in a solenoid of less resistance.

I preferred to utilize all the bobbins I possessed, and to ascertain the correction of the glasses by special experiment, submitting them to a known magnetic action of considerable power, and then reducing the results observed to what they would be under the conditions of the measurements with the gases.

The presence of the glasses in the tube in the course of the luminous rays gives rise to another inconvenience which complicates the regulation of the system. Each surface acts as a mirror, and a large number of reflected images appear in the field of vision. However, they cannot be confounded with the images reflected in the mirrors, because of their faint lustre; and they are nearly obliterated when the analyzer is turned so as to obtain equality of tint.

Nevertheless it sometimes happens that these accidental images encroach on the images to be studied; and as the parts which are superposed have not the same plane of polarization, the optical measurements become very difficult, and sometimes impossible. It is then necessary, by manipulation, either to displace the mirrors a little, or to wedge the tube in a slightly different position, so that the accidental images do not interfere with the measurements to be effected; usually the accidental images are situated a little above or a little below the images reflected by the mirrors.

When the apparatus was regulated, it was left as long as possible in the same position. At the end of each day of experiments the mirrors, which would be affected by the air, were simply removed, and then replaced in exactly the same position when a fresh series of observations was begun.

The gases were introduced into the tube without altering its position, by simple displacement under the influence of a slight pressure. The greatest care was taken to avoid any pressure which would either distort the glasses or cause them to exhibit the phenomena of chromatic polarization.

Almost the whole series of experiments below recorded were made without displacing either the tube or the glasses from their position, which remained the same for several months.

The difficulties of construction presented by the very delicate regulation of all the instruments, as well as by the workmanship of the surfaces of the glasses and mirrors, have been surmounted with rare skill by M. T. Duboscq, who made the apparatus.

On the Employment of Piles of Amplifying-glasses.—In order to augment still further the magnetic rotations observed, the polarized rays might be received on one or more piles of glasses placed in front of the analyzer.

It is known that, if the pile is suitably placed, a slight rotation of the incident plane of polarization can be considerably amplified.

I had two piles of glasses made, constructed exactly according to the directions given by M. Fizeau *; but I did not find, in the present case, that the use of them gave us all the results that we had a right to expect.

In the first place, it is clear that, if a polariscope of the kind described is employed, it is necessary to have a prism the two planes of which only make a very slight angle ; for this angle is itself increased by passing through the piles of glasses, and it may happen, as in the case of the polariscope described above, that all sensitiveness disappears. In fact, the piles of glasses gave an amplification of nine times a slight rotation of the incident plane of polarization ; and the principal planes of the two halves of the image from the polariscope were considerably thrown out. I attempted to make use of a large Nicol's prism furnished with *a half-wave-length plate*, so as to form a polariscope of variable sensitiveness ; but this apparatus necessitates the employment of a monochromatic source of light, and did not give good results. Even with monochromatic light the two halves of the image are not in the same physical conditions ; the one is polarized elliptically, and the other rectilinearly. For such delicate measurements as those here spoken of, I very much prefer the Nicol cut in half. If a suitable polariscope had been sufficient, I could have introduced this arrangement with the same precision as the method of amplification by the mirrors. But the use of piles of glasses has, in the present case, a much more serious objection. I had intended to use them concurrently with the mirrors, so as to obtain rotations of several degrees ; but unfortunately the slightest variation in the angle made by the incident rays with the normal of the piles of glasses changes the amplification considerably, so that it is not the same for the different images. It would then be necessary, after each measurement, to determine experimentally the amplification due to the piles of glasses, which would greatly complicate the observations. Besides, this amplification is not the same for rays of different colours ; it is thus necessary to make use of a source of light almost perfectly monochromatic and, if the rotations of rays having different wave-lengths are studied, to measure experimentally in each case the amplification which corresponds to them. It will hereafter be seen what difficulties arose from the want of homogeneity in the source of light we were obliged

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* *Annales de Chimie et de Physique*, viii. p. 153 (1859).

to employ. The corrections due to this class of phenomena with the piles of glasses were thus very complex.

This multiplicity of experimental measures and the uncertainty as to the real degree of amplification in each case, take away all the advantage of the large rotations which may be obtained, and do not permit a greater relative approximation. Therefore, in order not to multiply to excess the very delicate precautions with which the measurements were already surrounded, I relinquished the idea of making use of piles of glasses.

On the Distribution of Magnetic Intensities in the Apparatus.—Magnetic Rotation of a Column of liquid Carbon Bisulphide.

It is well known that the magnetic rotations of the plane of polarization of light are proportional to the magnetic intensity; the numbers given below will have real significance only when the corresponding magnetic intensity is accurately determined.

This can be estimated in absolute measure ; but the determination offers many difficulties; and it is much simpler and more accurate to measure the magnetic rotation of a well-defined body under the same conditions of magnetic intensity as those used for the experiments, and to compare the numbers obtained with the magnetic rotation of this body taken as unity.

In my previous researches on magnetic rotatory polarization I had already taken as unity the rotation of the plane of polarization of the yellow rays of the flame of sodium traversing a column of liquid carbon bisulphide at a temperature of 0°, with a thickness equal to that of the body studied, and subjected to the same magnetic intensity. In the present experiments I have kept to the same unit.

It thus became necessary to measure the magnetic rotation obtained with the yellow light of sodium passing through a column of carbon bisulphide equal to the length of the distance traversed by the luminous rays in the apparatus above described. Now, at different points of the line traversed by the luminous rays the magnetic intensity is not the same : in the inside of the large solenoid formed by the six electromagnetic bobbins it is nearly constant; but it decreases very rapidly outside these bobbins, and is no longer appreciable at about 0·20 metre from their extremities.

This exterior action is very weak compared with the magnetic influence of the interior of the solenoid ; nevertheless, on account of the very peculiar conditions of the experiments, it was important to note carefully the variation in intensity with the distance, and the precise value of this intensity at different points.

In fact, I found that it was necessary to interpose in the course of the luminous rays plates of *glass* about 0·005 metre in thickness. Now, these glasses have a magnetic rotatory power which, for the yellow light, is three thousand times that of air. It is, then, conceivable that, in spite of the feeble magnetic intensity to which they are subjected, these plates may lead to corrections which it is necessary to ascertain as accurately as possible.

In order to effect these various determinations, I caused a copper tube 0·50 metre long to be constructed, and placed it in the centre of the different bobbins. The exact position of one of the extremities of the tube was fixed by a small reading-microscope; the tube was then slipped along so as to bring the other end into precisely the same place as that occupied by the first. The magnetic rotation of the column of carbon-bisulphide was ascertained in each position; and the sum of the rotations obtained was equal to the rotation of a single column having the same length as the solenoid.

The magnetic rotation was measured by the yellow light of soda.

I have stated above that the magnetic intensity varied in a continuous manner, in consequence of the variations in the intensity of the electric current. In all the measurements I have reduced the results to what they would have been if the current had been constant and given in the sine-compass a deviation of 24°.

The continual elevation of the temperature in the interior of the bobbins during the passage of the very intense electric current employed, introduces a most important correction into the measurements. This elevation of the temperature decreases the magnetic rotatory power of the carbon bisulphide, first by reducing its density, secondly by a direct effect independent of dilatation. This question has been studied with various liquids by M. de la Rive, and with carbon bisulphide by M. Bichat, who, to express the rotation of this body at various temperatures, has given the formula

$$1-0{\cdot}00104\,t-0{\cdot}000014\,t^2.$$

If dilatation alone intervened, it would give

$$\frac{1}{1+0{\cdot}0011398t+0{\cdot}000001370\,t^2}.$$

Up to about 12° the two expressions do not differ by a unit in the third decimal place; beyond that the difference is marked. I have assumed that the first formula expresses tolerably approximately the progress of the phenomenon; and I have reduced the rotations observed to what they would be at a temperature of 0°. The numbers obtained differ by only a few minutes of arc from those which would result from the application of the formula of dilatation of the carbon bisulphide.

The following table contains the numbers obtained for the rotation of carbon bisulphide in the interior of the different bobbins. The multiplicity of the measurements in each case enables us, as an average, to count upon an accuracy of some tenths of a minute; the hundredths of a minute, resulting from the calculation of the averages, have been recorded.

Magnetic Rotations of Carbon Bisulphide (yellow light D).

	Average temperature of each series.	Rotations reduced to the temperature of 0°.	Rotations due to the glasses which close the 0·50-metre tube.
1st bobbin	15°·0	716′·21	1′·88
2nd "	17·2	746·39	2·51
3rd "	20·6	770·04	2·60
4th "	22·6	767·83	2·60
5th "	23·5	747·97	2·51
6th "	23·6	702·41	1·88
Total	......	4450·85	13·98

The glasses which close the tube of 0·50 metre are of crown glass, and are about 1·5 millim. thick. The magnetic rotation which they cause the plane of polarization of the light to undergo is very feeble. It was ascertained directly, the tube being empty of liquid; and the rotations are recorded in the last column of the preceding table. The sum of these corrections is 13′·98, and it must be subtracted from the total amount of the rotations observed.

It is equally important to subtract from each measurement the rotation due to magnetic influence on the column of air traversed by the luminous rays. In accordance with what will hereafter be shown, this rotation at 20° and at a pressure of 760 millim. amounts to about 0′·58, say 3′·48 to be subtracted from the sum of the rotations.

Placing the 0·50-metre tube on the outside of the first bobbin under the same magnetic conditions and at a temperature of 14°, a rotation was obtained which, corrected by that of the air, was found to be 42′·41; this at 0° would give 43′·17. It is, however, useless to attempt to estimate the magnetic intensities at a greater distance from the bobbins; for a column of 0·25 metre, on issuing from the first bobbin, gives, at a temperature of 15°, 40′·6 (say 41′·37 at 0°), a rotation which is nearly equal to that of the 0·50-metre tube.

Finally, the variation in intensity with the distance was studied by observing the rotation of a heavy piece of flint glass, in our previous researches designated as glass No. 1 (Feil). This piece was 50·028 millim. long. It was placed endwise in three positions: the first was such that one of the faces coincided with the exterior surface of the bobbin. The following numbers, corrected for the rotation of the air, were thus obtained:—

Distance of one of the faces of the piece of flint glass from the first bobbin.	Magnetic rotations.	
	Of the flint glass.	Of an equal thickness of bisulphide of carbon.
millim.		
1st …… 0	31′·35	23′·49
2nd …… 50	14·34	10·74
3rd …… 100	6·73	5·00
Total ………	……	39·23

In the third column, the rotations of an equal thickness of carbon disulphide have been calculated. The numbers thus obtained give facilities for verification, since their sum ought to represent the rotation of a column of 0·15 metre of carbon bisulphide. Now, experiment has given 39′ for the rotation of 0·15 metre of carbon bisulphide under these conditions; and the preceding amount is 39′·2. This verification is as satisfactory as it can be. By means of these data it has been possible to construct a curve which gives with great accuracy the rotation of a definite thickness of carbon bisulphide at any distance from the bobbin. We have used this curve, as will be shown, for the determination of various corrections which might affect the direct measurements.

In short, it is clear that, supposing a luminous ray traverses a column of infinite length of carbon bisulphide submitted to the action of the six electromagnetic bobbins previously described, it will be enough, in order to take into account the exterior action of this solenoid, to add to the rotations obtained above 43′·17 × 2 = 86′·34.

In making all the corrections just indicated, it may be said that, for luminous rays traversing a column of carbon bisulphide contained between the polariscope and the analyzer, and submitted to the electromagnetic action previously defined, corresponding to 24° of deviation in the sine-compass, the plane of polarization of the yellow rays D would undergo by the reversal of the electric current a rotation of 4520′ = 75° 20′.

This number appears exact to about 10′; for the different rotations measured are exact to nearly 1′; and the only uncertainty that could give rise to an error of a few minutes is the error which might have been made regarding the successive positions of the tube in its displacement inside the bobbins.

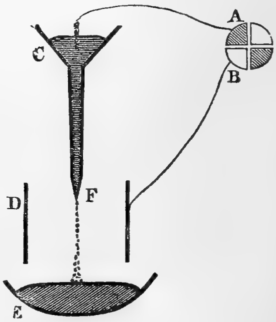
When the numbers obtained for the gases are compared with this last number, the error on the rotation of the carbon bisulphide does not amount to 0·002. This precision is greater than that which I have been able to realize in the measurements relative to the gases.

[To be continued].

XLII. *On some Capillo-electric effects, and a Theory of Contact-Potential.* *By* S. Lavington Hart, *B.A., D.Sc., Scholar of St. John's College, Cambridge**.

HAVING occasion, during the course of some experiments on contact-potential described further on, to use an electrometer, one electrode of which (A) was in connexion with an iron wire dipping in mercury contained in a funnel (C), to the neck of which was adapted a fine glass nozzle, while the other electrode (B) was put to an iron cylinder (D) around the falling mercury drops, I observed that there was a deflection of the needle showing A to be positive to B by an amount equal to about ·6 D (D being the electromotive force of a sawdust Daniell). This deflection remained the same if the electrode B was put to earth

Fig. 1.



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* Communicated by the Author, having been read (in Abstract) before Section A of the British Association (September 1881).

by an iron wire, or if this was dipped in the mercury collected in the cup E.

In this latter case there were attached to both electrodes iron wires dipping in mercury, exposed to the air, and in almost identical conditions—the only difference being in the *capillary* relations of the mercury contained in the cup and of that in the funnel and neck.

The question arose, *Is the formation of drops at the end of a very fine tube accompanied by electrical phenomena such as to cause a positive charge to be gradually accumulated in the mercury-reservoir?*

The effect observed was not due to the electrification produced by *friction* against the glass; for this effect is of much smaller dimensions, and the mercury would be negatively charged.

The case of these falling drops is in many respects analogous to that of mercury advancing in a capillary tube. I tested therefore the behaviour, in such a tube, of a column of mercury broken by an air-bubble, by connecting the cups in which dipped the ends of the tube to the two electrodes of an electrometer, then suddenly raising one cup.

I found that the advancing column was always positive to the retreating mercury; and by multiplying the effect by inserting many air-bubbles a deflection was obtained, when the rate of flow was sufficient, corresponding to ·7 D. (The frictional charge, as shown by a low-resistance galvanometer and an undivided column of mercury, was extremely slight.) In such an apparatus the retreating end is oxidized, the coating of oxide appearing after prolonged action, while the advancing end is reduced and acquires a positive charge.

Similarly, the advancing drop in the apparatus first described beomes positively charged, *so long as any reduction takes place.* This depends on previous exposure to the air and other circumstances which favour oxidation, but also on an action which takes place more or less continuously in the following way:—The end of the thread, after the drop has detached itself, suffers a momentary retraction; and this therefore causes a slight oxidation of the surface, which, however, is not of sufficient duration to lower the potential of the mass of mercury in the column to any extent; for, the forward motion at once setting in, the oxide locally deposited is reduced, this reduction being equivalent to a positive charging; and, *since the drop takes time to form,* the positive charge is communicated to the column, which thus becomes more and more charged by each forward motion of the column, so long as oxidation can occur immediately after each drop has fallen.

The positive potential obtained must then depend on the degree of oxidation possible—that is, on the rate of fall in some way or other, and upon the medium around the drops, as well as the degree of insulation obtained, &c. This was found to be the case—so much so that, by increasing the oxidation of the drops by fanning or breathing, a much greater deflection (·8 D) was obtained, while by substituting coal-gas for the air around the drops the deflection could be reduced to zero.

It thus appears that, in air, a convex advancing thread of mercury is reduced, this being accompanied by a positive charging of the mercury column, the reverse taking place in the retreating end.

This behaviour is precisely similar to that observed in capillary electrometers; and the tube with the divided column of mercury bears a strong resemblance to the electrometer of the Scott-Dewar form. Thus another point of analogy can be traced between the action of liquid and that of "gaseous electrolytes" on the metals placed in them; and it is to attempt to refer these two sets of phenomena to the same cause, and to discover, if possible, the same laws which govern them, that this paper has been written.

The influence of the atmosphere upon the difference of potential supposed to be set up between metals has been recognized. Thus, Professors Ayrton and Perry remark that only in a vacuum could the true contact-potential be determined. Further, this influence has been clearly shown to exist, and to bear not only on the quantitative measurements, but also upon the actual *order* of the series, as was proved by Brown (Phil. Mag. August 1878) in the case of an iron-copper couple placed in air and also in hydrogen-sulphide gas: a "gaseous electrolytic cell" is thus formed; and its behaviour is similar to that of a "liquid cell," the electrolyte being water in the one case and hydrogen-sulphide solution in the other.

The analogy is sufficiently striking to invite one to seek for some one general law which may lead more or less satisfactorily to the one cause of these electrical phenomena; but the real justification of such a step will depend on the measure in which it enables one to explain already known cases and also to predict others.

The ordinary way in which a body acquires a certain potential is by receiving an electric charge. Have we not sufficient ground to refer the potential-differences of metals and electrolytes in question to the charges accompanying the electrolytic ions as these go to one or other of the metals, and thus to lay down the following rule?—

That metal to which the electro-$\left\{\begin{matrix}\text{positive}\\ \text{negative}\end{matrix}\right.$ *ion* goes, receiving an electro-$\left\{\begin{matrix}\text{positive}\\ \text{negative}\end{matrix}\right.$ *charge,* acquires thereby an electro-$\left\{\begin{matrix}\text{positive}\\ \text{negative}\end{matrix}\right.$ *potential* relatively to that of the electrolyte.

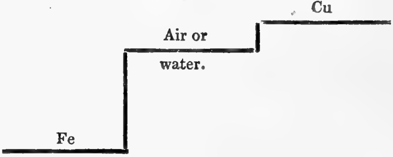
Thus, when iron and copper are each of them placed alone in water, both metals are oxidized, though differently: both therefore are negative, though by different amounts, to the water; but if these metals are placed in the same water, the iron alone is oxidized, the copper has hydrogen deposited on it and becomes therefore positive to the iron, as before, but in addition is positive to the acid.

We have as yet no means of estimating the actual difference of potential between an electrolyte and any metal; for the measurement of the Peltier effect on the passage of a current is the only way of determining this difference, and such a method is impracticable. Since there exists no proof of the contrary, I have assumed that when two metals are introduced into an electrolyte, the potential of the latter is intermediate between those of the two metals. Now, if the production of potential depends on the charges brought by the dissociating ions, this assumption must necessarily represent the actual case; and it is moreover borne out in the experiments that follow.

We have therefore two cases to consider, whether in liquids or in gases:—The metals (say iron and copper) are in the same electrolyte and

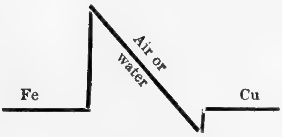
(1) *Insulated.* In this case the iron is negatively charged by the electronegative ion, the copper is coated with hydrogen (in the case of *liquid* electrolytes, and also in that of *gases*, if the distance between the metals is sufficiently small to overcome in part electrolytic diffusion), and thus is at a positive potential relatively to the electrolyte.

Fig. 2.



(2) *Touching one another.* The metals are then at the same potential; but the same differences of potential between metal and electrolyte must obtain, and this is only possible when the potential around the iron is higher than around the copper.

Fig. 3.

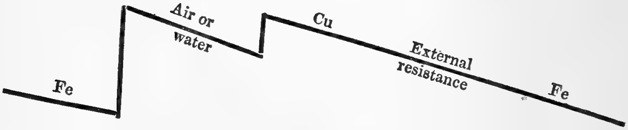


This difference of potential, in air, has usually been regarded as that existing between the two metals; but in experiments similar in principle to Sir William Thomson's half-disk apparatus, it is the potential of the air or gas that has been determined, and not that of the metal.

In water, this difference of potential has been shown by Brown (Phil. Mag. Feb. 1879).

A third, intermediate state of affairs exists, namely that in which the metals are connected through a considerable external resistance. Then the fall of potential is distributed according to the external and internal resistances.

Fig. 4.



Two suppositions have been made; and these require to be confirmed by experiment:—

(1) Metals in contact are at the same potential, though differences of potential exist in the medium (electrolytic) around them.

(2) Metals in the same electrolyte and in sufficient proximity, when insulated, are at different potentials, though the medium may be at one intermediate potential between them.

We have thus to prove:—

I. The equality of potential of metals touching (say in air).

I used the apparatus described in the first part of the paper, and, as mentioned, obtained a deflection depending on the rate of fall, the amount of oxidation, and consequently the medium around the drops. The cause of this has been explained; but by substituting coal-gas for the air about the drops (and also the exposed surfaces, as that of the mercury in the funnel) the oxidation is more and more reduced, till the positive charge passes into a negative one. (It is necessary, to obtain this effect, that the drops run slowly.) That is to say, when the medium is neutral and no chemical action is set up, no deflection at all is observed. What is the condition of affairs at this point?

The electrode B (fig. 1) is in connexion with an iron cylinder, which, not being oxidized in this medium, is, according to the theory, at the potential of the neutral gas around it. The change in the medium at this juncture is such that no oxidation or reduction takes place; the mercury therefore (according to the theory) receives neither positive nor negative charge, and is therefore likewise at the potential of the air. The only remaining possible cause of deflection being the contact of iron and mercury (to both electrodes are attached iron wires)—since there is no deflection observed, there is no difference of potential between the iron and the mercury (except perhaps that answering to the slight Peltier effect).

II. The influence of the presence of one metal on the potential of the other, and the difference of potential thereby set up.

In the case of the liquid cell, this influence has accidentally been proved by experiments previously performed. Thus, Perry and Ayrton find that the air near copper touching water in which is also dipped an iron bar, is at a higher potential than the air about the iron; for the air oxidizes *both* metals, though unequally (for the copper is out of the reach of the influence of the iron through air); whereas the water oxidizes the iron, rendering it negative to itself, and therefore renders the copper positive by the deposit of hydrogen which clings to it. The air being positive to both metals, is thus necessarily at a higher potential about the copper than about the iron. Thus, though copper would get oxidized in water alone (as it does in the air), the presence of iron prevents its oxidation in the water, whence the difference of potential amounting to ·16 volt.

Now this same influence ought to be felt in air, if the distance is sufficiently small to overcome to a sensible extent electrolytic diffusion; that is, iron, brought quite close to a less electro-negative metal, ought to reduce the oxidation going on at the surface of that metal. For this purpose the same apparatus was used; and when the deflection was steady, a clean iron bar was put to earth and brought up to the nozzle and laid as close as possible to the falling drops. For each approach of the iron, the deflection, which, as shown, depends on the oxidation of the drops, was reduced by thirty divisions, equivalent to about ·15 D. Thus, through a film of air of less than 1 millim. thickness, this influence of one metal on the other can be made evident.

I repeated this in another way; for I put the bar to the electrode B (fig. 1) and connected the electrode A to earth: the change in the deflection showed that the iron is rendered more negative (by about ·15 D) by being brought quite close up to the mercury. The oxidation of the electro-negative element is increased by the presence of another less negative metal; which proposition is the counterpart of the preceding one.

Hence, in gaseous as well as in liquid electrolytes, metals, unless touching, are at different potentials, dependent on this mutual influence as well as on the amount of chemical action set up by the one, more electro-negative, which becomes negative to the electrolyte, and still more so to the electro-positive metal.

As a conclusion, these two points, I fancy, hold generally for gases and liquids:—

(i) The metal to which goes the electro-$\left\{\begin{array}{l}\text{positive}\\ \text{negative}\end{array}\right.$ ion becomes, by the charges brought by the dissociated atoms, electro-$\left\{\begin{array}{l}\text{positive}\\ \text{negative}\end{array}\right.$ relatively to the electrolyte; and

(ii) Metallic contact entails equality of potential.

The simplicity of these laws is evident, as also the advantage of bringing back to generally received laws of action the phenomena of potential due to contact of metals and electrolytes.

I wished to have added one or two experiments to the above, which I wanted also to repeat to get surer quantitative determinations, and to render the conditions more conducive to accuracy; but the time at my disposal was limited, as well as the opportunities for performing the experiments.

St. John's College, October 1881.

XLIII. *On Curves of Electromagnetic Induction.*
By W. Grant[*].

[Plates IX., X., & XI.]

In the month of June 1879 I communicated to the Physical Society a series of measurements of the conjugate positions of two equal circular coils[†] of wire whose axes were parallel to each other—that is to say, measurements of the relative positions of the coils when they were so placed that their coefficient of mutual induction became nothing. I also showed how, by the aid of these measurements, a curve could be drawn such that when one coil remained fixed, and the other was moved with its centre always in the curve and its axis parallel to that of the first coil, the mutual induction between the coils retained the constant value zero. An obvious extension of this investigation was to try to trace out some of the

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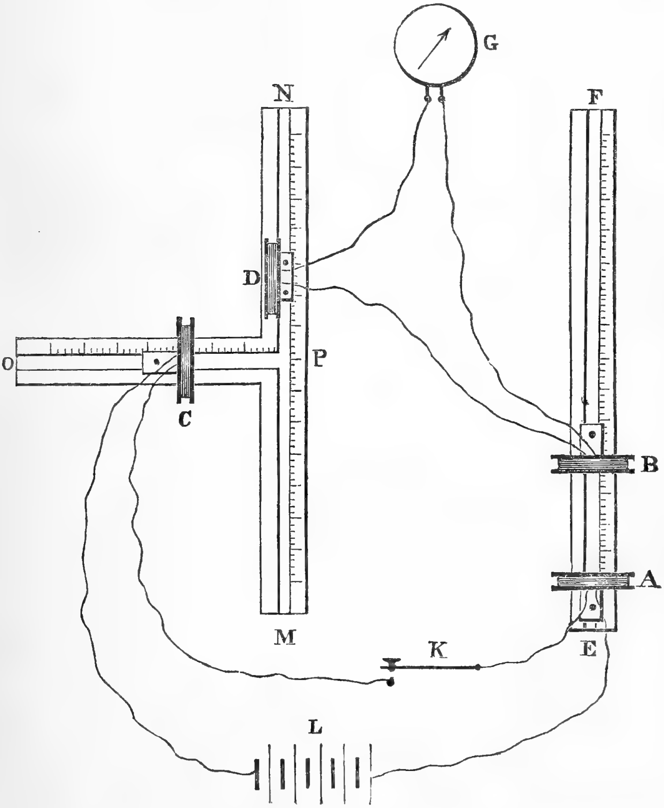
[†] Phil. Mag. for November, 1879.

curves of constant positive or negative induction lying on opposite sides of the curve of no induction; and it is the results of experiments made for this purpose, along with those of others which were made for the purpose of tracing some of the curves of constant induction between the coils with their axes perpendicular to each other, which I have now to bring before the Society.

The arrangement of the apparatus and the nature of the experiments will be easily understood by reference to the annexed diagram.

A, B, C, and D are four coils of wire made as nearly identical in all respects as possible. Each coil consists of 182 turns of No. 24 silk-covered copper wire, of 0·065 centim. diameter, wound in a rectangular groove in a flat wooden reel. The inner circumferences of the coils are 21·95 centim.; their



outer circumferences are 28·2 centim. The depth of the section of the coils perpendicular to the axis is 0·9947 centim.; the width of the groove parallel to the axis is 1·0 centim.: the section of the coils is therefore approximately one square centimetre. The mean radius is 3·99, or approximately 4·0 centim. The resistances do not agree quite so closely; but their mean gives R=2·4 ohms at 11°·5 C.

The coils are mounted on straight guides which have grooves in their upper surfaces, along which the coils can be made to slide; and their positions are indicated by scales attached to the guides. The readings are obtained to centimetres and millimetres direct, and tenths of a millimetre by estimation. The coils A and B are mounted with their axes coincident on a straight guide E F, of sufficient length to allow their centres to be separated to a distance of 70 centimetres. The coils C and D are mounted on a pair of guides O P and M N, O P being about half the length of M N, and attached to it at right angles midway between its ends. The axes of the coils are parallel to O P and perpendicular to M N: hence C can be moved parallel and D perpendicular to the axes without destroying the parallelism.

The coils A and C were connected with the battery L and with the key K to form the primary circuit; the coils B and D were connected with the galvanometer G, or sometimes with a telephone, to form the secondary circuit. Now it is evident that with this arrangement, in order that there may be no current in the secondary circuit on making and breaking the primary one, the coils C and D must be so placed that their mutual induction is equal to that between A and B; and, with reference to the secondary circuit, that the electromotive forces induced in the coils B and D must oppose each other. Hence, when A and B were clamped at a fixed distance apart, every pair of positions in which the coils C and D could be placed, so as not to cause a deflection of the galvanometer on making and breaking the primary circuit, were positions in which their mutual induction was constant, and determined one point of a curve of constant induction. Beginning with the coil D at P, the middle point of the guide M N, the coil C was moved until the induction between C and D balanced that between A and B. C was then moved nearer and nearer to P, being shifted a centimetre or so at a time; and the balance was restored in each case by moving D towards M or N. Now, as the arrangement is symmetrical with respect to the axis of C, it is evident that when a balance was obtained by displacing the coil D on one side of P, it could also be obtained by moving it to the corresponding position on the other side. In all cases these two balancing positions were found; and the half-difference of the readings on the scale M N, which was numbered from one end, was taken as the result of the experiment. In this way the measurements obtained were independent of any uncertainty in the determination of the point P. In using the numbers thus obtained for plotting a curve of constant induction, the distances read off along O P were taken as abscissæ, and the half-differences of the pairs of readings along M N were taken as ordinates. Thus in all the figures which accompany this paper, the axis of x is taken as coinciding with the axis of the primary coil with its centre fixed at the origin of coördinates, and the curves of constant induction are to be taken as representing the paths traced out by the centre of the secondary coil in a plane containing the axes of x and y. The induced electromotive force changes sign when the centre of the secondary coil passes from one side to the other of the curve of no induction. In what follows, the induction is reckoned positive when the axes of the primary and secondary coils coincide. Hence, in the figures, all the curves lying between the axis of abscissæ and the zero-curve are to be taken as curves of positive induction; while those which lie further from the axis of abscissæ than the zero-curve are to be taken as curves of negative induction. The negative curves were traced experimentally in the same way as the positive curves, except that the two leading wires of one of the four coils were interchanged. Thus the inversion of electromotive force, due to the relative positions of the coils C and D, was counteracted by the inversion of the connections; and the inductive action between A and B could be balanced by that between C and D, just as in the measurements which gave the curves of positive induction.

The positive and negative divisions of the set of curves already alluded to, which, in what follows, are spoken of as the first set of curves, were each traced out with the apparatus arranged as in the diagram. But in order to trace out another set of curves, which are afterwards spoken of as the second set of curves, the axis of the coil D was set at right angles to that of C, but otherwise every thing remained unaltered. The zero-curve of the second set coincides with the axes of x and y; and therefore it did not require to be traced; each of the other curves is complete in one quadrant. The induced electromotive force changes sign when the centre of the coil D passes from one side to the other of the axes of x or of y: hence in the figures the second set of curves are to be taken as positive in one quadrant and negative in another alternately.

The measurements for the second set of curves were made experimentally in the same way as for the first set; but as the axes of the coils C and D were at right angles, there was a considerable part of the field near the origin in which no measurements could be obtained, on account of the coils coming into contact with each other.

The positive or negative induction corresponding to any one of the plotted curves being equal to that between A and B when placed at some definite distance from each other, it became necessary, in order to place the curves so that the numerical value of the induction-coefficient might have a known relative for each, to determine the law according to which the induction between the two coaxal coils A and B varied with the distance between them. For this purpose the coil A was connected with a contact-key and with a battery of 20 Grove's cells to form the primary circuit; the secondary circuit was formed by connecting the coil B with a contact-key and with the coils C and D, which were placed close together and used as a galvanometer. The needle and mirror employed were rather heavy: this had the advantage of allowing the induced currents to exert their full effect on the needle before it had moved perceptibly from its position of equilibrium. The key was a double-contact key, so arranged that on depressing the key the primary circuit was completed; and immediately afterwards, when the current was established, the secondary circuit, which was already complete, was opened, and was kept open by a spring acting on the lower key, while the primary circuit was then broken. The time occupied in depressing and releasing the key was in general about one third of a second; hence the time during which the primary current circulated every time that contact was made was approximately that period. With this arrangement only the induced current on making was allowed to act on the galvanometer, while that on breaking was prevented from circulating in the secondary circuit.

The temperature of the primary coil was indicated by a delicate thermometer divided to tenths of a degree Centigrade, which had its bulb placed in contact with the silk covering of the coil. The variation of the temperature of the coil, due to the heating effect of the current during the experiments (as indicated by this thermometer) was approximately $1^\circ{\cdot}0$ C. The scale-readings obtained at the highest and lowest temperatures were found to agree so closely that it was unnecessary to make any correction on account of the variation of the temperature of the primary coil: its mean temperature during the experiments was $11^\circ{\cdot}5$ C.

As the state of the battery was liable to change and the strength of the current to vary, it was important that errors arising from these causes should as far as possible be eliminated. In order to effect this, a double set of observations was taken, first with increasing distances between the coils, and next with decreasing distances. In every position deflections were taken towards both ends of the scale; so that the recorded results represent the means of never less than four readings; and in most cases they are the means of twelve or sixteen. The observations were begun with the coils A and B as close together as possible without touching, and were continued till the distance between them was 70 centim.; but beyond about 50 centim. the galvanometer above mentioned, formed by the juxtaposition of the coils C and D, was not sufficiently sensitive. It was therefore replaced for these distances by a delicate reflecting galvanometer; and the necessary reduction was made after the ratio of the indications of the two instruments had been found. The deflections of the needle were indicated by the movements of a spot of light on a circular scale of one metre radius. The readings were obtained in centimetres and millimetres, and, after having been corrected for the effect of damping, were reduced to degrees. The value of the induced electromotive force, or, in other words, the strength of the induced current, was in each case taken as being proportional to the sine of half the angle through which the needle was deflected—that is, proportional to the sine of one fourth of the corrected scale-reading reduced to degrees. In using the numbers thus obtained for plotting a curve which graphically represents the values of the coefficient of mutual induction M for different distances of the coils A and B, the distances read off along the scale E F for the positions of the coil B were taken as abscissæ, and the values obtained for the sines were taken as ordinates. The results of these experiments are given in Table I., where the columns headed x give distances between the coils, and the columns z give the corresponding values of the coefficient of mutual induction. The curve plotted by means of these numbers is given in fig. 1, Plate XI.; the centres of the small circles in that and the following curves represent the points found in the experiments.

This curve is related to the curves of constant induction which form the main subject of this paper as the vertical section of a surface is related to the contour-lines of that surface. Imagine, then, a surface such that the three rectangular coordinates x, y, and z of any point upon it represent respectively the distance of that point from the centre of the

Table I.

Distance between centres of coils $=x$.	Relative value of induction-coefficient $=z$.	Distance between centres of coils $=x$.	Relative value of induction-coefficient $=z$.
centim.		centim.	
1·6	39·567	15·0	0·9
1·7	37·731	16·0	0·758
1·8	35·936	17·0	0·631
1·9	34·016	18·0	0·557
2·0	32·251	19·0	0·48
2·2	29·551	20·0	0·423
2·5	25·593	21·0	0·364
3·0	21·194	22·0	0·321
3·5	17·341	23·0	0·284
4·0	14·468	24·0	0·25
5·0	10·227	25·0	0·224
6·0	7·449	30·0	0·131
7·0	5·504	35·0	0·083
8·0	4·163	40·0	0·056
9·0	3·158	45·0	0·040
10·0	2·528	50·0	0·029
11·0	2·021	55·0	0·022
12·0	1·641	60·0	0·017
13·0	1·338	65·0	0·013
14·0	1·093	70·0	0·011

primary coil, x being measured parallel to the axis of the coil, y perpendicular to it, and z being taken equal to M, the coefficient of mutual induction between the primary coil and the secondary coil placed with its centre at the point x, y, o. Then, the curve just described may be viewed as a section of this surface in a plane containing the axes of x and z, the curves of constant induction may be looked upon as contour-lines of the surface, or as sections of it in planes parallel to the plane of x and y, and the curve whose coefficient of mutual induction is equal in value to z will pass through the point in question.

The values of M, whether positive or negative, are synonymous with those of z: hence in the figures the curves of variable induction, which are situated in vertical planes, are to be taken as positive if they are above the plane of x and y, and negative if they are below it. Where the values of the coordinates of any curve are represented in the Tables by x and y, the curve is situated in a horizontal plane; where they are represented by x and z, the curve is situated in a vertical plane. On examining the numbers in this Table, it appears that the values of z or M are approximately inversely proportional to the cube of the distance from the centre of one coil to the mean circumference of the other—that is, that the product Mc^3 is approximately constant, c being put for $\sqrt{a^2+x^2}$, where a is the mean radius of one of the coils. On closer examination it is seen that this product decreases somewhat from $x=0$ to about $x=a$, and then slowly increases for greater values of x.

By means of the curve (fig. 1, Plate IX.) it was easy to place the coils A and B so that their coefficient of mutual induction might have any desired value within the available range, and thus to assign determinate relative values to the coefficients of mutual induction corresponding to the curves to be traced out by the coils C and D. In Table II. the *first* and *fourth* columns give reference numbers referring to the several curves plotted in fig. 2, Pl. X., whose coordinates are given in Table III.; the *third* and *sixth* give the corresponding values of M, the coefficient of mutual induction; and the *second* and *fifth* the distance along the scale E F at which the coils A and B had to be clamped in order to obtain these values.

TABLE II.

No. of curve.	x.	M.	No. of curve.	x.	M.
1	0·5	Uncertain.			
2	1·0	Uncertain.			
3	2·025	32·0	13	31·6	− 0·125
4	3·72	16·0	14	24·0	− 0·25
5	5·78	8·0	15	18·7	− 0·5
6	8·15	4·0	16	14·45	− 1·0
7	11·05	2·0	17	11·05	− 2·0
8	14·45	1·0	18	8·15	− 4·0
9	18·7	0·5	19	5·78	− 8·0
10	24·0	0·25	20	3·72	−16·0
11	31·6	0·125	21	2·025	−32·0
12	......	Zero.			

The position on the scale E F, in which the coil B had to be placed in order to give the required value to the induction-coefficient of any curve, was found by inspecting Table II.; the coil was clamped in the position indicated; and a number of balancing pairs of positions of the coils C and D were then found.

In determining the values of the coordinates of the curve of zero induction the coils C and D only were employed. The primary circuit was the same as that in the diagram, omitting the coil A; the secondary circuit was also the same as that in the diagram, omitting the coil B. The coil C was brought as near as possible to the point P, and clamped in that position, sufficient room being left for the coil D to pass without touching it. D was then moved to a balancing position on one side of P, and then to the corresponding position on the other side of it, and the half-difference of the readings on the scale M N was taken as the value of the ordinate, the reading on the scale O P for the position of the coil C during the experiment being taken as the value of the corresponding abscissa.

The least distance between the centres of the coils C and D at which readings could be obtained when their axes were coincident was 1·6 centimetre, as their thickness prevented a nearer approach; the least distance between their axes at which readings could be obtained, when the mean planes of the coils were coincident or at a less distance than 1·6 centim. from each other, was about 9·2 centim. There was, therefore, in each quadrant a rectangular area of which these dimensions are the sides within which no results could be obtained.

In order to extend the experimental curves within this area, two coils, whose centres could be brought within 2 millim. of one another, were constructed from the remainder of the piece of wire from which the other four were made. Their diameters were approximately the same as those of the other coils; but, owing to their thinness and the consequent small number of convolutions of wire, their coefficient of mutual induction was much less; but this defect was compensated to some extent by their proximity. The lines of force due to them were slightly different in form from those due to the thick coils; but as in most positions of the thin secondary coil the lines of force passed through it nearly at right angles, there was less objection to the use of the thin coils than if they had been at a greater distance apart. They were fitted one to each of the coils C and D, so that the same mountings served for both, and the readings were obtained from the same scales. That which was fitted to the former we shall call C_0, that which was fitted to the latter we shall call D_0.

In continuing the measurements within the area already mentioned, the coils C_0 and D_0 were placed in the same balancing pair of positions which C and D had occupied when the last point of a given positive curve was determined. The coil B was then moved towards F along the guide E F to a position in which the mutual induction between A and B balanced that between C_0 and D_0; this gave the desired value to the induction-coefficient of the curve; and the remaining points were found as in previous cases. Without again shifting the coil B, a negative curve, the induction-coefficient of which had the same relative value as that of the positive curve, could either be continued or completely traced out by interchanging the leading wires of the coil D_0 and passing it to the negative region. Thus the curves, whether positive or negative, could either be continued or completely traced out with the thin coils just as with the thick ones; and a constant value could be given to the mutual induction between the coils C_0 and D_0 in each pair of balancing positions, proportional to, although not the same as the value assigned to M for a given curve. Hence the path traced out by the centre of the coil D_0 was practically the same as that which would have been followed by the centre of the coil D, had it been possible to use it in tracing the same curve in that part of the field. The value of the induction-coefficient of each of the two curves nearest to the origin is uncertain, as the value of z which corresponds to that of x for either of these curves is beyond the range of the curve, fig. 1. A convenient, although uncertain value was given to the induction-coefficient of either of these curves by placing D_0 at the point P and moving C_0 towards O until the distance between the centres of C_0 and D_0 was equal to that between the origin and the point where the curve was required to cross the axis of x, then balancing by the coil B and finding the remaining points of the curve. In completing several of these curves for which the value of M was comparatively small, it was found that the mutual induction between A and B was greater than that between C_0 and D_0, even when B was placed at the extreme end of the scale E F. Hence, in order to reduce it in such cases, B was laid down flat on the guide and displaced towards E or F until a balance was obtained; and although the value of the induction-coefficient was uncertain, the mutual induction between A and B was equal to that between C_0 and D_0 when they occupied a balancing pair of positions previously occupied by C and D, and remained constant while A and B retained the same relative positions.

The coordinates of the first set of curves of constant induction are given in Table III., and are numbered in accordance with Table II.

The results obtained by means of the thin coils C_0 and D_0 are distinguished in the Table by being enclosed in square brackets [ ].

Table III.

Curve 1, M uncertain.		Curve 2, M uncertain.	
x.	y.	x.	y.
[0·2]	[0·97]	[0·2]	[1·77]
[0·3]	[0·81]	[0·5]	[1·43]
[0·4]	[0·58]	[0·7]	[1·14]
[0·5]	[0·0]	[0·9]	[0·67]
		[1·0]	[0·0]
Curve 3, M=32.		**Curve 4, M=16.**	
[0·2]	[3·06]	[0·2]	[4·35]
[1·0]	[2·4]	[1·0]	[4·07]
1·6	1·59	1·6	3·74
1·9	0·88	2·0	3·46
2·025	0·0	3·0	2·37
		3·5	1·35
		3·72	0·0
Curve 5, M=8.		**Curve 6, M=4.**	
[0·2]	[5·17]	[0·2]	[5·65]
[1·0]	[5·07]	[1·0]	[5·69]
1·6	4·985	1·6	5·695
2·0	4·885	2·0	5·705
3·0	4·505	3·0	5·68
4·0	3·855	4·0	5·5
5·0	2·705	5·0	5·15
5·5	1·66	6·0	4·53
5·78	0·0	7·0	3·5
		7·5	2·69
		8·0	1·345
		8·15	0·0
Curve 7, M=2.		**Curve 8, M=1.**	
[0·2]	[5·89]	[0·2]	[6·0]
[1·0]	[6·0]	[1·0]	[6·15]
1·6	6·105	1·6	6·325
2·0	6·195	2·0	6·475
3·0	6·43	3·0	6·89
4·0	6·585	4·0	7·28
6·0	6·495	6·0	7·84
8·0	5·69	8·0	7·84
9·0	4·935	10·0	7·25
10·0	3·685	12·0	5·925
10·5	2·73	13·0	4·685
10·8	1·8	14·0	2·885
11·05	0·0	14·3	1·555
		14·45	0·0

Table III. (*continued*).

Curve 9, M=0·5.		Curve 10, M=0·25.	
x.	*y*.	*x*.	*y*.
[0·2]	[6·07]	[0·2]	[6·12]
[1·0]	[6·24]	[1·0]	[6·29]
1·6	6·445	1·6	6·515
2·0	6·63	2·0	6·715
3·0	7·18	3·0	7·325
4·0	7·745	4·0	8·04
6·0	8·79	6·0	9·475
8·0	9·435	8·0	10·66
10·0	9·61	10·0	11·465
12·0	9·33	12·0	11·845
14·0	8·455	14·0	11·825
16·0	6·845	16·0	11·45
17·0	5·55	18·0	10·56
18·0	3·49	19·0	9·9
18·5	1·8	20·0	9·05
18·7	0·0	21·0	8·05
		22·0	6·775
		23·0	4·85
		23·5	3·2
		23·8	1·74
		24·0	0·0

Curve 11, M=0·125.

x.	*y*.
[0·2]	[6·14]
[1·0]	[6·32]
1·6	6·545
2·0	6·76
3·0	7·42
4·0	8·215
6·0	9·965
8·0	11·635
10·0	13·025
12·0	14·075
14·0	14·82
16·0	15·2
18·0	15·2
20·0	15·0
22·0	14·45
24·0	13·55
25·5	12·525
27·0	11·175
28·0	10·05
29·0	8·725
30·0	6·83
30·5	5·5
31·0	3·95
31·4	2·05
31·6	0·0

Curve 12, M=zero.

x.	*y*.
[0·2]	[6·15]
[1·0]	[6·35]
1·6	6·575
2·0	6·8
2·5	7·135
3·0	7·5
4·0	8·38
5·0	9·395
6·0	10·505
7·0	11·675
8·0	12·89
9·0	14·115
10·0	15·4
11·0	16·745
12·0	18·055
13·0	19·45
14·0	20·775
15·0	22·15
16·0	23·54
17·0	24·9
18·0	26·275
19·0	27·65
20·0	29·0
21·0	30·376
22·0	31·775
23·0	33·13
24·0	34·5

Table III. (*continued*).

Curve 13, M=−0·125.			Curve 14, M=−0·25.		
x.	y.	y'.	x.	y.	y'.
0	......	26·0	0	......	20·22
[0·2]	[6·16]	......	[0·2]	[6·18]	......
[1·0]	[6·38]	25·97	[1·0]	[6·42]	20·105
1·6	6·615	25·86	1·6	6·64	19·97
2·0	6·845	25·81	2·0	6·89	19·83
3·0	7·6	25·5	3·0	7·72	19·35
4·0	8·58	25·0	4·0	8·855	18·56
5·0	9·795	24·35	5·0	10·475	17·365
6·0	11·275	23·4	5·7	12·4	15·75
7·0	13·33	21·95	5·9	14·05	14·05
7·7	15·325	19·95			
8·0	18·05	18·05			

Curve 15, M=−0·5.			Curve 16, M=−1·0.		
0	......	16·27	0	......	13·305
[0·2]	[6·2]	......	[0·2]	[6·24]	......
[1·0]	[6·48]	16·21	[1·0]	[6·56]	13·125
1·6	6·725	15·975	1·6	6·865	12·85
2·0	7·0	15·79	2·0	7·195	12·6
3·0	7·96	15·09	3·0	8·73	11·27
4·0	9·62	13·74	3·2	9·49	10·605
4·4	11·9	12·33			

Curve 17, M=−2·0.			Curve 18, M=−4·0.		
0	......	11·085	0	......	9·445
[0·2]	[6·35]	......	[0·2]	[6·64]	......
[1·0]	[6·7]	10·835	[0·5]	[6·81]	9·33
1·6	7·215	10·4	[0·7]	[7·02]	9·225
2·0	7·825	9·845	[1·0]	[7·445]	[8·85]
2·2	8·48	9·18	[1·2]	[8·15]	[8·15]

Curve 19, M=−8·0.			Curve 20, M=−16·0.		
[0·2]	[7·165]	[8·42]	**0**	**7·45**	**8·0**
[0·3]	[7.33]	[8·29]	**0·1**	**7·52**	**7·95**
[0·4]	[7·6]	[8·02]	**0·15**	**7·7**	**7·83**

Point of maximum negative intensity, No. 21, M=−32·0.	
x.	y.
0	**7·7**

The first set of curves of constant induction are given in fig. 2, Plate X. Curve 20 and the point of maximum negative induction could not be found experimentally; they are therefore only given as near approximations. The rectangular space near the origin shows the region within which the auxiliary coils C_0 and D_0 (see p. 338) had to be employed. These curves represent the contour-lines of one quadrant of the surface above described, and show at what distance from the origin any curve cuts the axis of y. Knowing this, and knowing also the relative value of M or of z for each curve, we obtain a set of values for the coordinates of a curve which may be viewed as a section of the surface in question in a plane containing the axes of y and z. These coordinates are given in Table IV.; the curve plotted by means of them is given in fig. 3, Pl. XI.; but the dotted part of it is only approximate.

TABLE IV.

y.	z.	y.	y'.	z.
2·6	39·55	6·15	26·0	− 0·125
3·07	32·0	6·17	20·22	− 0·25
4·38	16·0	6·19	16·27	− 0·5
5·18	8·0	6·23	13·305	− 1·0
5·64	4·0	6·33	11·085	− 2·0
5·88	2·0	6·58	9·44	− 4·0
5·99	1·0	7·02	8·48	− 8·0
6·06	0·5	7·45	8·0	−16·0
6·11	0·25	7·7	7·7	−32·0
6·13	0·125			
6·14	0·0			

In order to give a better idea of the symmetry of the curves of constant induction than can be got from fig. 2, the curves in that figure have been repeated in the other quadrants, and are given in fig. 4, Pl. XI., for the whole of the magnetic field of the primary coil. Now every thing is symmetrical relatively to the axis of x, and each curve represents a section of a surface of revolution about that axis. Hence, if the curves are supposed to revolve round the axis of x, a number of surfaces of revolution will be generated, each of which will be a surface of constant induction, the surfaces of positive induction being separated from those of negative induction by the surface of no induction. The positive surfaces may be described as shells which enclose one another, and each of which turns inwards, closing up round the axis of x on each side of the origin. The zero-surface, which divides the positive from the negative surfaces, instead of closing up, may be supposed to extend in space to an infinite distance. The negative surfaces turn outwards and close up in a plane containing the axes of x and z; they are hollow circular rings which enclose one another, and which have their common centre at the origin of coordinates.

In the series of measurements for the determination of the second set of curves, the axis of the coil D was perpendicular to that of C, but otherwise every thing was arranged as in the diagram. The same values were given to M for these curves as for the others, by setting the coil B in the same positions on the scale E F which it had occupied during the previous experiments. These curves are numbered in accordance with Table V.

TABLE V.

Number of curve.	x.	M.
1	31·6	0·125
2	24·0	0·25
3	18·7	0·5
4	14·45	1·0
5	11·05	2·0
6	8·15	4·0

The zero-curve of this set coincides with the axes of coordinates; and, in general, when two similar coils with their axes perpendicular are employed, one as primary and the other as secondary, their mutual induction is zero when the axis of the one lies in the mean plane of the other. This shows that any curve of the second set for which the value of M is small must make a near approach to the axes of coordinates in the neighbourhood of the origin, and that a part of each curve in that region cannot be traced experimentally with the coils employed. The coordinates of these curves, as far as it was found possible to determine them experimentally, are given in Table VI.; but there is with this set a larger space near the origin within which the dimensions of the coils made it impossible to get measurements than was the case with the first set of curves. In the figures, the curves are continued conjecturally within this space by dotted lines. The second set of curves are given in fig. 5, Pl. X.

TABLE VI.

Curve 1, M=0·125.

x.	*y*.	*y'*.	*x*.	*y*.	*y'*.
0·06	......	5·02	5·5	0·05	21·88
0·1	......	7·1	6·0	0·05	22·22
0·2	......	9·26	8·0	0·12	23·3
0·3	......	10·45	10·0	0·23	23·85
0·4	......	11·34	12·0	0·47	24·15
0·5	......	12·1	14·0	0·84	23·97
0·7	......	13·3	16·0	1·35	23·49
1·0	......	14·54	18·0	2·3	22·57
1·5	......	16·2	20·0	3·67	21·1
2·0	......	17·37	22·0	5·72	18·9
3·0	......	19·18	23·0	7·29	17·05
4·0	......	20·48	24·0	10·25	13·48
5·0	......	21·42			

Curve 2, M=0·25.

x.	*y*.	*y'*.	*x*.	*y*.	*y'*.
0·1	......	5·09	5·0	......	17·26
0·2	......	7·18	5·5	0·1	17·54
0·3	......	8·28	6·0	0·11	17·77
0·4	......	9·16	8·0	0·24	18·36
0·5	......	9·77	10·0	0·53	18·45
0·7	......	10·74	12·0	1·03	18·02
1·0	......	11·82	14·0	1·9	17·1
1·5	......	13·13	16·0	3·44	15·45
2·0	......	14·13	17·0	4·55	14·22
3·0	......	15·55	18·0	6·51	12·27
4·0	......	16·54	18·4	8·0	10·4

Curve 3, M=0·5.

x.	*y*.	*y'*.
0·21	......	5·4
0·3	......	6·4
0·4	......	7·24
0·5	......	7·84
0·7	......	8·72
1·0	......	9·72
1·5	......	10·88
2·0	......	11·65
3·0	......	12·82
4·0	......	13·59
5·0	0·17	14·1
5·5	0·19	14·28
6·0	0·23	14·38
8·0	0·5	14·54
10·0	1·07	14·06
12·0	2·19	12·87
13·0	3·17	11·82
14·0	4·69	10·17
14·5	6·34	8·35

Curve 4, M=1·0.

x.	*y*.	*y'*.
0·4	......	5·42
0·5	......	6·1
0·7	......	7·02
1·0	......	7·92
1·5	......	8·89
2·0	......	9·57
3·0	......	10·52
4·0	......	11·06
5·5	0·39	......
6·0	0·46	11·46
8·0	1·01	11·04
10·0	2·33	9·64
11·0	3·85	8·11
11·4	5·0	6·9

Table VI. (*continued*).

Curve 5, M=2.			Curve 6, M=4.		
x.	*y*.	*y'*.	*x*.	*y*.	*y'*.
0·8	......	5·54	1·6	......	5·5
1·0	......	6·17	1·8	......	5·76
1·6	......	7·27	2·0	......	6·0
2·0	......	7·76	2·5	......	6·45
3·0	......	8·54	3·0	......	6·72
4·0	......	8·91	4·0	......	6·96
5·0	......	9·0	5·0	......	6·82
5·2	0·69	......	5·5	1·55	6·66
6·0	0·92	8·85	6·0	1·9	6·31
7·0	1·37	8·41	6·5	2·45	5·78
8·0	2·23	7·58	7·0	3·62	4·61
9·0	4·13	5·56			

The positive and negative curves of the second set have the same form, and are positive in one quadrant and negative in another alternately. They have been treated as an independent set of curves; but, as will afterwards appear, they are not so, but are merely a special case in which the positive curves of the first set have become separated by the intervening zero-curve which coincides with the axes of x and y. In this case any corresponding pair of positive and negative curves, besides having the same form, have also the same linear dimensions. In every other case the form and linear dimensions of any corresponding pair of positive and negative curves are different, the linear dimension of the negative curve being always less than that of the positive. Hence this is the case in which the linear dimension of any negative curve is greatest, and in which that of the corresponding positive curve is least. The second set of curves, for the whole of the magnetic field, are given in fig. 6, Pl. IX., which shows the relative positions occupied by them in the various quadrants.

When a system of curves of constant induction have been obtained for a given pair of coils, they may be used to give the total inductive effect produced on one of the coils by a given change of relative position while the other coil is traversed by a current of known strength. To simplify the statement, suppose the primary coil in which there is a current of uniform strength C to remain at rest while the secondary coil is moved from a position such that the coefficient of mutual induction is M_1, to a position in which this coefficient becomes M_2. Then, if t is the time occupied by the movement, the average electromotive force e which acts in the secondary circuit during the movement is

$$e=\frac{M_2-M_1}{t}C;$$

and if r be the resistance of the secondary circuit, the average strength of the secondary current is

$$c=\frac{e}{r}=\frac{M_2-M_1}{rt}C.$$

The total quantity, q, of electricity conveyed by the secondary current is consequently

$$q=ct=\frac{M_2-M_1}{r}C,$$

and is therefore independent of the time occupied in the displacement of the secondary coil.

The values of M_1 and M_2, the coefficients of induction corresponding to the initial and final positions of the secondary coil, can be obtained by inspection of diagrams of curves of constant induction such as those which accompany this paper. It is evident that the total secondary current is nothing in every case in which the secondary coil is moved so as to make $M_2=M_1$ (that is, whenever the secondary coil in its final position is on the same curve as in its first position), and that the secondary current is positive when the absolute value of M_2 is greater than that of M_1, and negative when M_2 is less than M_1.

The spaces between the curves in the figures may be filled up as follows by other curves, for which the values of M lie within the available range of the curve fig. 1. Assume a value for M which will lie between its values for two curves already drawn; take this as the value of z, and find the point of the curve fig. 1 to which it corresponds. A point on the axis of x will thus be found, which will divide the distance between the two points on that axis, whose values are those of x for the given curves, into two parts whose ratio may easily be found. Then, if a series of points between the two given curves are found for which this ratio is constant, a curve joining these points will be a curve of constant induction, for which the value of M will be that which was assumed.

The curves of constant induction may also be employed in a graphic method for determining, at a number of points in the magnetic field, the direction of the resultant inductive effect on the secondary coil in the positions in which it was used in the measurements for determining the two sets of curves. If the curves figs. 2 and 5 are superposed as in fig. 7, Pl. X., we may resolve by the parallelogram of forces at a number of points where the curves intersect. For this purpose components are taken whose values are proportional to the values of M for the curves, and, from the point where any two curves intersect, lines are drawn proportional to these values for the two intersecting curves. The component which is proportional to the value of M for the curve of the first set is drawn parallel to the axis of x; and that for the curve of the second set is drawn at right angles to it. Compounding, we then obtain as resultant a straight line proportional to the magnitude, and in the direction, of the resultant inductive effect on the secondary coil in two positions at right angles to each other. The arrows at the points of intersection of the curves in fig. 7 show the directions of these resultants. If the curves had been obtained by means of an indefinitely small secondary coil, the resultants would have been tangents to the lines of force, and the straight lines drawn through the same points at right angles to the resultants would have been tangents to the equipotential curves; but with the coils employed this is not strictly the case.

The two sets of curves in the figures are so related to one another that the one set may be viewed as a modified form of the other. If we begin with the axes of the primary and secondary coils parallel, and gradually increase the angle between them, the curves of maximum induction, as it were, carry the curves of constant induction round with them, while the curves of zero induction, in moving round, always form lines of demarcation which separate the positive from the negative regions. The positive curves of constant induction at the same time become distorted and gradually contract, the linear dimension of each curve becoming less until the angle between the axes is about 90°. When this angle has become exactly 90°, the positive curves of constant induction have become separated into two distinct divisions by the intervening zero-curve (which coincides with the axes of x and y), so that they then occupy two opposite quadrants. Now, both the positive and the negative curves move round together; but as the negative curves are carried round they become distorted and gradually expand, the linear dimension of each curve becoming greater until the angle between the axes of the coils is 90°. When this, which is the extreme or limiting case, has been reached, the form and linear dimension of any negative curve are the same as those of either of the two corresponding positive curves for which the value of M is the same, and the two sets of negative curves, which originally were each bisected by the axis of y, have then come to occupy the two remaining opposite quadrants. Thus the one set of curves of constant induction merges into the other, and the forms of both sets, which may be described as ovals, depend upon the angle between the axes of the primary and secondary coils.

We see, then, that the second set of curves, although found experimentally, are not entirely independent of the first set, and that being positive and negative in alternate quadrants, they have no corresponding surfaces of revolution, that the only curves of constant induction which have corresponding surfaces of revolution are those which are obtained when the axes of the primary and secondary coils are parallel, and that all curves of constant induction which are due to the coils when their axes make any other angle with each other are merely modified forms of them.

The arrangement of apparatus employed in this investigation, namely two similar coils in the primary and two other similar coils in the secondary circuit, was employed by Dove in his researches, and was called by him the "Differential Inductor" (*Annales de Chimie et de Physique*, tome iv. 1842).

The same arrangement was employed by Felici, a short account of whose researches is given in Maxwell's 'Electricity and Magnetism,' vol. ii. pp. 169–172.

In conclusion, my special thanks are due to Prof. G. C. Foster, for the general interest which he has taken in this subject, and for many important suggestions made during the progress of the investigation.

XLIV. *On a New Polarizing Prism.*

By Professor Silvanus P. Thompson, *B.A.*, *D.Sc.*[*]

1. NEITHER the polarizing prism of Nicol nor that of Foucault can be regarded as perfect. The latter especially has so small an angular aperture available, as to be very inconvenient for any but narrow beams of parallel light. The author has sought to improve upon the existing forms of polarizing prism; and his investigations into the cause of their defects have led him to produce prisms having a considerably wider effective angular aperture.

2. In the text-books it is usual to tell students that in the Nicol prism the ordinary ray is suppressed by total reflexion because the ordinary index of refraction is greater than that

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[*] Communicated by the Author, having been read before Section A of the British Association, September 7, 1881.

of balsam, and that the extraordinary ray is transmitted because the extraordinary index of refraction is less than that of balsam. Neither of these statements is completely true. All that its inventor claimed for the Nicol prism*, and all that it actually performs, is as follows:—The critical angle of total reflexion being different for ordinary and extraordinary rays, the ordinary ray is totally reflected and thrown out of the field at an incidence at which the extraordinary ray is still transmitted, the available field of polarized light being the region between the points where the extraordinary ray itself vanishes by total reflexion and the ordinary ray enters by lack of total reflexion. The former limit is in all ordinary Nicol prisms marked by a broad blue iris or band of colour; the latter is delimited by a curved band at the opposite side of the field, in which, amidst a prevailing line of red and orange, a system of interference-bands can be seen. The existence of these interference-fringes was examined by the author in 1877, in a paper which appeared in the Proceedings of the Physical Society of London, vol. ii. p. 157. In the Foucault prism a similar limitation of the field occurs, interference-fringes being visible at both limits.

3. The refractive index of balsam for light of mean refrangibility may be taken as 1·54, that of the ordinary ray in calc-spar as 1·66, that of the extraordinary ray as 1·487. The reciprocals of these are very nearly in the respective proportions of 65, 67, 60. The extraordinary index, however, is 1·487 only for rays at right angles to the crystallographic axis, having there a minimum, and increasing up to 1·66 for rays whose direction coincides with that of the axis. The ellipsoidal wave-surface of the sheet of extraordinary waves lies partly without and partly within the spherical wave-surface for canada balsam, while the spherical wave-surface of the sheet of ordinary waves lies wholly within. Hence total reflexion may occur for the extraordinary as well as for the ordinary rays; but of the extraordinary rays only those can suffer total reflexion which are situated in such a direction with respect to the optic axis that their corresponding portion of the ellipsoidal wave-surface lies within the spherical wave-surface for balsam. As the Nicol prism is usually constructed, this limit of possible extraordinary total reflexion occurs for rays (in a principal plane of section) inclined at about 10° to the balsam film, giving rise to the limit of the polarized field marked off by the blue iris before mentioned.

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* See Edinburgh New Philosophical Journal, 1828, p. 23, W. Nicol, "On a Method of so far increasing the Divergency of the two Rays in Calcareous Spar that only one Image may be seen at a time."

4. The author has succeeded in widening the available field of polarized light by constructing polarizing prisms in which this blue iris, and the total reflexion of the extraordinary ray which produces it, are got rid of. This can be done by cutting the crystal so that (1) the balsam film lies in a principal plane of section, and (2) the crystallographic axis is at right angles to the axis of the prism.

The result of this mode of orientation of the axis and film is to gain 9° of angular aperture at this side of the "field," supposing the angles respectively made by the film and by the terminal planes with the axis of the prism to be the same as in the Nicol prism.

It is possible to produce a further increase in width of available aperture at the other side of the field by reflecting back the ordinary ray more than in the Nicol prism by making the terminal faces more oblique; but there is then more loss of light by reflexion at the surfaces.

5. Beside the advantage of a wider angular aperture, this new form of polarizing prism has the advantage of producing a field in which the rectilinear polarization approximates more uniformly and symmetrically to a polarization in one plane than is the case in the ordinary Nicol. There is, however, more waste in cutting the spar, with proportionate increase in cost.

XLV. *Phenomena of Binaural Audition.*—Part III.
By Professor SILVANUS P. THOMPSON, *B.A.*, *D.Sc.*[*]

1. IN the author's papers of 1877 and 1878 he stated that, when two separate simple tones reach the ears without previously mingling, the difference-tone, or grave harmonic, is not heard, though beats are if the tones are within beating-distance. The latter statement referred to the beats of imperfectly-tuned unisons, which was the only case of primary beats examined at the time[†].

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[*] This paper, read before Section A of the British Association at York, continues the researches communicated to the British Association by the Author in 1877 and 1878, and printed in this Magazine. See p. 274, Oct. 1877, and p. 383, Nov. 1878.

[†] This phenomenon of subjective interference, which was announced in the author's paper of 1877, and further discussed in his paper of 1878, was independently announced on Nov. 28, 1877, by Professor Graham Bell as having been discovered by himself and Sir W. Thomson (subsequently to the publication of the author's first paper). The phenomenon was also known in 1874 to Mach, who mentions it at the end of a paper on the Functions of the Pinnæ of the Ears (published in the *Archiv für Ohrenheilkunde*), and ascribes it to a conduction of the sound through the mass of the skull.

2. The beats of mistuned consonances of the form $n:1$, which were not explicitly mentioned in the author's papers, nor at that time subjected by him to detailed examination, have lately claimed attention on account of the revival of the dispute between acousticians of two schools concerning the origin of difference-tones, or grave harmonics. Dr. König of Paris still consistently advocates the theory of Smith and of Young—that the beats (which represent in frequency the difference between the vibration-frequencies of the two interfering tones) pass, when occurring sufficiently rapidly, into beat-tones (as he calls the differential tones). All mathematical acousticians agree, on the other hand, that this is physically impossible if the tones are pure and of so small an amplitude that the squares and higher powers of the displacements may be neglected. A very important paper on this matter by Mr. Bosanquet has lately appeared*, in which the following statements are made:—(1) That the beats and difference-tones of imperfect consonances other than unisons are *subjective*†; (2) that the beats consist, as König discovered in 1875, of variations in the intensity of the *lower* of the two interfering tones; (3) that if the squares and higher powers of the displacements be not neglected in the equations, a term appears having a period whose frequency is the difference of the frequencies of the generating tones, as required by Helmholtz's theory of the difference-tones, and corresponding to König's *lower* beat-notes. In this last we have the apparent reconciliation of the experiments of König with the theory of Helmholtz. To his former experiments König has now added a fresh series with

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* Phil. Mag. June 1881, and Supplementary Number, June 1881.

† I am not quite sure whether I entirely understand the sense in which Mr. Bosanquet uses the word *subjective*. If he means by this term that the phenomena of beats and difference-tones exist only in the mind, brain, or nerve-structures of the ear, being generated in the sensory apparatus by something which physically has no existence, being in fact only phantoms of the imagination, then I entirely differ from him. But if by *subjective* Mr. Bosanquet means that the existence of these phenomena, though physically and mechanically true, is limited to the receptive mechanism of the ear, then I beg in the first place to disagree with such a perversion of the adjective, and in the second place to deny that any such limitation exists. The beats are *objective*: they can be *seen* in a manometric flame if the primary tones are sufficiently loud. The difference-tones of the mistuned consonances in question are also *objective*, inasmuch as a suitable asymmetrical resonator will reinforce them. What is the ear but a complex resonator upon whose parts vibrations from without can be forced? And if, by reason of asymmetry in the ear, the higher terms of the displacements can "by transformation" generate resultant displacements whose periods are those of the tones in question, and force them upon the sentient apparatus, then the same tones can, for precisely the same reason, be taken up by any other suitable asymmetrical resonator.

a new instrument—the *wave-siren*[*]. In this instrument disks, upon whose edges have been cut the wave-forms of mistuned consonances, are rotated in front of a nozzle through which air is blown, and yield, when rotated slowly, *beats*, but when rotated rapidly, *beat-tones*.

3. The author has recently submitted to examination by binaural hearing the case of mistuned consonances of the form $n : 1$, with the following results.

(*a*) *Mistuned Unison.*—Two stopped organ-pipes (C' of 4-foot octave), sometimes blown from one source, sometimes from two, were used, one being kept to exact tuning, the other raised and lowered in pitch by altering position of stopper or by holding an obstacle to the lip of the pipe. A glass funnel was placed in front of each pipe; and two indiarubber tubes led the sounds to the ears independently. *Beats* were heard, which ceased if either pipe was nipped. The rise and fall of the individual tone of the one pipe could also be distinguished. The beats appeared to wander from ear to ear.

(*b*) *Mistuned Octave.*—A similar arrangement applied to the mistuned octave entirely confirmed the second point of Mr. Bosanquet's research. Whether the upper or the lower note of the octave interval was tampered with, and whether raised or lowered, beats were heard, and those beats consisted exclusively of variations in loudness of the *lower* note. That ear which received the higher note heard merely variations in pitch when that note was tampered with, heard no variations either of pitch or of intensity when the lower note was tampered with. The ear which received the lower note heard merely variations of intensity (beats) when the upper note was tampered with, but heard variations both of intensity and pitch when the lower note was tampered with.

Two forks giving C' and C'' (mistuned) were next tried side by side in a room, funnels placed opposite them being connected to separate ears. Here the sounds mingled to some extent before reaching the ears, and beats were heard by both ears; but very careful listening showed that, as before, the beats were variatious of loudness of the lower tone. When this was relatively much the louder of the two, the beats were best heard in the ear listening to the higher fork. When the higher fork was the louder of the two, the beats were much more distinct to the ear listening to the lower.

The experiment was then repeated, but with the essential difference that the forks were sounded in different rooms, and their sounds conveyed to the observer by indiarubber tubes through the key-holes of the doors. No sound was audible

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[*] Wied. *Ann.* March 1881, Bd. xii. Heft 3, p. 335.

except through the tubes. Under these circumstances very distinct beats were heard; and they were beats exclusively of the lower note. Here, however, a very curious subjective phenomenon presented itself, similar to that noted in the author's earlier researches in the case of unisons in different phase: the beats of the lower notes did not seem to be mere maxima and minima of loudness; they were heard as a moving of the lower tone backward and forward from the ear to the back of the head.

(*c*) *Mistuned Twelfth.*—For this interval the arrangement with stopped organ-pipes was reverted to, the higher note being raised and lowered in pitch. To one ear the higher note simply rose and fell in pitch without the least throb. To the other ear the lower note merely throbbed. Extreme mistuning, so that the beats became too rapid for distinction (at 60 or 70 per second), yielded no differential tone, though such was heard strongly when the two sounds were mingled together before reaching the ears.

An experiment with tuning-forks gave a similar result.

At this stage resonators, consisting of rectangular boxes closed at one end, were tried, with the result of bringing out the phenomenon much more loudly. The beats of the lower note are not subjective.

(*d*) *Mistuned Double-octave.*—The mistuned double-octave gave equally marked results. When the upper note was altered the effects in the ears were as follows: the lower note throbbed; the upper note merely rose and fell in pitch. When the lower note was altered the upper remained unchanged, but the lower rose and fell and throbbed.

Experiments on Interference between Objective and Subjective Tones.

4. The author has tried to produce interference between an objective and a subjective tone in the following manner:—In the spring of the present year, after a violent catarrh he was troubled at intervals for several days with a "singing" in the left ear of a shrill note somewhere near d'''. He tried several times to produce a note within beating-distance of this; but not having forks for this octave, nor of a lower octave sufficiently near, he did not succeed in the attempt before the ailment vanished.

He has, however, succeeded in another case. After listening for a considerable time to a note that is played very loudly, the ear becomes fatigued and hears the note less loudly; but when the note ceases to be played, it is still heard ringing in the ears for a brief moment. Between this transient sound and a real objective note tuned to beat with it, the author *has* produced beats. He brought the tones to the ears with tubes, one being intolerably loud, the other faint; and arrangements were made by which a single movement opened one pipe and closed the other at the same instant. Beats were heard for about 1½ second after the cessation of the loud sound.

An extremely curious case was mentioned to the author some years ago by an eminent acoustician. A person who was afflicted with mumps on one side of the head heard all the sounds in that ear raised about a semitone*, and heard *beats* accompanying notes in the lower part of the scale. The author inquired some time after of a non-scientific friend who had suffered from a similar attack, whether any thing similar had been noticed, but could only learn that voices and musical notes jangled "like a harmonium out of tune."

Effect of Fatigue upon Binaural Perception of Direction.

5. While trying the above experiments on interference after fatigue, the author noticed an acoustic illusion due to the same origin. Let one ear be fatigued, as before, by listening to a loud pure note. Then let the listener try to estimate the direction of a sound of the same pitch. If his *left* ear has been fatigued, he will invariably imagine the source of sound to be further to the *right* than it really is, and *vice versâ*. The illusory displacement in the direction of the sound is greater the more complete the fatigue. When one ear was fatigued with a c'' fork no illusory displacement was perceived in an a'' fork. The author has, as yet, only tried these experiments in a room, and with the almost simple tones of tuning-forks. The observations are of interest, however, in the theory of Binaural Audition.

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* In a discussion which followed the mention of this case, Professor Michael Foster made the suggestion to the author that this abnormal tuning-up of the receptive mechanism of the ear might be due to the existence of muscular tissues in the structures of the Corti organ. He remarked that in the *ligamentum spirale externum*, outside the attachment of the basilar membrane of the Corti organ, there may be seen, at least in many specimens, cells which very closely resemble *plain muscular fibres* when examined in the microscope. If this be so, they could, apparently, pull up the basilar membrane and tighten it. Professor Foster, who kindly permits me to add this very interesting point, informs me that while this view of these structures is taken by Todd, Bowman, and Böttcher, their muscular nature is denied by Kölliker and Waldeyer.—S. P. T.

XLVI. *Earthquake Observations and Experiments in Japan.* *By* John Milne, *F.G.S., and* Thomas Gray, *B.Sc., F.R.S.E.*[*]

We propose to give in this paper a brief *résumé* of some experiments on earthquakes performed by us during our residence in Japan. Some parts of Japan, and notably the Yedo plane, where these experiments were made, are subject to very frequent seismic disturbances. It must be borne in mind, however, when considering the experiments and results here described, that the earthquakes which occur in this region are seldom of a destructive character.

We have found it convenient to divide this paper into two parts. In the first part a very brief discussion of the more important instruments used by us is given; while in the second part some of the most interesting points brought out by the use of these instruments are referred to.

Part I.

The instruments may be classified, according to the purpose for which they were intended, into the following groups:—

1. *Seismoscopes.*

These are instruments which merely show that an earthquake has taken place. A great number of instruments of this class have been used; and descriptions of most of them are to be found in the Transactions of the Seismological Society of Japan. The following are some of the more important:—

(*a*) *Vessels containing Liquid.*—Vessels of liquid have often been referred to in descriptions of earthquakes as giving indications, by the motion of the liquid up the side of the vessel, of the direction and intensity of a shock. In large shocks, provided they can be made to record their motions by some such method as colouring the liquid, or coating the side of the vessel with some substance which can be easily washed off, these may sometimes be of value. For shocks of ordinary intensity we have found them quite useless. Many methods, both mechanical and chemical, were tried; but as yet no practicable method of recording by means of a liquid has been found.

The chief difficulties are to obtain a liquid which will neither evaporate nor freeze, and at the same time will not creep up the sides of the vessel. Then, again, it is difficult to get a

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[*] Communicated by the Authors.

vessel of the size or shape which will ensure a washing-up at all. A vessel, the fundamental period of oscillation of the liquid in which nearly coincides with the period of the earthquake, would possibly show a considerable motion. A difficulty here arises however. Our observations show that few earthquakes have any definite period; and different earthquakes have generally different, sometimes very different, periods.

(*b*) *Columns.*—These generally consisted of round cylinders, with their ends cut very accurately at right angles to the axis of the cylinder. Specimens of such cylinders, of various diameters and the same height, were set on end on a level plane with the view of finding, from the direction in which they fell, the direction of motion during the earthquake-shock. We were somewhat surprised to find that even the smallest cylinder that can be set on end but seldom falls in ordinary non-destructive shocks. When the shock is tolerably severe, they fall in *various* directions, even when placed on the same slab. The reason of this appears to be that most earthquakes commence gently,—the columns rock, and, whilst rocking, gradually change their plane of motion before being overturned. Occasionally it may be due to the presence of two sets of vibrations in different directions, some columns being overturned by one set, whilst others are overturned by the other set. Because of this indefiniteness in the indications of columns, we have called them Seismoscopes. In addition to round cylinders, columns of various shapes (as, for instance, inverted cones) have been employed. Columns have also been caused to stand on segments of small spheres. These variations have not as yet shown any advantage over the ordinary column.

Besides the ordinary columns which are used as earthquake-indicators, we may mention bodies like strips of glass, pins, &c., which will not stand unless supported on one side. When such bodies are thus supported, they are overturned more readily by motions inclined to the plane of support than the finest columns we are able to set on end. In employing this principle of propping up, it is necessary to use a number of bodies placed against planes in different azimuths.

(*c*) *Microphones*, or circuit-breakers in conjunction with a galvanometer so arranged that if the needle swings it comes into contact with a piece of iron and is held, and many other similar contrivances have been employed to indicate small motions.

(*d*) *Tremor-Indicator.*—This is a modification of a pendulum seismometer, in which the bob of the pendulum is at the time of an earthquake approximately steady. Against this bob are placed the ends of two small strips of wood resting in

V-grooves, the direction of the strips being at right angles to each other. The outside end of each of these strips carries one thread of the bifilar suspension of a mirror, the other thread of the bifilar being attached to a point fixed to the earth. When the strips are pushed inwards against the bob, by the earth moving them, the mirrors turn, and a beam of light coming from a lamp and reflected from a mirror to a scale has its image permanently deflected. An instrument of this class is so sensitive that, at times, it would appear as if a continual earthquake were going on.

(*e*) *Circuit-closer Seismoscope.*—Underneath the centre of the bob of a pendulum a vertical index is so arranged that its upper end is pivoted in a universal joint near to the centre of gravity of the bob of the pendulum; about one inch beneath this joint it passes through a second universal joint fixed to the framework from which the pendulum is suspended. The upper end of this index is attached to one pole of a battery; the lower end, which is a platinum wire, dips into a small depression in the surface of a cup of mercury, after the fashion of Palmieri's circuit-closer. A slight motion of the framework causes a considerable motion of the index, and hence causes the platinum wire to come into contact with the mercury. With this arrangement it is easy to close an electric circuit with a motion of the ground equal to one twentieth of a millimetre. The index forms an automatic key, which closes an electric circuit in which an electromagnet is included. The electromagnet deflects a lever carrying a pencil, which rests on a disk kept constantly revolving by clockwork. The position of the mark upon this disk indicates the hour and minute of the earthquake, or rather the earth-tremor.

The clock is a cheap small American clock, the minute-hand being replaced by a wooden disk. To avoid the pencil continually tracing the same circle, the clock is mounted on wheels and is caused to wind itself along a stretched string which is passed round the hour-axle.

2. *Seismometers and Seismographs.*

(*a*) A seismometer which we have found to act well is a pendulum with an index arranged like that of the circuit-closer just described, except that at the lower end of the index a light sliding needle rests on the surface of a smoked glass plate. On this plate the pointer gives a magnified record of the motions of the earth. This instrument is especially adapted to show direction, maximum amplitude of the earth's motion, and the existence or non-existence of transverse vibrations. We find it advisable when using pendulums as seismometers, to control the motion by introducing a small amount of friction. This friction is applied by allowing a vertically placed sliding rod to rest with its lower end, which is pointed, on a plate of glass placed on the bob of the pendulum. This rod is weighted until, for displacements equal to that of the largest earthquake likely to occur, the pendulum is "dead beat." We have found that when the rod is thus adjusted the error in amplitude due to friction is very small.

(*b*) *Pendulum Seismometers, with apparatus for registering two or three components of the motion.*—These machines are almost identical in principle with the tremor-indicator described above. For a detailed description see Phil. Mag. for September 1881.

(*c*) *Torsion-Pendulum Seismograph.*—In this instrument the bob of the pendulum is in the form of a flat ring, on the top surface of which a flat disk of smoked glass is placed. The suspending wire of the pendulum is so proportioned that the torsional period is about one minute. Above the glass plate two light connecting rods radiate from the suspending wire in directions at right angles to each other. The outside ends of these rods are hinged to the short arms of two light indices having one axis in rigid connexion with the earth, and the points of their long arms resting on the same radius of the smoked plate. When this instrument is arranged for use, the bob is twisted through rather more than 180° and held by a catch, which at the time of an earthquake can be released by an electromagnet in connexion with a circuit-closer. The bob and glass plate then twist slowly round, and the indices at the same time write two rectangular components of each motion of the earth on the smoked glass. The bob may be arranged so that it is again caught after one turn if thought desirable; and for very small earthquakes it is well to do so, in order to prevent the subsequent motion of the pendulum from confusing the record. The peculiarity of this instrument is that the bob of the pendulum is used both as a receiving-plate for the record, and as a steady mass for the purpose of working the levers which write it. This gives simplicity and compactness to the instrument; but it is evident that it can only be used in conjunction with an automatic starting-apparatus.

(*d*) *Conical-Pendulum Seismograph.*—For a detailed description, with drawings, of this instrument see Phil. Mag. for September 1881.

(*e*) *Bracket Seismograph.*—This consists essentially of a heavy weight carried at one end of a horizontal bracket which is free to turn on a vertical axis at its other end. When the frame carrying this axis is moved in any direction, except that parallel to the direction of the length of the bracket, the weight causes the bracket to turn, while it itself remains nearly steady. This frame is firmly attached to the head of a stake driven into the ground. A light prolongation of the bracket forms an index, which writes on the surface of a smoked glass plate a magnified representation of the motion given to the frame by the ground. Two of these instruments, placed at right angles to each other, are used in conjunction for the purpose of writing two rectangular components of the motion.

The principle involved in this instrument (namely, that of a mass supported in neutral equilibrium at the end of a bracket) was, we believe, first used by Prof. W. S. Chaplin, of the Tokio University—and subsequently in the instrument described above, which was invented by one of the authors of this paper, who was not then aware that the principle had been previously applied to this purpose. An instrument involving the same principle has also been invented by Prof. J. A. Ewing, and is described in the Proceedings of the Royal Society for February 1881.

(*f*) *Double-Bracket Seismograph.*—This instrument consists of two brackets hinged to each other, and one of them to a fixed post. The planes of the two brackets are then placed at right angles to each other, so as to give to a mass suspended at the end of the outermost bracket two degrees of horizontal freedom. (See Phil. Mag. for September 1881.)

(*g*) *Rolling-Sphere Seismograph*;

(*h*) *Rolling-Cylinder Seismograph.*

For descriptions and drawings of both these instruments see Phil. Mag. for September 1881.

Ball-and-Plate Seismograph.—In this instrument three balls rest at the corners of a triangle on a "surface plate." On the top of these balls is placed another "surface plate." When the lower surface-plate, which is fixed to the earth, is moved, the balls roll and the upper plate is left behind. Pivoted on the upper plate is the end of a long light index. A short distance below this pivot the index passes through a universal joint fixed to the lower plate. The lower end of this index carries a sliding needle, the point of which rests on a smoked glass plate, and there writes a magnified representation of the earth's motion.

3. *Instruments for recording Vertical Motion.*

(*a*) *Ordinary Spiral Spring.*—The earliest form of instrument used to record vertical motion was a spiral spring with a weight at the lower end, the weight being supposed, on account of its inertia, to remain steady at the time of a shock. No satisfactory result has ever been obtained from such an instrument; for it is extremely inconvenient to make the spring sufficiently long to allow of enough elongation to give a long period of vibration.

(*b*) *Flexible-bottomed-Cylinder Seismograph.*—This consists of a tin can with an indiarubber bottom, which rests on a thin flat spring. Against the lower side of this spring, opposite the centre of the rubber bottom, the end of the short arm of a lever presses. The can is partly filled with water, which by its inertia causes the bottom to be deflected if the can be suddenly raised or lowered. The motions of the bottom are communicated to the lever, a point at the end of the long arm of which is caused to register the motion on a suitable receiving-plate.

(*c*) *Compensated-Spring-Lever Seismograph*;

(*d*) *Hydrometer Seismograph.*

For descriptions, with drawings, of these instruments, see Phil. Mag. for September 1881.

4. *Apparatus on which to record Earthquake Motions.*

(*a*) *Carriage Receiver.*—A simple and convenient instrument is a strip of smoked glass resting on a three-wheeled carriage. This carriage is pulled along by means of a small weight, while a thread from the opposite end of the carriage is wound round the axle of a large fan and causes it to turn in a vessel of oil, thus regulating the motion. This fan is held by a pin, which at the time of an earthquake is drawn back by means of an electromagnet in connexion with a circuit-closer. When this occurs, the weight as it falls pulls the carriage slowly along, while the pointers of a seismograph write the undulations of the earth on its surface.

A check on the regularity of the plate's movement is obtained by causing a small pendulum, to which is attached a flexible spring, to mark time upon it. This pendulum is set free during the first turn of the fan, which knocks out a prop used for holding the pendulum in a deflected position.

The plate is subsequently coated with photographer's varnish, and then photographed by the "blue process" so well known to engineers.

(*b*) *Drum-Clock Receiver.*—Another very convenient method is to take an ordinary clock, and place a drum on an axis formed by a prolongation of the key-pin of the striking apparatus. Fasten back the detents governing the striking, and attach the starting-apparatus to the fan. If the clock is governed by a balance-wheel, it is easy to arrange the starting-lever so that, by starting the drum, it will stop the clock. If the clock is governed by a pendulum, the best method is to catch the pendulum.

The drum on the axis of the striking-spring is covered with smoked smooth paper, on which the points of the indices of a seismograph rest.

(*c*) *Continuous-motion Receivers.*—These are simply strong clocks, arranged for the purpose of driving a drum about 9 inches, or a glass plate about 18 inches in diameter, at the rate of about one revolution in two minutes. So long as no earthquake occurs, the pointers of the seismograph continue to move along the same line. When an earthquake occurs, the apparatus is in motion and ready to receive the whole record. It is yet a question whether this is necessary, as it is quite possible to *start* an apparatus before an ordinary seismograph (giving, say, a multiplication of 10) is able to show any movement on the somewhat broad line which has been previously formed. Certainly in earthquakes, when the motion is considerable, there is no use for such an apparatus.

With apparatus for this purpose it is difficult to produce uniform motion, as it is evident that an escapement-method of governing is not suitable. Conical pendulums, troughs of liquid with dipping vanes, after the pattern of Airy's governor, have been tried, but, though acting well when there is no disturbance, are likely to be affected by the shock, and hence may not act well when they are wanted. It appeared that a better arrangement would be, to reverse the action of the Airy governor and cause the trough of liquid to rotate, the vane remaining stationary. The liquid, from the mere fact of its rotation, would not be so likely to be affected by the shaking; and, besides, the trough, in consequence of its comparatively great mass, would have a fly-wheel governing-action. If the vane be placed near the outside of the trough, the governor can be made to have great power, because the raising-up of the liquid by centrifugal force immerses a greater area of the vane. It was found on trial that an arrangement of this kind makes an excellent governor, which we think might be taken advantage of for many other purposes.

5. *Time-Takers.*

For taking the time of a shock, the simplest plan is for an observer, when the shock is felt, to quickly draw out his watch and observe the second, minute, and hour successively, and then in as short a time as possible to compare his watch with a standard clock.

One of the chief objects in taking time accurately is to compare the times of arrival of the shock at two or more places some distance apart; as this is the most reliable method of finding the direction from which a shock has come, and in some cases its origin. The only satisfactory method of comparing the times taken at two different places is by an electric signal. The interchange of watches, even if several are interchanged simultaneously, has not proved satisfactory, owing to the variable rate of the watch during its journey. The interchanges made by us were by railway between Tokio and Yokohama, a distance of about 18 miles.

Comparison of the times at the two places can be made by referring each to a well-regulated chronometer. But for ordinary observers the taking of the chronometer-rate is extremely troublesome.

Clock-stopping.—The ordinary method of taking the time is, as in Palmieri's apparatus, to stop a clock. We have already indicated how an ordinary clock with a balance-wheel may be stopped. A pendulum-clock may be stopped, as Mallet has shown, by dropping a catch on a pin fixed in the pendulum-bob. The best way of relieving this catch, which is a toothed-bar axled at one end, is to hold up the other end by resting it on the extremity of a horizontal wire fixed to the bob of a conical pendulum—for example, the extremity of one of the indices of a conical-pendulum seismograph. The wire ought to be in the same straight line as the bar which holds the bob of the conical pendulum deflected. The whole of this apparatus, which can be made for a few pence, can easily be made small enough to go inside an ordinary clock-case.

Time-taker.—Rather than the method of stopping a clock at the time of an earthquake in order to obtain the time, we are now using the following method:—A clock with a central seconds' hand is taken. The hour-hand and minute-hand are each produced and bent outwards at right angles to the face. The hour-hand, however, is made slightly longer than the minute-hand, and the minute-hand slightly longer than that indicating seconds. In consequence of the bending upwards, the ends of the three hands move in the same plane, instead of being slightly below each other. These ends are furnished with very small heads of wash-leather, which from time to time are soaked with an oily ink. A light flat ring, with divisions on it corresponding to those of the clock-dial, is so arranged that, at the time of a shock, it can be quickly advanced until it touches these heads, and then drawn back. By so doing, the hour, minute, and second of the shock are indicated on the varnished face of the ring. This backward-and-forward motion of the ring is effected by means of a crank and connecting-rod, the crank being caused to make half a turn by means of a pulley which is turned by a falling weight. At the time of a shock an electromagnet in connexion with a circuit-closer pulls a catch away from the pulley, and the crank-pin, which is at its lowest position, moves to its highest position, while the weight moves to a small platform. This half-turn of the pulley causes the ring to be pushed out towards the clock and then to be withdrawn.

The instruments which we have now described, and many of them variously modified, have all been more or less tested by subjecting them to a series of actual earthquakes. From our experiments, it would seem that the choice of an instrument must depend somewhat on the kind of record which is sought for.

For a direct single record, either the pendulum with vertical index, or the double-bracket instrument appears to be the best. It is probable that two conical pendulums arranged to take up two rectangular components, and, again, by means of connecting-rods, to communicate these components to the same vertical lever, may act well; but this has not been tried. For component records, a pendulum with two indices, brackets, conical pendulums, and cylinders seem all to act well; but the conical-pendulum instrument is probably the best.

For large earthquakes it is probable (although we have had no practical experience) that the bracket and pendulum instruments would prove useless. For a component of the record, either a simple hollow cylinder of considerable mass and resting with its axis horizontal on a level plane, or a three-wheeled carriage similarly supported, will be found good.

Part II.

Earthquake Motion.

The subject of earthquake motion may be treated from three points of view:—1st, theoretically; 2nd, by the examination of artificially produced vibrations; and 3rd, by the instrumental observation of the vibrations we feel in actual earthquakes.

(*a*) *Theoretically.*—The probable nature of earthquake movement may be gathered from the writings of Hopkins, Mallet, &c., and from treatises on elasticity by several mathematical writers. From these we conclude that there ought to be at least two sets of vibrations, known respectively as normal and transverse vibrations. The normal vibrations travel more quickly than the transverse vibrations, to which they are at right angles. According to Poisson, these velocities would be $\sqrt{3} : 1$. Poisson's theory, however, has been shown to be at fault in this respect; and we have to rely upon direct experiments on the elasticity-moduli of the materials for correct theoretical estimates of this ratio. The following, according to such experiments made by us (an account of which we hope shortly to publish), is the value of this ratio for the kinds of rock mentioned in the table, the actual velocities being also given:—

	Velocity of normal vibrations, in centimetres per second.	Velocity of transverse vibrations, in centimetres per second.	Ratio of normal to transverse.
Granite	395×10^3	219×10^3	1·80
Marble	381×10^3	208×10^3	1·83
Tuff	285×10^3	209×10^3	1·36
Clay-rock	348×10^3	254×10^3	1·37
Slate	451×10^3	286×10^3	1·58

Deductions of this kind, however, are invalidated by the fact that the specimens used for experiment are, of course, nearly homogeneous, whilst the earthquake passes through a mass which is heterogeneous and more or less fissured. The results here given are deduced from moduli which were determined by cross-bending and twisting of long round cylinders. Mallet, by experiments "on the compressibility of solid cubes of these rocks, obtained the mean modulus of elasticity," with the result that "nearly seven eighths of the full velocity of wave-transit due to the material if solid and continuous, is lost by reason of the heterogeneity and discontinuity of the rocky masses as they are found piled together in nature."

(*b*) *Artificial Earthquakes.*—So far as we are aware, the presence of the normal and transverse vibrations has never been satisfactorily proved; although Mallet proved that their presence could be observed in artificially produced vibrations.

In performing a similar set of experiments to those carried out by Mallet, we succeeded in obtaining records of both sets. These records proved:—1st, that the two sets of vibrations exist; 2nd, that they travel at different rates, which rates were measured; 3rd, that they are differently affected by distance, one set dying out before the other; 4th, that there is a difference in their period, the transverse wave having rather the longer; 5th, the amplitude of normal vibrations is approximately in the inverse ratio of the distance from the source.

These vibrations were produced by dropping a heavy iron ball, weighing about a ton, from heights varying up to 35 feet. The ground through which the vibrations were propagated was a hardened mud. The records here referred to were written by a pair of bracket-seismographs, with their indices resting on a smoked glass plate drawn along by clockwork.

The measurements on the rate of dying out were mostly made by means of rolling-sphere seismographs.

The velocities obtained for direct and transverse vibrations were 438 feet and 357 feet per second. This gives 1·23 as the ratio of the propagation of these two sets of vibrations.

The very low rate of propagation is due to the soft medium through which the vibrations were propagated. The actual results are probably very nearly correct, from the manner in which they were performed, there being no personal equation to take account of. Similar experiments were tried at a point where the medium was rock. As might have been expected, in this case we obtained hardly an appreciable motion even at a short distance from the ball.

Actual velocities, measured in America at the time of the Hell-Gate explosions, varied between 3500 and 8000 feet per second. The rock was in this case gneiss (?)

As the base-lines which were used in America were long, they must have afforded an excellent opportunity for obtaining a good separation of the normal and transverse vibrations. If observations were made on these, we have not met with any quotation of them.

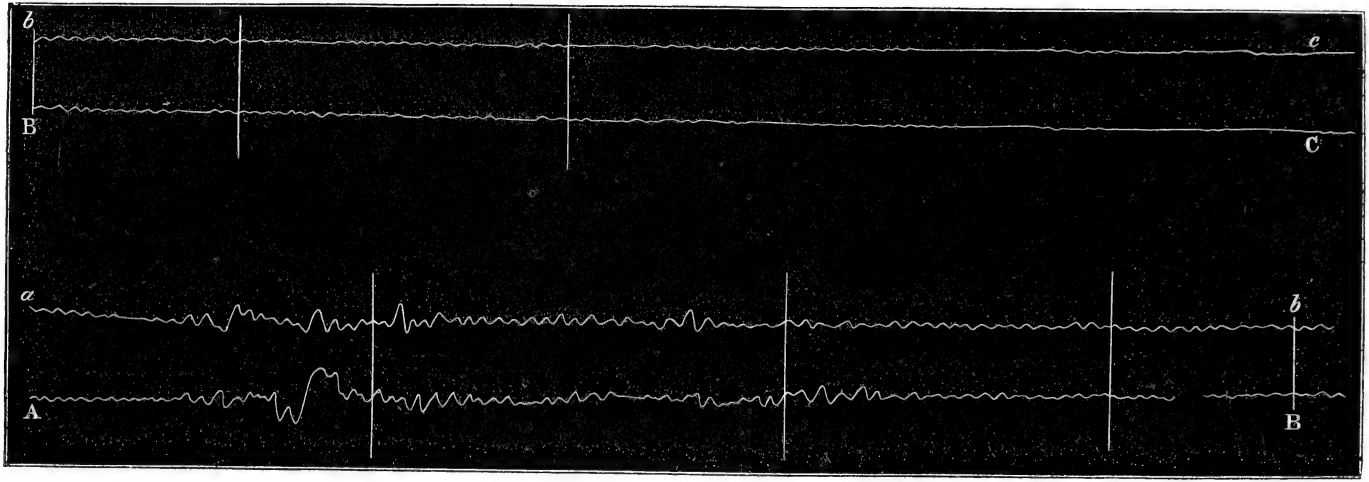
Mallet obtained velocities, in feet per second, as follows:—

Sand	824·915
Granite (shattered) .	1306·425
„ (solid) . . .	1664·574

We presume that these velocities are all for direct waves.

(*c*) From records drawn by seismographs on smoked glass plates, it appears that the motion during an earthquake is generally irregular in its character: the extent of movement increases gradually to a maximum, and then varies backwards and forwards through several minima and maxima during one earthquake. This gradual increase from small to large amplitudes at the beginning of earthquakes has considerable value in connexion with deductions from falling columns &c., with regard to the side from which the earthquake came. The amplitude of movement in earthquakes felt in Yedo is seldom above a few millimetres, even when they are sufficient to do considerable damage to buildings. Usually the maximum amplitude is even under one millimetre. The period usually varies between $\frac{1}{5}$ and $\frac{1}{2}$ of a second, having in the majority of recent earthquakes been nearer to the latter value. In several cases the direction of motion has been found to change during the same earthquake, thus giving evidence either of reflection or of transverse motion. Most of the points here referred to will be found illustrated in the adjoining diagram.

This diagram is a tracing of the record given by a conical-pendulum seismograph of a small earthquake which occurred at Tokio on the 25th of July, 1881.



It is to be observed that, as the record was written on a strip of smoked paper wound round a drum which was started automatically by the earthquake, the very beginning of the earthquake is not shown. The waving line A B C shows the successive motions of the earth in a direction N. 15° E. magnified about ten times; while the line *a b c* shows the motions at right angles to that direction magnified to the same extent. The spaces inclosed between the cross lines were moved over in five seconds. The extremely gradual manner in which the motion dies out is well illustrated in this record.

In Yedo, earthquakes showing east-and-west directions of vibration have been proved by accurate time-observations to have originated in the south. This would seem to indicate that the normal vibration did not appear prominently in the record. It is possible that in some cases the normal vibrations are never very prominent, the original strain on the material being more of a shear than a compression or extension. If this supposition holds, it follows that we cannot with any certainty make a deduction with regard to the direction of propagation from the direction of motion. The direction of original shear might be inclined at *any* angle to the direction of propagation.

Evidence that the motion is in some cases of a nature similar to that here suggested has been obtained in the following manner. Instruments were set up at several stations, about 20 miles apart, round Yedo Bay; and in conjunction with the observations of these instruments the time of arrival of the shock was observed at Yedo and Yokohama. From the latter observations, it was found that the hyperboloid which forms the locus of possible positions of the origin lies between Yokohama and Yedo, and also that it is concave towards Yokohama. These observations are corroborated by the much greater intensity, greater verticality, and greater number of the shocks as felt at Yokohama than at Yedo. In some cases, however, the directions of motion at Yedo and at stations situated at points in lines at right angles to the line between Yedo and the probable origin, are found to be nearly the same—thus indicating a disturbance which is propagated in one direction as a normal vibration, while at right angles to that it is almost a pure transverse or distortional vibration.

The existence of such an irregular kind of disturbance is rendered still more probable when we take into consideration the highly irregular nature of the vibrations which are felt both by our senses and by our instruments, and combine these with the observation that the rocky masses in the vicinity of our probable origin are highly faulted, and bear strong evidence of recent elevation. The jolting movements felt in an earthquake resemble very much what one would expect to be produced by the tearing open of a fracture or the sliding of two irregular surfaces over each other.

It has been usual to deduce the angle of emergence from observations of the horizontal and vertical components of the earth's motion. If, however, we accept the possibility of distortional vibrations playing an important part in earthquake phenomena, the accuracy of this mode of estimating that angle is very questionable. It is probable that in many cases the vertical motion may be due simply to a vertical component of transverse movement. When we take this into consideration, it is at once evident that some means must be adopted for the purpose of finding out to which set of vibrations the vertical motion is due. This can be done by writing on a continuously moving plate a record of the whole movement, and observing the vertical movement corresponding to each individual horizontal movement. We must not conclude, however, that when the vertical movement coincides with the first indicated movement it is a genuine vertical component of a normal vibration, unless distinct evidence of the presence of transverse vibrations is afterwards observed. In order to form a reliable estimate of the nature of the vibration, and therefore of the angle of emergence, the direction of propagation as well as the direction of vibration must be independently observed at several stations. The direction of propagation can only be reliably obtained from observations of time of arrival.

An element of uncertainty in time-observation when taken by clock-stopping apparatus is the prolonged duration of the shock, and hence the difficulty of making all instruments act at the same part of the shock. In consequence of this, it becomes necessary either to make the stations at considerable distances apart, or to place them so close together that they can be conveniently put in electric communication, and then to mark particular instants on the different chronographic plates used for receiving the record. In this way, if instruments can be obtained which will not of themselves considerably modify the nature of the record, either the beginning of the shock, or, if that is too gradual, some particular prominent feature of the shock, may be selected on the different records, and from it and the time-signals the interval between the two stations obtained. For direction of propagation three stations are of course necessary; and for the determination of origin two sets of three, if the elements of each set be close together, are necessary. The following are one or two other methods which might be adopted for this purpose:—

If the time of arrival of a shock at three stations a long distance apart be taken, and an average velocity of propagation assumed, it is possible to construct for each pair of these stations a surface (which will be approximately an hyperboloid) on which the origin will be situated. The mutual intersection of these three surfaces will be approximately the origin required. A correction on this may be obtained from succeeding shocks by instituting a fourth station on a line through the approximate origin and one of the previous *stations*, and by this means approximating more closely to the velocity of propagation.

As another method, if we have the times of arrival at three stations close together accurately measured (these give us the direction of propagation and the horizontal velocity), a fourth station at some distance, say one or two miles, but placed obliquely to the determined direction, and worked in conjunction with the first three, will give us an hyperbola; and the point on which the direction obtained from the three stations intersects this gives us the *epicentrum*, or point vertically above the origin of the disturbance or *centrum*. It is possible that in some cases two points of intersection may exist; but the relative intensity of the shock at the two points will be sufficient to allow the proper one to be chosen. A fifth station would render the determination of the epicentrum perfectly definite.

The most general case for the determination of the centrum from time-observations requires six stations; and these six stations must not be in the same line through the origin: and even here it is assumed that the velocity of propagation has been the same in all directions.

By varying the elements which are known—as, for instance, the number of stations at which time was observed, the relative positions of these stations, the directions of vibration observed at various points, the angle of emergence (if for small shocks such an angle is determinable), &c.—we see that a large number of problems may be presented the solution of which will give to us more or less correctly the centrum or epicentrum of the shock.

The duration of an earthquake-shock is seldom less than one minute, in Yedo, and is often two or three minutes. It must be remarked, however, that, judging from our senses, many of the shocks seem to last only a few seconds.

The intensity of a shock is evidently best estimated from the maximum velocity of translation produced in a body during an earthquake. This is evidently the element according to which the destructive power is to be measured, it being proportional to the maximum kinetic energy of the bodies on the earth's surface relative to that surface during the shock. According to this, then, a shock, the vibrations in which are of one-second period, will have only one fourth of the intensity of a shock with vibrations of half a second period, if the amplitudes be the same. We may write intensity proportional to amplitude divided by the square of the period, or

$$I \propto \frac{A}{T^2}.$$

Marked instances of the effect of period are often to be observed by simply referring to our senses for the intensity, and then to our records for the amplitude and period. We find sometimes that what we considered a severe shock has had a very small amplitude of movement, but that it has had a short period.

Another interesting point to be observed, is the variation of the affected area with intensity, amplitude, and duration. Of these elements it would appear from our observations that duration is the most important. A comparatively small earthquake, if it last for several minutes, will be felt over a large area; whereas a severe earthquake which consists of only a few bumps will only be felt for a few miles. A very good illustration of this was obtained on December 23, 1880, when an earthquake which was felt at Yedo as a somewhat severe shock, and which lasted between three and four minutes, was felt in several places in Yezo; whereas a previous earthquake, which was much more destructive, but which lasted less than half the time, was not felt through half the distance. Both these shocks seemed to originate near the same place.

The geological nature of the rocks near the origin from which some of the earthquakes we have felt in Yedo appeared to emanate, is as follows:—

Round the northern end of Yedo Bay we meet on all sides with greyish-coloured partially clay-like rock, which on close examination appears to consist of a decomposed volcanic ash. These tuffs are horizontally stratified, and very little faulted or altered. As we go southwards on both sides of the bay, these same rocks gradually become coarser in texture, until finally they are little more than agglomerates consisting of undecomposed volcanic ashes. Along with this lithological change we observe a physical change to be taking place. The rocks become gradually bent into gentle anticlinal and synclinal folds; and faults become more numerous. Near to the entrance of the bay, in the extreme south, the foldings are more steep, and the faults exceedingly common. At many points in the faulted districts distinct evidence of a very recent elevation are to be seen in the form of borings of *Lithodomi* 6 to 10 feet above high-water mark. In some of these holes the shells still exist. As the rock is exceedingly soft, and at the same time exposed to the weather, it is reasonable to suppose that the elevation must be of a very recent date.

It is in this district of faults and marks of recent elevation that we have determined an origin for several of the more recent earthquakes.

Classification of Earthquakes.

Instead of classifying earthquakes according to the impression they have on our senses, we would suggest that they might be better classified by the records we obtain from our seismographs. So far as records have gone, it would appear that we might classify shocks into (1) Direct, (2) Transverse, (3) Compound shocks.

By a *direct shock* we mean a shock consisting of vibrations in the direction of propagation, the degree of horizontality being expressed by the ratio of the horizontal to the vertical motion.

By a *transverse shock* we mean a shock which is for the most part due to vibrations at right angles to the direction of propagation. The character of this might be expressed in the same manner as the character of the direct shock.

By a *compound shock* we mean a shock showing combinations of the direct and transverse vibrations. The horizontality might be expressed as before.

In defining an earthquake we should consider it as the result of an irregular disturbance in the earth's crust, generally producing both waves of compression and distortion. In certain cases the former of these may be prominent, whilst in other cases the latter.

Frequency of Earthquakes at different Seasons.

The following Table, extracted from the records of the Meteorological Observatory, is a good summary of the number and intensity of earthquakes felt in Tokio, as recorded by one of Palmieri's instruments during the last four years.

Months.	No. of shocks.							Maximum Shock.				No. of Observations under each point.			
	1876.	1877.	1878.	1879.	1880.	Totals.	Force above 5°.	Intensity.	Direction.	Day.	Year.	S.S.E. or N.N.W.	S.S.W. or N.N.E.	W.S.W. or E.N.E.	W.N.W. or E.S.E.
Jan. ...	3	4	3	6	8	24	1	21° 0′	W.S.W. or E.N.E.	20	1876	4	2	2	3
Feb. ...	4	5	**8**	7	**9**	33	4	**78 0**	S.S.E. or N.N.W.	**22**	**1880**	5	5	6	**10**
March .	6	6	5	**13**	6	**36**	3	11 10	S.S.W. or N.N.E.	4	1879	4	5	**8**	5
April ...	**10**	5	2	**0**	5	22	3	9 30	S.S.W. or N.N.E.	14	1880	2	2	3	5
May ...	5	8	5	9	2	29	1	7 30	W.N.W. or E.S.E.	7	1876	2	3	2	4
June ...	3	7	4	4	9	27	2	6 0	W.N.W. or E.S.E.	11	1878	**7**	4	2	7
July ...	3	6	3	3	7	22	4	12 0	W.S.W. or E.N.E.	30	1876	4	4	5	2
August .	3	4	**1**	4	3	15	1	6 30	S.S.E. or N.N.W. W.N.W. or E.S.E.	27	1877	3	**1**	2	4
Sept. ...	3	**1**	2	1	**1**	**8**	**0**	3 40	S.S.W. or N.N.E.	21	1879	**1**	**1**	**0**	**1**
Oct.......	3	7	4	7	3	24	3	10 50	S.S.W. or N.N.E.	18	1879	4	3	1	8
Nov. ...	4	6	5	6	8	29	**5**	12 10	S.S.W. or N.N.E.	10	1880	5	7	2	9
Dec. ...	6	**9**	4	9	8	**36**	4	34 10	S.S.W. or N.N.E.	23	1880	3	**9**	7	7
Total ...	53	68	**46**	**69**	**69**	305	31	......	......	......	......	44	46	**40**	**65**

From this table we see that during the six winter months (beginning with October) there has been a total of 182 shocks, while during the remaining six months only 123 shocks have been recorded. This result, it will be observed, accords with the general result obtained by Mallet from an examination of the earthquake records of many countries. These shocks, however, were destructive; whilst those we speak of, with the exception perhaps of one or two, were so small that they would not have found a place in the records examined by Mallet.

Not only do we experience a greater number of shakings in the cold months, but they are of greater intensities as compared with those of the warm months. The intensities of the former period may be denoted by 20, and those of the latter by 11.

Prof. W. S. Chaplin has published a critical examination of these earthquakes with regard to the connexion between the time of their occurrence and the position of the moon. The results obtained did not show any of the connexion observed by M. Perrey.

If we turn to the destructive earthquakes of Japan, we see from the following Table (which is drawn up in the same manner as those compiled by Mallet) that there have been more shocks during the summer than during the winter. This result, it will be observed, is not in accordance with the results obtained by an examination of the recent earthquakes, nor with the general result deduced by Mallet for the earthquakes of the world.

The table has been compiled from Japanese earthquake literature. Of this literature we are acquainted with 65 books treating on earthquakes, 7 of which are earthquake calendars and 23 are earthquake monographs. No doubt there are many other books besides those we here refer to.

These earthquakes, examined as a whole, do not show any decided decrease in seismic activity. If, however, we select a particular seismic district—as, for instance, that of Kioto, for which we have a series of records extending over about 1200 years—a decrease in the activity of that region is observable. If a similar examination could be made for *special* seismic areas in Europe, instead of an examination of the earthquakes of a country, we are inclined to think that similar results might possibly be arrived at.

The fact that there is not a general decrease to be observed in the number of earthquakes in many countries when taken altogether may perhaps be, as it is in Japan, due to the fact that the earthquakes in certain areas have only been recorded during a short period, and hence there is an apparent increase in late years. In searching for a decrease, such as we have found to exist (providing our calendars are correct) in the

Kioto area, in other countries, we should be inclined only to examine those districts where earthquakes are now common.

Century.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Autumn and Winter.	Spring and Summer.	Date of year only.	Total.
B.C.																
300–200 ...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1
200–100.																
100–0.																
A.D.																
0–100	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1
100–200.																
200–300 ...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	1	1
300–400 ...	...	...	1	...	...	...	...	...	...	...	...	...	...	...	...	1
400–500 ...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	2
500–600 ...	...	...	...	...	1	...	...	...	1	...	...	...	...	...	...	2
600–700 ...	2	...	...	...	...	1	1	...	2	...	6	5	...	...	...	17
700–800 ...	1	1	...	...	3	4	4	...	1	...	...	...	...	...	...	14
800–900 ...	5	3	5	7	5	4	7	7	4	6	7	4	...	...	1	65
900–1000	...	1	1	1	3	1	6	2	...	3	1	1	...	...	1	21
1000–1100	2	1	1	4	2	1	2	1	1	1	2	7	...	...	...	25
1100–1200	1	1	1	...	3	3	2	1	1	1	3	3	...	...	...	20
1200–1300	1	...	1	...	6	4	3	2	2	3	...	...	...	...	...	22
1300–1400	1	1	2	3	2	4	4	5	1	1	2	1	...	...	...	27
1400–1500	3	8	4	3	4	3	...	2	5	4	5	2	...	...	2	45
1500–1600	...	1	4	1	1	1	1	2	4	1	...	...	...	...	1	17
1600–1700	2	4	2	1	1	6	1	1	...	...	2	3	1	...	2	26
1700–1800	...	1	4	4	2	2	2	3	3	3	4	3	...	...	...	31
1800–1900	...	5	3	3	1	...	5	4	...	...	1	5	...	...	1	28
Total ...	18	27	29	27	34	34	38	30	25	23	33	34	1	...	13	366
	Winter.			Spring.			Summer.			Autumn.						
	74			95			93			90						

Winter months, 165; Summer months, 188.

Thus, for instance, we should not search for a decrease in England, even if our records for the last ten centuries were perfect, because it is probable that the country has settled down to a seismic state which approximates to constancy; in other words, the curve representing intensity of seismic action in relation to time has become nearly asymptotic to the axis of time.

Occurrence of Earthquakes in Groups.

It is often to be observed that subsequently, and now and then previously, to the occurrence of an earthquake of unusual severity, we have a more or less rapidly occurring series of smaller attendant shakings. This fact appears to be a confirmation of what we have already said respecting the origin of shocks by faulting, the small preceding shocks indicating the first giving way of the ground, the large shock the final yielding, and the subsequent shocks the gradual settling down of the ground into a state of equilibrium.

Effect of Earthquakes on Buildings.

So far as we have been able to observe, the effect of an earthquake on a building depends on the following circumstances:—

Whether the different parts of the building are so proportioned that they are able to vibrate in unison without producing excessive stresses at any part. We have observed, for instance, that brick chimneys passing through the roofs of wooden houses, and chimney-stacks built outside a house and supported by tie-rods from the house, are very subject to damage by earthquakes. We have observed, also, that cracks produced by an earthquake generally open and close during the vibrations of succeeding earthquakes, thus giving evidence of a want of agreement in the vibrational period of the portions of the building on opposite sides of the crack.

The absence of arches having sharp angles at the crown or at the buttresses is also of considerable importance. An examination of a great number of brick arches in similar buildings, some of which had sharp angles, as here described, while others were curved at these points, showed a very marked difference in the effects which had been produced in the two cases. The rounded arches were seldom at all damaged, while the others were nearly all more or less cracked.

The nature of the foundation on which a house is built may also influence the effect considerably. Probably the safest foundation is a soft one, the building standing on a broad concrete base. We have been led to this conclusion from observations on the relative effects of the same earthquake on buildings, some of which were built on rock, others on piles driven to considerable depths in soft materials, and others on concrete. The buildings on rock are generally more shattered than those in the same locality but on a softer material, such as hardened mud. Again, those on piles appear to suffer more than those on concrete. Whether this may be due to the piles causing the shock to be more severely given to the building, or due to the piles sinking and hence causing stresses in a more indirect way, we are not yet able to say.

The configuration of the ground in the vicinity of the building is a very important factor. The greatest damage dependent on this element is done to buildings which are placed near the edge of bluffs, or near the junction of a plane with a steep hill. The effect of a bluff is probably due to the free surface causing a greater amplitude of motion, or possibly a more one-sided motion than takes place in an open plane.

Evidence that the rocks near to the edge of one of the bluffs above Yokohama were subject to great stress during a recent earthquake is obtained in the form of long cracks parallel to the edge of the bluff. It is more difficult to account for the damage done to buildings close to the foot of steep cliffs; but possibly an explanation may be afforded by reflexion of vibrations causing increased activity in that region.

Rotation of Bodies.

After a severe earthquake it has often been observed that many heavy bodies (like obelisks, gravestones, chimneys, &c.) have more or less rotated. This phenomenon has been explained in many ways. Some have supposed that they indicate a vorticose motion in the ground; others that the phenomena are due to reflected and direct shocks acting simultaneously on the body which has been rotated. Mallet offers the explanation that the rotation is due to the centre of friction of the base of the body not coinciding with its geometrical centre.

In certain cases no doubt these explanations, especially the latter, may be correct.

If vorticose motion is the cause of the rotation, we ought to find that all twisted bodies near the same place have been turned in the same direction; and if we take Mallet's view, the rotation ought to follow no definite law.

What we actually find, after having examined a great number of cases, is that they do follow a law, but not that which would be obtained on the supposition of vorticose motion.

The law is that all similar bodies, such as gravestones, having similar sides parallel, are in the same district rotated in the same direction; while another set, having their faces placed at an angle to these, may be rotated in an opposite direction.

This is accounted for if we suppose rotation to be due to a direct shock. If the shock comes broadside on to the body, it only tends to throw it over; but if it comes obliquely to this direction (but not exactly along a diagonal), it tends to tilt it up on a corner. This shock may be resolved into two components—one along the projection of the line joining the centre of inertia and the corner, tending to tilt it up on that corner, and the other at right angles to that direction tending to whirl it round. This explanation is not only verified by observations after an earthquake, but is easily verified by experiment. It will be seen that, according to this explanation, the direction of rotation may be used for the determination of the direction of vibration of a shock. One direction produces a rotation corresponding to the hands of a watch, whilst another gives an opposite rotation. It will also be observed that a body tends to turn until it becomes broadside on to the direction of shaking.

XLVII. *Notices respecting New Books.*

On the Alteration of Superficial Deposits by the Infiltration of Atmospheric Waters, &c. [*Mémoire sur les Phénomènes d'Altération des Dépôts superficiels par l'Infiltration des Eaux météoriques, étudiés, &c.*] By Ernest Van den Broeck, Conservator in the Royal Museum of Natural History at Brussels, &c. 4to. 180 pages, with coloured lithographic plate and numerous woodcuts. Brussels: F. Hayez, 1881.

THE author has herein brought together on a systematic plan the observations made by himself and others on the results of the chemical action of rain-water on superficial deposits and on rocks lying within its influence. This subject has, of course, been within the cognizance and under the study of geologists from an early period; and many notes and papers have been given on the several facts observed; but M. Van den Broeck has taken this subject particularly under consideration, and furnishes this compendious and useful memoir, based on careful notes on the phenomena of superficial alteration observed in Belgium and elsewhere, and illustrated with many clear sections, some of which are coloured.

In treating of the various aspects of superficially-altered rocks, and of the results of meteoric (atmospheric) agency, the author first notices the mechanical disintegration, and then the chemical dissolutions, decompositions, and new combinations; also the universality of these phenomena. Then he takes:—(1) the felspathic rocks and their veinstones; (2) the metalliferous deposits, especially where sulphur and iron play an important part; (3) the argillaceous schists and the clays, the latter being pure, sandy, or glauconitic; (4) the siliceous rocks, with interesting remarks on changes in both shallow and deep deposits, alterations and accumulations of quartz, modifications of jasper, colloid silica, decomposition of glauconitic sands, "silex nectique," and the Tertiary Millstone of Paris; (5) calcareous rocks, especially limestone-gravel, Cretaceous strata, the "clay with flints," sand-pipes, &c. The Appendix treats particularly of infiltrations in the Quaternary beds, such as the Hesbayan loam or loess, and the Diluvium of the valley of the Seine.

The chemical phenomena of dissolution and oxidation, which are the essential points in M. Van den Broeck's researches, as here detailed, have certainly been long known and recognized; but we have to thank him for collating and coordinating both the facts (some of which are quite new) and his inferences, as shown in this memoir and thus made available for further consideration, in view of the importance which these hydrochemical operations have in falsifying certain aspects of geological sections in all parts of the world.

The Ground (Substrata) of the Cities of Europe. Geological Studies. By Felix Karrer. [*Der Boden der Haupstädte Europa's &c.*] With 23 woodcut sections. 8vo. 68 pp. Alfred Hölder: Vienna, 1881.

This is a compendious and highly instructive memoir on the geological structure of the areas on which the seven great cities of Europe are situate. The Vienna Basin, traversed by the valley of the Danube, comes first, and is rather more fully described, as to both present and past conditions, than the others (pp. 1–16); Paris and the Seine valley comes next (pp. 16–25); then London and the Thames (pp. 25–32); Brussels and the Senne (pp. 32–41); Berlin and the Spree (pp. 42–53); St. Petersburg and the Neva (pp. 53–59); lastly, Rome and the Tiber (pp. 59–68). Transverse geological sections of these valleys, and vertical sections of the deepest wells and borings in the said cities, illustrate this admirably concise and well-considered little book; and the general remarks on these interesting localities, and the special notes on their water-supply and sanitary conditions, still further enhance the value of M. Karrer's admirable work. We must add that the author supplies copious references to authors and observers, and carefully acknowledges the original sections and notes communicated by his many helpful friends.

XLVIII. *Intelligence and Miscellaneous Articles.*

THE SPONTANEOUS THINNING OF LIQUID FILMS. BY J. PLATEAU.

In my memoirs 'On the Figures of Equilibrium of a Liquid Mass without Weight,' and in my 'Statics of Liquids submitted to Molecular Forces only,' I have from time to time insisted that the gradual thinning of liquid films is due, entirely in certain cases, and in great part in the rest, to the suction exerted by the small surfaces with strongly concave transverse curvatures which line the margins of those films. Now, on considering the capillary pressures in themselves, and starting from the principle that the pressure of a plane or relatively but little curved excels that of a strongly concave surface, one may ask if the gradual thinning does not consist in this—that the two superficial layers of the film, by their excess of pressure, press out from between them the interposed layer, driving it towards the very concave portions,—or if the superficial layers participate in this motion. To solve this question I have had recourse to the following experiment. A plane film of glycerine liquid was formed in a ring of iron wire of 7 centim. diameter, supported by three feet, and quite horizontal. While it was still colourless it was sprinkled with a light coat of lycopodium powder; it was then covered over with a glass bell, and was observed while it was gradually being attenuated, which was ascertained by the appearance and succession of the colours. Now, the lycopodium was seen little by little to gain the margin; about half an hour after, it had accumulated along the ring, and the film was clear of it.

Thus, in the act of thinning, the superficial layers travel towards the margins of the films; but as these necessarily adhere to the interposed layer, they could not move without dragging the latter. It must, then, be admitted that the whole of the film partakes of this motion, whether the action be exerted directly upon the superficial layers only, or is exerted directly upon the interposed layer also, or, lastly, only upon the latter.

When the apparatus is not covered with a bell, the slight agitations of the ambient air determine extensive irregular movements in the film, which are manifested by the lycopodium. Let us say further that, to distribute the lycopodium powder properly over the film, a little of it is introduced into a small paper tube, and blown into the air at a certain distance from the apparatus: the powder then descends and deposits itself upon the film in a state of sufficient dissemination.—*Bull. de l'Acad. Roy. de Belgique*, sér. 3, tome ii. no. 7 (1881).

ON SECONDARY BATTERIES. BY J. ROUSSE.

In order to accumulate electricity so as to produce electric light or motive force, I have arranged several secondary batteries which differ notably from that of M. G. Planté.

(1) At the negative pole of the secondary battery I employ a palladium plate, which, during the electrolysis, absorbs more than 900 times its volume of hydrogen. At the positive pole I employ a plate of lead. The liquid electrolyzed is sulphuric acid diluted to 10 per cent. This element is very powerful, even when of small dimensions.

(2) Another secondary element, which has also given good results, is formed, at the negative pole, of a thin plate of sheet-iron: this absorbs more than 200 times its volume of hydrogen when it is electrolyzed in a solution of ammonium sulphate. The positive pole consists of a plate of pure lead coated with litharge, or with pure oxide, or white-lead, or with a mixture of all these substances. These metallic plates dip into a 50-per-cent. solution of sulphate of ammonia.

I have also employed, with some success, other similar combinations. For example:—

At the negative pole a plate of sheet-iron; at the positive pole a cylinder of ferromanganese. The liquid electrolyzed is sulphate of ammonia at 40 per cent.

I have remarked that, in general, for composing a secondary battery it is sufficient to place at the negative pole of the voltameter a metal which possesses the property of absorbing hydrogen when it is placed in a suitable solution. On the other hand, it is necessary to place at the positive pole a metal which absorbs oxygen and becomes peroxidized.—*Comptes Rendus*, Oct. 3, 1881, t. xciii. p. 545.

ELECTRICAL TESTING.

To the Editors of the Philosophical Magazine and Journal.

Electrician's Department, General
Post Office, Oct. 4, 1881.

GENTLEMEN,

In your issue for October last (no. 75, vol. xii.) I notice an article by Mr. Thomas Gray, "On the best Arrangement of the Wheatstone's Bridge for the Measurement of a particular Resistance," in which he says, "so far as I am aware, no one has considered in detail all the different cases." I would beg to point out that, in the second edition of my 'Handbook of Electrical Testing'*, published in the month of June of the present year, I have fully worked out the whole question (including the particular cases referred to by Mr. Gray), and I have done this without the use of the calculus; moreover I draw attention to the fact that, although the calculus method shows the conditions for obtaining an absolute maximum of sensitiveness, yet it does not show (as I have done) that this absolute condition may be very widely departed from without *practically* affecting the sensitiveness of the arrangement.

Yours faithfully, H. R. KEMPE.

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* Published by Messrs. E. and F. N. Spon, Charing Cross.

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[FIFTH SERIES.]

DECEMBER 1881.

XLIX. *On the Theoretic Determination of Vapour-pressure and the Volumes of Vapour and Liquid. By* Prof. R. Clausius*.

§ 1. WHEN a gas is more and more compressed at constant temperature, at a certain pressure, as is known, condensation commences, and is completed without any increase of the pressure; and only when it is finished is an augmentation of the pressure required for a still further diminution of the volume, which pressure must then be increased in proportion very greatly. Besides this actual course of the thing, J. Thomson, as is well known, has imagined another process, which, it is true, cannot in reality take place, because the states of equilibrium occurring in it are in part unstable, but is yet theoretically conceivable—namely, an alteration of volume in which the entire mass is supposed to be constantly homogeneous, and the pressure accordingly to change continuously. The curve which represents the change of pressure corresponding to the change of volume for this last process may be named briefly the *theoretic* isothermal. The real isothermal differs from it by this—that for a certain length, which in the case of compression corresponds to the process of condensation, and, conversely, in that of expansion corresponds to the process of vaporization, the curved line is replaced by a straight line parallel to the axis of abscissæ. This straight line must, as can be demonstrated from the second proposition of the mechanical theory of heat, be situated

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* Translated from Wiedemann's *Annalen,* vol. xiv. pp. 279-290.

so that the external work done in the vaporization is equal to that which would be done in the same increase of volume if the pressure changed in accordance with the theoretic isothermal*.

This principle governing the position of the straight line we can now employ in order to derive, from the general theoretic pressure-formula which holds for all volumes, that pressure which is exerted by saturated vapour. The first published research on this subject which has come to my knowledge is found in an interesting essay by Van der Waals†. The author has, it is true, refrained from communicating in full his calculations and the final equations resulting from them, because the former were too lengthy, and the latter too complicated and, besides, valid only for a limited portion of the curves; but he has collected a series of important consequences drawn therefrom. Another, likewise very valuable, investigation upon the subject has recently been published by Planck‡, in which both the general equations and their special application to carbonic acid are given.

I also had already, before I became acquainted with these investigations, been for a long time occupied with the same subject; and the conclusion of my investigation was delayed only by the toilsomeness of the numerical calculations which were necessary for the comparison of the theoretic formulæ with the data of observation. But now that those investigations of Van der Waals and Planck are published, I think I

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* When, in my paper on the behaviour of carbonic acid (Wied. *Ann.* ix. p. 337, 1880), I determined the position of the straight line in the manner above stated, I treated the question as still an open one. At the same time I had derived my knowledge of Maxwell's views from what I could not but consider the most authoritative source, namely his work upon the Theory of Heat, and, indeed, from the last edition, revised by him, published in 1875. In this edition he has omitted a view enunciated in the earlier editions which differs from the above, but without putting another view in its place, from which I was of course obliged to conclude that he recognized that view as erroneous, but had not yet obtained one more satisfactory to him. I have subsequently learned, from a communication kindly made to me by M. van der Waals, that Maxwell has again spoken upon the subject in another place, and there enunciated a view agreeing with the above,—namely, in a lecture delivered at the meeting of the Chemical Society on the 18th February 1875, which lecture is printed in 'Nature,' March 4 and 11, 1875. Why Maxwell has not mentioned the view there enunciated in the new edition of his work, published in the same year, is unknown to me.

† Van der Waals, *Onderzoekingen omtrent de overeenstemmende eigenschappen der normale verzadigden-damp- en vloeistoflijnen*, Amsterdam, 1880; also admitted into Roth's translation of the work entitled *Over de continuiteit van den gas- en vloeistoftoestand*, Leipzig, 1881.

‡ Planck, Wied. *Ann.* xiii. p. 535 (1881).

must no longer delay the publication of mine; and I will take leave to communicate first, in this paper, the general formulæ, independent of the nature of the particular substances, and a series of numbers relating to them, reserving for another paper the applications to definite substances.

§ 2. The formula which, in my treatise on the behaviour of carbonic acid, I constructed for the representation of pressure as a function of volume and temperature, to be annexed to previous formulæ set up by other authors, is

$$p=\mathrm{R}\frac{\mathrm{T}}{v-\alpha}-\frac{c}{\mathrm{T}(v+\beta)^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which p, v, and T denote pressure, volume, and absolute temperature, and R, c, α, and β are constants. This formula I first formed for carbonic acid, by a comparison with the results of Andrews's observations; and I added, as a conjecture only, that with a different determination of the constants, without any other alteration, it could be applied also to the other gases. When, however, I made the experiment of applying it to those substances for which extended and reliable series of data of observation exist, especially to steam, I found that to bring about a satisfactory accordance a still further alteration of the formula must be undertaken—which I had previously contemplated when occupied with carbonic acid only, but which at that time I abandoned on account of the uncertainty of the data upon which I had been obliged to base the formula. Namely, in the place of the fraction c/T, occurring in the last term, a more general temperature-function, with more indefinite constants, must be put.

Since the more exact knowledge of the temperature-function is not necessary for the general developments here in the first place contemplated, we will, for the present, content ourselves with indicating it by introducing a new symbol. For the convenience of the calculations, however, it is advisable not to choose the new symbol simply to put it in the place of the fraction c/T, but to make it represent another quantity containing that fraction. For this purpose we will give to equation (1) the following form:—

$$\frac{p}{\mathrm{RT}}=\frac{1}{v-\alpha}-\frac{c}{\mathrm{RT}^2(v+\beta)^2};$$

and herein the fraction $\frac{c}{\mathrm{RT}^2}$ may be replaced by $\frac{27(\alpha+\beta)}{8\theta}$, in which θ shall denote the temperature-function left undetermined, of which we may provisionally say only that for $\mathrm{T}=0$ it likewise has the value 0, and for the critical temperature it has the value 1. By this substitution the foregoing equation is transformed into

$$\frac{p}{\mathrm{RT}}=\frac{1}{v-\alpha}-\frac{27(\alpha+\beta)}{8\theta(v+\beta)^2}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In order to apply this equation to the process of vaporization, we will, for distinction, denote the pressure of saturated vapour by P, and employ for the volume of the saturated vapour and the liquid standing under the same pressure the symbols s and σ, which I have also previously used. Now, as the equation must hold good for the liquid as well as for the saturated vapour, we can form from it the following two equations:—

$$\frac{\mathrm{P}}{\mathrm{RT}}=\frac{1}{\sigma-\alpha}-\frac{27(\alpha+\beta)}{8\theta(\sigma+\beta)^2}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{\mathrm{P}}{\mathrm{RT}}=\frac{1}{s-\alpha}-\frac{27(\alpha+\beta)}{8\theta(s+\beta)^2}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Further, to express that the external work performed in the vaporization must be equal to that which would be obtained with the same increase of volume if the pressure changed in accordance with the theoretic isothermal and the formula corresponding thereto, we have to put

$$\mathrm{P}(s-\sigma)=\int_\sigma^s p\,dv;$$

and if in this we put for p the value determined by equation (2), then perform the integration, and divide the resulting equation by RT, we get

$$\frac{\mathrm{P}}{\mathrm{RT}}(s-\sigma)=\log\frac{s-\alpha}{\sigma-\alpha}-\frac{27(\alpha+\beta)}{8\theta}\left(\frac{1}{\sigma+\beta}-\frac{1}{s+\beta}\right). \quad . \quad (5)$$

For convenience we will also introduce the following simplified symbols :—

$$\left.\begin{array}{ll}\Pi=\frac{\mathrm{P}}{\mathrm{RT}}, & \gamma=\alpha+\beta, \\ w=\sigma-\alpha, & \mathrm{W}=s-\alpha.\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Equations (3), (4), and (5) then become :—

$$\Pi=\frac{1}{w}-\frac{27\gamma}{8\theta(w+\gamma)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{I.})$$

$$\Pi=\frac{1}{\mathrm{W}}-\frac{27\gamma}{8\theta(\mathrm{W}+\gamma)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{II.})$$

$$\Pi(\mathrm{W}-w)=\log\frac{\mathrm{W}}{w}-\frac{27\gamma}{8\theta}\left(\frac{1}{w+\gamma}-\frac{1}{\mathrm{W}+\gamma}\right). \quad (\mathrm{III.})$$

These three equations are to be employed for the calculation, because from them the values of Π, w, and W for every value of θ can be determined, which determination further leads to the determination of Π, w, and W for every value of T also, if θ is known as a function of T.

§ 3. If we wished to carry out the calculation with the aim of expressing Π, w, and W directly as functions of θ, we should have to treat a transcendental equation which cannot be solved in a closed form. Hence, as Planck rightly says, it is better first to determine all four quantities Π, w, W, and θ as functions of a suitably chosen new variable. Planck has selected as such new variable an angular magnitude ϕ, which he provisionally unites with another quantity r, and, together with this, defines by the following equations:—

$$W=r\cos^2\frac{\phi}{2};\quad w=r\sin^2\frac{\phi}{2}.$$

I, on the contrary, have chosen, in my calculations, simply the quantity $\log(W/w)$, occurring in equation (III.), as the new variable, which I have denoted by λ.

Before introducing this symbol into the above equations, we will somewhat further transform them. From (I.) and (II.) follows directly:—

$$\frac{1}{w}-\frac{27\gamma}{8\theta(w+\gamma)^2}=\frac{1}{W}-\frac{27}{8\theta(W+\gamma)^2};$$

and from this we get

$$\frac{27\gamma}{8\theta}=\frac{\frac{1}{w}-\frac{1}{W}}{\frac{1}{(w+\gamma)^2}-\frac{1}{(W+\gamma)^2}},$$

or, transformed,

$$\frac{27\gamma}{8\theta}=\frac{(W+\gamma)^2(w+\gamma)^2}{Ww(W+w+2\gamma)}. \quad \ldots \quad (7)$$

Inserting this value of $27\gamma/8\theta$ in equation (I.), we obtain

$$\Pi=\frac{1}{w}-\frac{(W+\gamma)^2}{Ww(W+w+2\gamma)},$$

which expression can be transformed into

$$\Pi=\frac{1}{W+w+2\gamma}\left(1-\frac{\gamma^2}{Ww}\right). \quad \ldots \quad (8)$$

Lastly, with respect to equation (III.), which may now be written in the following order—

$$\log\frac{W}{w}=\Pi(W-w)+\frac{27\gamma}{8\theta}\left(\frac{1}{w+\gamma}-\frac{1}{W+\gamma}\right),$$

it changes, if for $27\gamma/8\theta$ and Π we put the values given under (7) and (8) and then again reduce, into the following:—

$$\log\frac{W}{w}=\frac{(W-w)(2Ww+\gamma W+\gamma u)}{Ww(W+w+2\gamma)}. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

To this equation we will now apply the equation

$$\lambda=\log\frac{W}{w}, \quad . \quad . \quad . \quad \quad . \quad . \quad . \quad (10)$$

and the equation resulting from it,

$$W=we^{\lambda}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

by which we get

$$\lambda=(e^{\lambda}-1)\frac{2we^{\lambda}+\gamma(e^{\lambda}+1)}{e^{\lambda}[\omega^{\lambda}(e+1)+2\gamma]};$$

or, differently written,

$$\lambda=(1-e^{-\lambda})\frac{2w+\gamma(1+e^{-\lambda})}{w(1+e^{-\lambda})+2\gamma e^{-\lambda}}. \quad . \quad . \quad . \quad (12)$$

This equation can easily be resolved with respect to w, and gives

$$w=\gamma\frac{1-2\lambda e^{-\lambda}-e^{-2\lambda}}{\lambda-2+(\lambda+2)e^{-\lambda}}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and from this, according to (11), it further results immediately that

$$W=\gamma e^{\lambda}\frac{1-2\lambda e^{-\lambda}-e^{-2\lambda}}{\lambda-2+(\lambda+2)e^{-\lambda}}. \quad . \quad . \quad . \quad . \quad . \quad (14)$$

If the quantities w and W are once calculated, equation (8) can be employed for the calculation of the quantity Π. But if we wish to represent Π as a function of λ, we must put for w and W in (8) the expressions given under (13) and (14); we then get, after some reductions:—

$$\Pi=\frac{e^{-\lambda}[\lambda-2+(\lambda+2)e^{-\lambda}].[(1-e^{-\lambda})^2-\lambda^2e^{-\lambda}]}{\gamma(1-e^{-\lambda})(1-2\lambda e^{-\lambda}-e^{-2\lambda}]^2}. \quad . \quad (15)$$

For the determination of the last quantity, θ, it follows from (7) that

$$\theta=\frac{27\gamma}{8}\frac{Ww(W+w+2\gamma)}{(W+\gamma)^2(w+\gamma)^2}; \quad . \quad . \quad . \quad . \quad (16)$$

and if in this we insert for w and W their values from (13) and (14), we get

$$\theta=\frac{27}{8}\,\frac{[\lambda-2+(\lambda+2)e^{-\lambda}](1-2\lambda e^{-\lambda}-e^{-2\lambda})^2}{(1-e^{-\lambda})(\lambda-1+e^{-\lambda})^2(1-e^{-\lambda}-\lambda e^{-\lambda})^2}. \quad (17)$$

By equations (13), (14), (15), and (17) what was aimed at is attained, namely to express the four quantities w, W, Π, and θ by one and the same quantity λ.

§ 4. If we expand the expressions found in series which proceed by powers of λ, we encounter a peculiar behaviour: in nearly all the factors which occur in the numerators and denominators the terms which are independent of λ and those affected with low powers of λ cancel one another, so that all the numerators and denominators have pretty high powers of λ for factors, which, indeed, then cancel one another in the fractions. The respective series presenting the factors are as follows:—

$$1-e^{-\lambda}=\lambda\left(1-\frac{1}{2!}\lambda+\frac{1}{3!}\lambda^2-\frac{1}{4!}\lambda^3+\frac{1}{5!}\lambda^4-\ldots\right)$$

$$\lambda-1+e^{-\lambda}=\lambda^2\left(\frac{1}{2!}-\frac{1}{3!}\lambda+\frac{1}{4!}\lambda^2-\frac{1}{5!}\lambda^3+\frac{1}{6!}\lambda^4-\ldots\right)$$

$$1-e^{-\lambda}-\lambda e^{-\lambda}=\lambda^2\left(\frac{1}{2!}-\frac{2}{3!}\lambda+\frac{3}{4!}\lambda^2-\frac{4}{5!}\lambda^3+\frac{5}{6!}\lambda^4-\ldots\right)$$

$$\lambda-2+(\lambda+2)e^{-\lambda}=\lambda^3\left(\frac{1}{3!}-\frac{2}{4!}\lambda+\frac{3}{5!}\lambda^2-\frac{4}{6!}\lambda^3+\frac{5}{7!}\lambda^4-\ldots\right)$$

$$1-2\lambda e^{-\lambda}-e^{-2\lambda}=2\lambda^3\left(\frac{1}{3!}-\frac{4}{4!}\lambda+\frac{11}{5!}\lambda^2-\frac{26}{6!}\lambda^3+\frac{57}{7!}\lambda^4-\ldots\right)$$

$$(1-e^{-\lambda})^2-\lambda^2e^{-\lambda}=\lambda^4\left(\frac{1}{2.3!}-\frac{2}{4!}\lambda+\frac{16}{3.5!}\lambda^2-\frac{12}{6!}\lambda^3+\frac{99}{4.7!}\lambda^4-\ldots\right)$$

Applying these expressions to the equations (13) and (14), and effecting in these the indicated multiplication and division, we obtain

$$w=\gamma\left(2-\lambda+\frac{3}{2.5}\lambda^2-\frac{1}{3.5}\lambda^3+\frac{17}{2^3.5^2.7}\lambda^4-\frac{1}{3.5^2.7}\lambda^5+\ldots\right), \quad (18)$$

$$\mathrm{W}=\gamma\left(2+\lambda+\frac{3}{2.5}\lambda^2+\frac{1}{3.5}\lambda^3+\frac{17}{2^3.5^2.7}\lambda^4+\frac{1}{3.5^2.7}\lambda^5+\ldots\right). \quad (19)$$

From this we see, what can also be otherwise demonstrated to be necessary, that the terms with even powers of λ are equal in the two expressions, and the terms with odd powers are equal and with opposite signs. Hence we can introduce two new quantities, M and N, containing only even powers of λ, namely

$$\left.\begin{aligned} M&=\gamma\left(2+\frac{3}{2.5}\lambda^2+\frac{17}{2^3.5^2.7}\lambda^4+\ldots\right),\\ N&=\gamma\left(1+\frac{1}{3.5}\lambda^2+\frac{1}{3.5^2.7}\lambda^4+\ldots\right);\end{aligned}\right\} \quad . \quad . \quad (20)$$

and then put

$$w=M-N\lambda, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

$$W=M+N\lambda. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

From the last two equations it follows that

$$W+w=2M, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

$$Ww=M^2-N^2\lambda^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

from which it is evident that the sum and the product of the two quantities W and w contain only even powers of λ. Now, as in the expressions of Π and θ given under (8) and (16) the quantities W and w occur only in combination as sum and product, it thence follows that the quantities Π and θ also contain only even powers of λ. This implies that in the vicinity of the critical temperature, where λ approaches the value *nil*, the quantities Π and θ behave in an essentially different manner from the quantities W and w. We shall revert to this further on.

§ 5. The equations hitherto evolved, presenting the four quantities w, W, Π, and θ as functions of λ, of course thereby determine indirectly the connexion in which each of the three quantities w, W, and Π stands with the quantity θ; but this indirect determination, effected by means of a third quantity, does not completely satisfy the requirements. In investigations of the process of vaporization, the temperature is usually considered as the given quantity, and it is wished to deduce from it immediately the pressure of the vapour and the volumes of the vapour and the liquid. In this sense, therefore, must we render more perfect our mode of determination. As in our developments hitherto the temperature does not occur explicitly, but only the yet undetermined temperature-function θ, we must provisionally attach the determination to this temperature-function and set ourselves the problem of arranging so that from the value of θ the corresponding values of w, W, and Π shall be obtained in the simplest possible manner. This I have endeavoured to accomplish by calculating a table giving for the different values of θ, increasing one hundredth at a time, the corresponding values of λ. From this table λ can easily be determined, by interpolation, for any value whatever of θ; and when λ is known, w, W, and Π can be directly calculated with the aid of the above formulæ.

To calculate the table, I first represented λ by a series progressing by powers of a quantity dependent on θ. For this the following quantity appeared to me the most suitable,

$$x=\sqrt{1-\theta}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (25)$$

which, like λ, when the critical temperature is approached, approaches the value *nil.* The series in question is

$$\lambda=6x+3{\cdot}24x^3+2{\cdot}8801716x^5+2{\cdot}885628x^7+\ldots. \qquad (26)$$

Before speaking of the application of this series to the calculation, a consequence resulting even from its form may be interpolated, which is connected with the remark made at the close of the preceding section. As is seen, the series contains only odd powers of x; and thence it immediately follows that the series which represents λ^2 can only contain even powers of x. Since, further, as mentioned above, the quantity Π, when the expansion takes place with respect to λ, contains only even powers of λ, it can only contain even powers of x, according to the foregoing, when expanded with respect to x, while the series representing the quantities w and W contain also terms with odd powers, and among them a term of the first order. We now get from (25), for the differential coefficients of x and x^2 with respect to θ, the following expressions,

$$\left.\begin{aligned}\frac{dx}{d\theta}&=-\frac{1}{2\sqrt{1-\theta}},\\ \frac{d(x^2)}{d\theta}&=-1,\end{aligned}\right\} \quad . \quad . \quad . \qquad (27)$$

which as the critical temperature is approached (for which $\theta=1$) differ essentially from each other in their behaviour, in that the former becomes infinitely great, while the latter remains finite. In just the same way must, according to what has been said above, the differential coefficients of the quantities w and W, taken with respect to θ, differ from the differential coefficients of the quantity Π. It may here be likewise added that the same holds good also for the differential coefficients taken with respect to T; and from this it follows that on approaching the critical temperature the volumes of the liquid and the vapour undergo changes which are infinitely great in proportion to the change of temperature. Van der Waals has already called attention to this singular difference.

With the help of the above series I have calculated λ for those values of θ and x for which that number of terms suffices for attaining the desired degree of accuracy. For higher values of x, and consequently lower values of θ, I returned to equation (17), from which, for given values of λ, the corresponding values of θ can be calculated, and which, conversely, by employing an approximation method, can also be used to determine, for given values of θ, the corresponding values of λ.

The table thus calculated, in which, in order to facilitate interpolation, the difference between each two succeeding numbers is added, follows at the end of the paper. If from this table the value of λ belonging to a given value of θ be taken, then, as already said, with the aid of this the corresponding values of w, W, and Π can be directly calculated from the above equations. Nor, after the table for λ is once calculated, is there any further difficulty in calculating tables of the same sort for w, W, and Π, giving for the same series of values of θ the corresponding values of those quantities.

θ	λ	Δ	θ	λ	Δ	θ	λ	Δ
0	∞		0·39	8·4767		0·70	4·0229	
0·05	67·4947	—	0·40	8·2507	0·2260	0·71	3·9220	0·1009
0·10	33·7185	—	0·41	8·0348	0·2159	0·72	3·8219	0·1001
0·11	30·6469	3·0716	0·42	7·8280	0·2068	0·73	3·7225	0·0994
0·12	28·0867	2·5602	0·43	7·6296	0·1984	0·74	3·6238	0·0987
0·13	25·9197	2·1670	0·44	7·4392	0·1904	0·75	3·5255	0·0983
0·14	24·0618	1·8579	0·45	7·2561	0·1831	0·76	3·4277	0·0978
0·15	22·4511	1·6107	0·46	7·0797	0·1764	0·77	3·3301	0·0976
0·16	21·0412	1·4099	0·47	6·9096	0·1701	0·78	3·2327	0·0974
0·17	19·7968	1·2444	0·48	6·7453	0·1643	0·79	3·1353	0·0974
0·18	18·6901	1·1067	0·49	6·5864	0·1589	0·80	3·0376	0·0977
0·19	17·6995	0·9906	0·50	6·4326	0·1538	0·81	2·9398	0·0978
0·20	16·8074	0·8921	0·51	6·2834	0·1492	0·82	2·8414	0·0984
0·21	16·0000	0·8074	0·52	6·1387	0·1447	0·83	2·7424	0·0990
0·22	15·2655	0·7345	0·53	5·9980	0·1407	0·84	2·6425	0·0999
0·23	14·5944	0·6711	0·54	5·8612	0·1368	0·85	2·5416	0·1009
0·24	13·9788	0·6156	0·55	5·7278	0·1334	0·86	2·4393	0·1023
0·25	13·4119	0·5669	0·56	5·5978	0·1300	0·87	2·3354	0·1039
0·26	12·8882	0·5237	0·57	5·4709	0·1269	0·88	2·2295	0·1059
0·27	12·4028	0·4854	0·58	5·3469	0·1240	0·89	2·1212	0·1083
0·28	11·9516	0·4512	0·59	5·2256	0·1213	0·90	2·00995	0·1112
0·29	11·5309	0·4207	0·60	5·1068	0·1188	0·91	1·89517	0·11478
0·30	11·1376	0·3933	0·61	4·9904	0·1164	0·92	1·77604	0·11913
0·31	10·7691	0·3685	0·62	4·8761	0·1143	0·93	1·65147	0·12457
0·32	10·4230	0·3461	0·63	4·7639	0·1122	0·94	1·52001	0·13146
0·33	10·0971	0·3259	0·64	4·6535	0·1104	0·95	1·37956	0·14045
0·34	9·7896	0·3075	0·65	4·5448	0·1087	0·96	1·22688	0·15268
0·35	9·4989	0·2907	0·66	4·4377	0·1071	0·97	1·05653	0·17035
0·36	9·2234	0·2754	0·67	4·3321	0·1056	0·98	0·85786	0·19867
0·37	8·9621	0·2614	0·68	4·2279	0·1042	0·99	0·60327	0·25459
0·38	8·7135	0·2486	0·69	4·1248	0·1031	1	0	0·60327
0·39	8·4767	0·2368	0·70	4·0229	0·1019			

Bonn, August 1881.

L. *On Skew Determinants.*

By Thomas Muir, *M.A., F.R.S.E.*[*]

1. In 1855 it was proved by Brioschi[†], and, it is said, simultaneously by Cayley, that any determinant of even order is expressible as a Pfaffian. The restriction of the statement to even-ordered determinants has since remained: I propose now to show that it may be done away with.

2. In the Quarterly Journal of Mathematics for this month (p. 174) there is given a new form for the product of two determinants; viz. when the order is the third, we have

$$\begin{vmatrix} \alpha_1 & \alpha_2 & \alpha_3 \\ \beta_1 & \beta_2 & \beta_3 \\ \gamma_1 & \gamma_2 & \gamma_3 \end{vmatrix} \times \begin{vmatrix} x_1 & x_2 & x_3 \\ y_1 & y_2 & y_3 \\ z_1 & z_2 & z_3 \end{vmatrix}$$

$$= \frac{1}{2^6} \begin{vmatrix} \alpha_1+x_1 & \alpha_2+x_2 & \alpha_3+x_3 & \alpha_3-x_3 & \alpha_2-x_2 & \alpha_1-x_1 \\ \beta_1+y_1 & \beta_2+y_2 & \beta_3+y_3 & \beta_3-y_3 & \beta_2-y_2 & \beta_1-y_1 \\ \gamma_1+z_1 & \gamma_2+z_2 & \gamma_3+z_3 & \gamma_3-z_3 & \gamma_2-z_2 & \gamma_1-z_1 \\ \gamma_1-z_1 & \gamma_2-z_2 & \gamma_3-z_3 & \gamma_3+z_3 & \gamma_2+z_2 & \gamma_1+z_1 \\ \beta_1-y_1 & \beta_2-y_2 & \beta_3-y_3 & \beta_3+y_3 & \beta_2+y_2 & \beta_1+y_1 \\ \alpha_1-x_1 & \alpha_2-x_2 & \alpha_3-x_3 & \alpha_3+x_3 & \alpha_2+x_2 & \alpha_1+x_1 \end{vmatrix}.$$

If, in order to find from this an expression for the second power of a determinant, we put $x_1, x_2, x_3, \ldots = \alpha_1, \alpha_2, \alpha_3, \ldots$ the result is nugatory; but, making the columns of the second determinant the same as the rows of the first, we obtain

$$|\alpha_1\beta_2\gamma_3|^2 = 2^{-6} \begin{vmatrix} 2\alpha_1 & \alpha_2+\beta_1 & \alpha_3+\gamma_1 & \alpha_3-\gamma_1 & \alpha_2-\beta_1 & 0 \\ \beta_1+\alpha_2 & 2\beta_2 & \beta_3+\gamma_2 & \beta_3-\gamma_2 & 0 & \beta_1-\alpha_2 \\ \gamma_1+\alpha_3 & \gamma_2+\beta_3 & 2\gamma_3 & 0 & \gamma_2-\beta_3 & \gamma_1-\alpha_3 \\ \gamma_1-\alpha_3 & \gamma_2-\beta_3 & 0 & 2\gamma_3 & \gamma_2+\beta_3 & \gamma_1+\alpha_3 \\ \beta_1-\alpha_2 & 0 & \beta_3-\gamma_2 & \beta_3+\gamma_2 & 2\beta_2 & \beta_1+\alpha_2 \\ 0 & \alpha_2-\beta_1 & \alpha_3-\gamma_1 & \alpha_3+\gamma_1 & \alpha_2+\beta_1 & 2\alpha_1 \end{vmatrix}.$$

By changing the signs of the elements in the last three rows and reversing the order of the six rows, the determinant becomes skew with respect to its zero-diagonal; and hence, on

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[*] Communicated by the Author.

[†] Crelle's *Journal*, lii. pp. 133–141.

extracting the square root of both sides, there results

$$\begin{vmatrix} \alpha_1 & \alpha_2 & \alpha_3 \\ \beta_1 & \beta_2 & \beta_3 \\ \gamma_1 & \gamma_2 & \gamma_3 \end{vmatrix} = \left| \begin{array}{ccccc} \tfrac{1}{2}(\alpha_2-\beta_1) & \tfrac{1}{2}(\alpha_3-\gamma_1) & \tfrac{1}{2}(\alpha_3+\gamma_1) & \tfrac{1}{2}(\alpha_2+\beta_1) & \alpha_1 \\ & \tfrac{1}{2}(\beta_3-\gamma_2) & \tfrac{1}{2}(\beta_3+\gamma_2) & \beta_2 & \tfrac{1}{2}(\beta_1+\alpha_2) \\ & & \gamma_3 & \tfrac{1}{2}(\gamma_2+\beta_3) & \tfrac{1}{2}(\gamma_1+\alpha_3) \\ & & & \tfrac{1}{2}(\beta_3-\gamma_2) & \tfrac{1}{2}(\alpha_3-\gamma_1) \\ & & & & \tfrac{1}{2}(\alpha_2-\beta_1) \end{array} \right| . \quad \text{(I.)}$$

Similarly we have the determinant of the fourth order $|\,\alpha_1\beta_2\gamma_3\delta_4\,|$ equal to

$$\left| \begin{array}{ccccccc} \tfrac{1}{2}(\alpha_2-\beta_1) & \tfrac{1}{2}(\alpha_3-\gamma_1) & \tfrac{1}{2}(\alpha_4-\delta_1) & \tfrac{1}{2}(\alpha_4+\delta_1) & \tfrac{1}{2}(\alpha_3+\gamma_1) & \tfrac{1}{2}(\alpha_2+\beta_1) & \alpha_1 \\ & \tfrac{1}{2}(\beta_3-\gamma_2) & \tfrac{1}{2}(\beta_4-\delta_2) & \tfrac{1}{2}(\beta_4+\delta_2) & \tfrac{1}{2}(\beta_3+\gamma_2) & \beta_2 & \tfrac{1}{2}(\alpha_2+\beta_1) \\ & & \tfrac{1}{2}(\gamma_4-\delta_3) & \tfrac{1}{2}(\gamma_4+\delta_3) & \gamma_3 & \tfrac{1}{2}(\beta_3+\gamma_2) & \tfrac{1}{4}(\alpha_3+\gamma_1) \\ & & & \delta_4 & \tfrac{1}{2}(\gamma_4+\delta_3) & \tfrac{1}{2}(\beta_4+\delta_2) & \tfrac{1}{2}(\alpha_4+\delta_1) \\ & & & & \tfrac{1}{2}(\gamma_4-\delta_3) & \tfrac{1}{2}(\beta_4-\delta_2) & \tfrac{1}{2}(\alpha_4-\delta_1) \\ & & & & & \tfrac{1}{2}(\beta_3-\gamma_2) & \tfrac{1}{2}(\alpha_3-\gamma_1) \\ & & & & & & \tfrac{1}{2}(\alpha_2-\beta_1) \end{array} \right| . \quad \text{(II.)}$$

and it is seen to be generally true that *any determinant is expressible as a Pfaffian.*

The Pfaffian, it may be noted, is symmetric with respect to its diagonal or axis.

3. Brioschi's result for even-ordered determinants being different in character from that obtained in the above way, there is thus brought to light an identity between two Pfaffians —an identity leading to the theorem that *a symmetric Pfaffian of the* 2n*th order is expressible as a Pfaffian of the* n*th order.*

4. Again, as the product of two determinants is expressible as a determinant in a way different from that of § 2, and each of the said three determinants is expressible as a symmetric Pfaffian, it follows that *the product of two symmetric Pfaffians is expressible as a symmetric Pfaffian.*

Other examples besides this of the application of our fundamental theorem will readily present themselves.

5. By skew or symmetric determinants are usually meant determinants which are skew or symmetric with respect to the principal diagonal, "skew" and "symmetric" being contrasted terms. There is, however, possible another kind of symmetry, and therefore also another kind of skewness, viz. with respect to the centre, or point of intersection of the two diagonals.

In a centre-skew determinant, the *m*th row from the beginning with its elements reversed in order and their signs all changed, becomes the *m*th row from the end.

A centre-skew determinant of the 2*n*th order is equal in magnitude to the corresponding centro-symmetric determinant, the sign-factor necessary for perfect equivalence being $(-1)^n$.

These are self-evident propositions; but when the order is the $(2n+1)$th, the determinant possesses a property which is not so readily apparent. For shortness in writing let us take a special case, the determinant of the 7th order,

$$\begin{vmatrix} a_1 & a_2 & a_3 & a_4 & a_5 & a_6 & a_7 \\ b_1 & b_2 & b_3 & b_4 & b_5 & b_6 & b_7 \\ c_1 & c_2 & c_3 & c_4 & c_5 & c_6 & c_7 \\ d_1 & d_2 & d_3 & \omega & -d_3 & -d_2 & -d_1 \\ -c_7 & -c_6 & -c_5 & -c_4 & -c_3 & -c_2 & -c_1 \\ -b_7 & -b_6 & -b_5 & -b_4 & -b_3 & -b_2 & -b_1 \\ -a_7 & -a_6 & -a_5 & -a_4 & -a_3 & -a_2 & -a_1 \end{vmatrix} \text{ or } \Delta \text{, say.}$$

If we increase each element of the first row by the corresponding element of the last row, each element of the second row by the corresponding element of the second row from the end, and each element of the third row by the corresponding element of the third row from the end, there results:—

$$\Delta = \begin{vmatrix} a_1-a_7 & a_2-a_6 & a_3-a_5 & 0 & a_5-a_3 & a_6-a_2 & a_7-a_1 \\ b_1-b_7 & b_2-b_6 & b_3-b_5 & 0 & b_5-b_3 & b_6-b_2 & b_7-b_1 \\ c_1-c_7 & c_2-c_6 & c_3-c_5 & 0 & c_5-c_3 & c_6-c_2 & c_7-c_1 \\ d_1 & d_2 & d_3 & \omega & -d_3 & -d_2 & -d_1 \\ -c_7 & -c_6 & -c_5 & -c_4 & -c_3 & -c_2 & -c_1 \\ -b_7 & -b_6 & -b_5 & -b_4 & -b_3 & -b_2 & -b_1 \\ -a_7 & -a_6 & -a_5 & -a_4 & -a_3 & -a_2 & -a_1 \end{vmatrix}$$

Treating this determinant in the same way, but dealing with columns instead of rows, we have

$$\Delta = \begin{vmatrix} a_1-a_7 & a_2-a_6 & a_3-a_5 & 0 & 0 & 0 & 0 \\ b_1-b_7 & b_2-b_6 & b_3-b_5 & 0 & 0 & 0 & 0 \\ c_1-c_7 & c_2-c_6 & c_3-c_5 & 0 & 0 & 0 & 0 \\ d_1 & d_2 & d_3 & \omega & 0 & 0 & 0 \\ -c_7 & -c_6 & -c_5 & -c_4 & -c_3-c_5 & -c_2-c_6 & -c_1-c_7 \\ -b_7 & -b_6 & -b_5 & -b_4 & -b_3-b_5 & -b_2-b_6 & -b_1-b_7 \\ -a_7 & -a_6 & -a_5 & -a_4 & -a_3-a_5 & -a_2-a_6 & -a_1-a_7 \end{vmatrix}$$

$$= \begin{vmatrix} a_1-a_7 & a_2-a_6 & a_3-a_5 \\ b_1-b_7 & b_2-b_6 & b_3-b_5 \\ c_1-c_7 & c_2-c_6 & c_3-c_5 \end{vmatrix} \times \omega \times \begin{vmatrix} -c_3-c_5 & -c_2-c_6 & -c_1-c_7 \\ -b_3-b_5 & -b_2-b_6 & -b_1-b_7 \\ -a_3-a_5 & -a_2-a_6 & -a_1-a_7 \end{vmatrix} . \quad \text{(I}$$

If $\omega = 0$, then $\Delta = 0$; so that, corresponding to Cayley's theorem, "An odd-ordered determinant which is skew with respect to a zero diagonal vanishes," we have an exact counterpart, viz. "*An odd-ordered determinant which is skew with respect to a zero centre vanishes.*" The latter, too, may be proved exactly like the former, viz. by multiplying the elements of every row by -1, and thus showing that $\Delta = -\Delta$.

But, further, the product of the two determinants in (III.) is equal to

$$(-1)^3 \begin{vmatrix} a_1-a_7 & a_2-a_6 & a_3-a_5 \\ b_1-b_7 & b_2-b_6 & b_3-b_5 \\ c_1-c_7 & c_2-c_6 & c_3-c_5 \end{vmatrix} \begin{vmatrix} a_1+a_7 & a_2+a_6 & a_3+a_5 \\ b_1+b_7 & b_2+b_6 & b_3+b_5 \\ c_1+c_7 & c_2+c_6 & c_3+c_5 \end{vmatrix},$$

and therefore, by Zehfuss's theorem, is equal to

$$(-1)^3 \begin{vmatrix} a_1 & a_2 & a_3 & a_5 & a_6 & a_7 \\ b_1 & b_2 & b_3 & b_5 & b_6 & b_7 \\ c_1 & c_2 & c_3 & c_5 & c_6 & c_7 \\ c_7 & c_6 & c_5 & c_3 & c_2 & c_1 \\ b_7 & b_6 & b_5 & b_3 & b_2 & b_1 \\ a_7 & a_6 & a_5 & a_3 & a_2 & a_1 \end{vmatrix},$$

that is to say, is equal to the centre-skew determinant obtained from Δ by deleting the middle row and middle column. Hence the theorem—*A centre-skew determinant of odd order is not altered by making all the elements of the middle row and middle column zero with the exception of the centre element.*

Instead of deducing this from (III.) we might reverse the order, viz. prove the theorem just enunciated by expanding Δ according to binary products of the elements of the middle row and middle column, and then by means of this theorem establish (III.).

Beechcroft, Bishopton, N. B.,
October 8, 1881.

LI. *Experimental Investigations on Magnetic Rotatory Polarization in Gases.* *By* M. Henri Becquerel.

[Concluded from p. 324.]

Discussion of the Sources of Error: Corrections.

In measurements so delicate as these about to be recorded, it was of the highest importance that the effect of the various disturbing causes proceeding from the apparatus itself should be most carefully studied and measured. Although the corrections which it was necessary to make in the results observed were very small, yet, as they were of the same order of magnitude as the latter, it was essential to determine them with the greatest accuracy.

They may be grouped in the following manner :—

I. Variations in electromagnetic intensity.
II. Want of homogeneity in the luminous source.
III. Magnetic rotations caused by the glasses and the glass of the mirrors interposed in the course of the luminous rays.
IV. Corrections due to variations of temperature in the gases.
V. Displacement of the luminous point.

I. *Variations in Electromagnetic Intensity.*—We have already stated that the heating of the conductors during the passage of the electric current and the operation of the pile gave rise to a regular variation in the electromagnetic intensity, and that the intensity of the induced current, which is in proportion to that of the principal electric current, was measured repeatedly. The numbers obtained enabled the observed results to be reduced by a simple proportion to what they would be if the intensity remained constant. The accuracy of these numbers will therefore depend on the precision with which the intensities are measured. We made use of a sine-compass placed at 10·50 metres from the middle of the large solenoid in the apparatus. At this distance the magnetic influence on the compass was still sensible, and deflected the needle about 10′, either to the right or left, according to the direction of the current in the bobbins. In order to eliminate this disturbing element, each determination of the intensity consisted of two successive measurements, made by reversing the direction of the electric current in the bobbins; and the average of the two deviations obtained was taken as the definitive measurement.

During the course of a series of observations these determinations were repeated very frequently, every ten or fifteen minutes; and it is certain that, for the measurements included in these intervals of time, the numbers obtained by interpolation gave the intensity of the current with a precision equal to that of direct determinations.

The deviations of the compass varied between 25° and 22°; the total variation from morning to evening for the same series scarcely amounted to 2°. All the results were reduced to what they would have been if the deviation had been constantly 24°. The error in the measurements of the angles did not exceed 1′; so that, with respect to the intensities, the error could not amount to 0·001 of their value. This accuracy is greater than that of the numbers obtained for the magnetic rotations of the gases.

II. *Want of Homogeneity in the Luminous Source.*—A shaded polariscope, formed of a divided nicol, such as the one employed by us, might be used for studying light of any colour whatever, provided that the luminous rays of different wave-lengths experienced the same action. Under the influence of magnetism, on the contrary, the planes of polarization of the different rays are unequally deflected, the image from the polariscope is tinged with various colours, and before any measurements are possible it is necessary to make use of a monochromatic source of light. On the other hand, our apparatus needed an extremely brilliant source of light, on account of the very long distance generally traversed by the luminous rays before reaching the eye: the passage through the glasses and the successive reflections are all causes of loss of light; and equality of tint is obtained by almost extinguishing the images, and preserving not more than about 0·0019* of the intensity of the incident ray.

I first attempted to direct the blast of a compound blowpipe onto a large piece of fused chloride of sodium. This light is extremely bright for a moment; but the salt begins to melt very rapidly, without giving time for a measurement to be completed by reversing the direction of the electric current. It is true that the light might be maintained by progressively displacing the piece of chloride of sodium, which melts only at one point; but this gives rise to a very grave cause of error: the luminous point is displaced; and this displacement causes corresponding deviations of the plane of polarization, which completely disturb the phenomena due to magnetic influence. I was therefore obliged to relinquish the employment of this source of light.

When the piece of chloride of sodium just spoken of is replaced by a piece of quicklime, the light emitted by the incandescent lime is composed of nearly all the rays which form the visible part of the solar spectrum, but the luminous point has the advantage of remaining almost perfectly fixed.

As the rotations observed in the apparatus are very slight, the images are only very faintly coloured, and the measurements of equality of tint can be made with great exactitude. They become still clearer if a coloured screen, which allows

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* This number is obtained as follows:—The position of the analyzer which corresponds to the equality of tint is at 2° 30′ from those which correspond to the extinction of each half of the image from the polariscope. In this position, therefore, the luminous intensity is represented by $\cos^2 87° 30' = 0·001900$, the intensity of the incident ray being taken as unity.

only a narrow portion of the spectrum to pass, is placed between the source of light and the polariscope.

In this point of view, the want of homogeneity in the source of light causes only a slight perturbation in the appreciation of the equality of tint of the two halves of the image; nevertheless an imperfect knowledge of the wave-length of the luminous rays which reach the eye causes a serious uncertainty, which it is most desirable to obviate.

In fact, it usually happens, as will be shown later, that the magnetic rotations of the planes of polarization of luminous rays of different wave-lengths traversing a gaseous column, are very nearly in the inverse ratio of the squares of their wave-lengths. The slightest variation, then, in the nature of the rays, of which the equality of tint is being estimated, modifies the size of the corresponding magnetic rotation very considerably; and our apparatus was sufficiently sensitive to demonstrate these variations.

A little difference in the pressure of the oxygen in the blowpipe causes a variation in the temperature of the incandescent lime, and consequently in the quality of the light emitted; for the higher the temperature the richer the light becomes in refrangible rays. At each passage through the glasses, and at each reflection on the mirrors, a small quantity of light is lost; so that the colour of all the reflected images is not exactly the same. In proportion as the number of reflections increases, the colour of the corresponding images approaches the colour emitted in large quantity by the source.

In order to profit fully by the sensitiveness of the apparatus, and to effect the measurements with the utmost precision of which they were capable, it was necessary, in each of the experiments, to estimate the average length of the luminous rays which reached the eye.

Measurements were taken of the rotation of the plane of polarization of these rays while passing through the same column of carbon bisulphide, or of some other body, such as glass, with which the phenomenon was previously studied.

For carbon bisulphide, for instance, the relations of the magnetic rotations to the yellow rays D, and to rays of any wave-length whatever, were known. It sufficed, then, to know the magnetic rotation of the rays studied, compared with the rotation of the yellow light, in order to be able to deduce the approximate value of the wave-length from it. I have already had occasion to apply this method in my previous researches *, and subsequently † to verify the accuracy of the results thus obtained.

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* *Annales de Chimie et de Physique*, t. xii. 1877.

† *Comptes Rendus de l'Académie des Sciences*, t. lxxxv.

In order to measure the rotation of the plane of polarization of light, other methods might be employed, by which the wave-length of the light under measurement would be very accurately ascertained; but, as I have already remarked in the memoir above quoted, these methods do not admit of the same sensibility as the shaded polariscope when the rotations to be measured are very small. In the present researches I propose to determine, at the same time, the magnetic rotations of various gases with luminous rays having the same wave-length, and to compare amongst themselves the magnetic rotatory powers of the same body with luminous rays having different wave-lengths. It has just been shown that the two studies should be pursued side by side.

I made use of the light of incandescent lime seen either directly or through coloured screens, which were:—

1. Rather dark-red glasses, made with cuprous oxide.
2. Light-yellow glasses.
3. A bluish green glass, which intercepted nearly all the red rays, and chiefly allowed rays to pass bordering on group E in the solar spectrum.
4. A liquid screen containing ammoniacal copper nitrate, which gave passage to a pencil of luminous rays, the most intense part of which had an average wave-length included between the lines E and F in the solar spectrum.

These various luminous sources, seen through a column of 0·50 metre of carbon bisulphide, gave the following results:—

The rotation obtained under the same magnetic conditions for the yellow rays D was taken as unity. The divergencies observable in the results relating to the same coloured light arise from the differences in temperature of the source during the various series, and show the importance attaching to the variations in brilliancy of the incandescent lime.

These results correspond to the light received after a single passage of the luminous rays through the apparatus; as is seen, they are rather variable with the luminous intensity. The first series was obtained with a particularly feeble intensity; while the other results, which are more concordant, correspond to very nearly the maximum temperature that I was able to impart to the cylinder of lime, a condition that I always sought to realize in the experiments. In order to fix the wave-length of each of the reflected images more precisely, I operated in the following manner:—The large tube was withdrawn from the interior of the coils, and the glasses unscrewed and placed inside the first coil, into which I caused the full current from the pile to pass, by detaching the other coils from the circuit. By this means the magnetic intensity became very considerable. I then arranged the optical system in the same way as for my experiments with the gases. The luminous rays passed through the glasses several times; and their plane of polarization experienced unequal magnetic rotations, which were measured for each image.

Magnetic Rotations of Carbon Bisulphide, with different luminous Sources.

	series.	Magnetic rotations compared with that of yellow light D.	Average.	Average length of wave deduced from the rotations.
Red glass, No. 1	2nd	0·812	0·808	645·0
	3rd	0·803		
	4th	0·809		
Red glass, No. 2	2nd	0·838	0·836	634·5
	3rd	0·824		
	4th	0·847		
Yellow rays D	...	1·000	......	589·2
Yellow glass	1st	0·968	1·028	582·0
	2nd	1·080		
	3rd	1·028		
White light, incandescent lime	1st	1·066	......	571·5
Green glass	1st	1·332	1·274	528·5
	2nd	1·232		
	3rd	1·245		
	4th	1·287		
Blue screen	2nd	1·458	1·480	500·0
	3rd	1·498		
	4th	1·482		

The following Table contains the results obtained. In order to facilitate the comparisons, the numbers observed were reduced to what they would be with the intensity of the electric current which I employed as unity throughout the work; but as they arise from more considerable rotations, their precision is greater. The results are given to the hundredths of a minute; at best the tenths of a minute can be relied on. In this Table I have been obliged to make some corrections, which will be justified later—in particular the action of magnetism on the column of air subjected to the action of the electromagnetic coil, and the influence exerted by the latter on the glass of the nearest mirror.

Finally, as the law of dispersion of the planes of polarization of the various coloured rays is ascertained for the crown glass, the approximate value of the average wavelength of the luminous rays which reach the eye for each image can be reduced from the observed rotations.

Magnetic Rotations obtained with the Glasses from the Tube placed in the first Coil.

		RED.			D.	YELLOW.		WHITE (incandescent lime).				GREEN.			BLUE.
		3rd image.	2nd image.	1st image.	Direct image.	3rd image.	2nd image.	4th image.	3rd image.	2nd image.	1st image.	3rd image.	2nd image.	1st image.	1st image.
Rotations observed		44′·98	29′·62	19′·29	7′·40	53′·60	39′·84	65′·41	53′·20	38′·50	23′·23	67′·99	45′·20	28′·32	33′·24
Correction	of the mirror	0·12	0·12	0·12	0·14	0·15	0·15	0·15	0·15	0·15	0·15	0·18	0·18	0·18	0·21
	of the air	**0·74**	**0·55**	**0·33**	**0·11**	**0·84**	**0·60**	**1·08**	**0·84**	**0·60**	**0·36**	**1·06**	**0·75**	**0·45**	**0·53**
	Total	0·86	0·67	0·45	0·25	0·99	0·75	1·23	0·99	0·75	0·51	1·24	0·93	0·63	0·74
Corrected rotations		44·12	28·95	18·84	7·15	52·61	39·09	64·18	52·21	37·75	22·72	66·75	44·27	27·69	32·50
Rotations for a single passage through the tube		6·30	5·79	6·28	7·15	7·52	7·82	7·13	7·46	7·55	7·57	9·53	8·86	9·23	10·83
Ratios		0·881	0·809	0·877	1·000	1·052	1·093	0·99	1·043	1·055	1·057	1·332	1·239	1·290	1·514
Approximate wave-lengths in millionths of millimetres		625	650	626	589·2	575	564	590	578	574	573	514	532	521	480

From this it is seen that the wave-lengths deduced from these observations are obviously the same as those with the carbon bisulphide. The composition of the light from the different images reflected varies little when coloured screens are used; but with the light seen directly without screens the influence of coloration from the successive images is very manifest. Thus we find:—

For the direct image	$\lambda=0{\cdot}000571$
„ first image reflected . . .	0·000573
„ second „ „ . . .	0·000574
„ third „ „ . . .	0·000578
„ fourth „ „ . . .	0·000590

When the number of reflections increases, the refrangibility steadily decreases.

III. *Magnetic Rotations caused by the Glasses interposed in the Path of the Luminous Rays.*—I have already mentioned that, in consequence of the exigencies of construction, the luminous rays had to pass through rather considerable masses of glass, subjected to an extremely weak electro-magnetic action. Nevertheless the rotatory power of solids is so great, compared with that of gases, that the rotations produced by these masses of glass are of the same order of magnitude as the phenomena to be measured.

A very simple means of determining all the corrections at the same time was to create a vacuum in the tube, and to measure the rotation obtained, under these conditions, for each source of light, then to fill the tube with gas, and make the experiments.

At first I tried to work in this way; but the process presented numerous difficulties—amongst others, exposing the glasses to the risk of fracture. When the tube is empty of air, the glasses support a pressure of about 50 kilog.; now it is known that under the influence of even a feeble pressure, the rotatory power of bodies diminishes very considerably. The real degree of influence exercised by the glasses on the rotations when the tube is full of gas is therefore not ascertained under these conditions; and the differences are not unimportant. Besides, under the strain which they have to support, the glasses present phenomena of elliptical polarization which completely alter the measurements. In order to avoid these various inconveniences, I adopted the plan of always operating at pressures very nearly equal to atmospheric pressure. Operating by displacement, the tube was filled with the different gases to be studied, several hundreds of litres of gas being passed into it. It is thus possible to obtain in the tube gases containing only a few hundredths of impurities, of which, however, an account is kept. Under these conditions it is certain that the glasses do not occasion any sensible phenomena of elliptical polarization which can confuse the measurements. For instance, I placed in the immediate vicinity of the polariscope a small tube of sugar and water, giving a rotation of 55′ for yellow light; and on measuring this rotation by means of the fourth reflected image, it was found to have exactly the same rotation, 55′. The rays had then traversed forty surfaces of glass.

It must, however, be remarked that when the tube is very hot the circumference of the glasses becomes unequally heated, and they present slight and momentary indications of strain. The glasses can be set so as to counteract the effects produced; but it is better to conduct the experiments in such a way as to avoid these perturbations, and to reject the observations which there is reason to believe are affected by this cause of error. In order to ascertain the influence of the glasses, the magnetic rotation through the tube full of air was measured; then the glasses were unscrewed, and the new rotations obtained measured; the difference between the two gives the action of the glasses. But this difference itself depends upon very small numbers; and it becomes of consequence to try, by special measurements, the value of this correction, since it affects all the other measurements.

The magnetic intensity at the position of the glasses was ascertained either directly or by means of the curve spoken of at page 323. This curve gives the ratio of the magnetic intensities inside the first coil, and at any distance from it. For the glasses whose centre is at about 0·14 metre from the exterior surface of the coils, the ratio of the magnetic intensity experienced has been found to equal 0·067 of that inside the coil.

A heavy piece of glass placed inside the first coil gave for the yellow light a rotation of 106′·10; and at the place of the glasses in the tube it gave 7′·1. The ratio of the two rotations is also 0·067. I adopted this number, and thus, knowing the magnetic rotations of the glasses inside the first coil (p. 399), was able to deduce from them the corrections sought for with the different colours and different images.

To this correction a second but less important one must be added. It has been seen that both before and after the successive reflections, the luminous rays pass through the mirrors themselves. These are made of St.-Gobain glass, and are 7·9 millim. thick. The magnetic intensity to which they are subjected has been found to equal 0·026 of that existing inside the first coil; and taking into account the relative thickness of the glasses and mirrors, it is clear that, in order to ascertain the magnetic rotation to which they give rise, the number obtained for the glasses inside the tube must be multiplied by 0·040.

In this way the following Table, which contains the necessary corrections to be made to the direct measurements, has been drawn up :—

Corrections for the Glasses and the Glass of the Mirrors.

Red.

	4th image.	3rd image.	2nd image.	1st image.
Glasses	3'·49	2'·94	2'·10	1'·26
Mirrors	0·25	0·25	0·25	0·25
	3·74	3·19	2·35	1·51

White (incandescent lime) ; yellow.

	4th image (D).	3rd image.	2nd image	1st image.
Glasses	4'·30	3'·50	2'·53	1'·52
Mirrors	0·28	0·29	0·30	0·30
	4·58	3·79	2·83	1·82

Green.

	4th image.	3rd image.	2nd image.	1st image.
Glasses	5'·75	4'·47	3'·10	1·85
Mirrors	0·38	0·38	0·37	0·37
	6·13	4·85	3·47	2·22

Blue.

	4th image.	3rd image.	2nd image.	1st image.
Glasses	......	......	......	2·16
Mirrors	......	......	......	0·42
	......	......	......	2·58

These numbers are obtained from a large number of observations ; although the tenths of a minute may be relied on, the hundredths are given as they resulted from the calculations. In concluding the examination of this question, I may remark that, the rotations to be measured in the gases being very small, all secondary considerations had to be sacrificed to perfection in the arrangement of the optical system, without which perfection the measurements would have been impossible.

IV. *Correction for the Temperature.*—I stated above that, in consequence of the passage of the electric current through the coils, the temperature of the tube rose rapidly to 30° or 40° C. It was therefore very important to ascertain the average temperature of the gases in each series of experiments.

To do this I measured the temperature of the copper by means of contact-thermometers. Moreover the tube itself acted as a gas-thermometer; it was in constant connexion with a mercurial manometer which, when the atmospheric pressure was ascertained, gave at every moment the pressure of the gas inside the tube. The temperature deduced from observations of the pressure was always the same as that given by the thermometers. The comparison of these temperatures enabled us to discover when there were leakages of gas in the tube.

In order to admit of comparison, the results observed had to be reduced to the same pressure of 760 millim. of mercury and to the same density. The correction relative to the pressure and to the density might amount to 0·10 of the value of the numbers found experimentally.

I have assumed that the magnetic rotatory power of the gases studied between 0° and 40° varied proportionately to the density, or in the inverse ratio of the modulus of dilatation $(1+at)$. The probable existence of a slight variation with the temperature in the molecular rotatory power of the gas has been thus neglected; but it may be remarked that if this variation is of the same order of dimension for gases as for other substances it must escape our measurements; for it is of the order of the errors of observation. The agreement of the results obtained at different temperatures seems to justify this hypothesis. Concerning the numbers given below, it may be stated that they were obtained at a temperature of between 10° and 40°, and that the different gases were reduced to a uniform pressure and volume by multiplying the numbers obtained by the factor $\frac{760}{\mathrm{H}}(1+at)$, H being the pressure of the gases, t their temperature, and a their coefficient of dilatation.

V. *Displacement of the Luminous Point.*—The luminous point does not invariably occupy a fixed position. Although the blowpipe is fixed to the polariscope, it happens that the luminous point rises or falls on the piece of lime according to the greater or less pressure of the oxygen which feeds the combustion. The lime itself becomes slightly hollow; and on this account the incandescent region is displaced.

These slight displacements of the luminous point cause corresponding variations in the plane of polarization of the different images; and if a sudden displacement occurred during the progress of a measurement, the latter would be erroneous. In order to guard against these variations, a large number of consecutive measurements were made by alternating the direction of the electric current in the coils.

During the greater part of the time it was easy to see, by the constancy of the direction of the planes of polarization reflected in one direction or the other, that the luminous point was fixed. From time to time a gradual displacement of the planes of polarization was apparent; but as the deviations in one direction or the other were measured alternately, this cause of error could be eliminated by taking the average of the deviations in one direction and the average of the deviations in the other. All the numbers obtained were examined with great care from this point of view, and the averages calculated in two ways:—the first, by taking the averages of the double rotations directly observed; the second, by deducing the rotation from the difference between the average positions of the planes of polarization deflected in one direction or the other.

When the two calculations did not give exactly the same result, the average of the two numbers was taken. The difference, however, did not generally amount to more than a few tenths of a minute of arc.

Finally, I may add that all the phenomena which might be due to dilatation, even of the copper tube, must be considered as introducing no appreciable perturbation into the measurements.

Accuracy of the Measurements.

The magnetic rotations obtained for the gases are sometimes so feeble that it is very important to take the most exact account possible of the degree of accuracy of the observations. As will be seen, this determination presents great difficulties; and I shall confine myself to indicating the superior limits of the errors to be feared.

The causes which may vitiate the measurements are the following:—

1st. The greater or less precision in the determination of the position of the planes of polarization, in consequence of the imperfection with which the eye appreciates the equality of tint in the two halves of the image from the polariscope.

2nd. The precision with which the electromagnetic intensity is measured.

3rd. The uncertainty as to the true wave-length of the light studied.

4th. The greater or less accuracy in the measurement of the temperature and pressure of the gases.

5th. The accidental displacement of the luminous point.

The influence of the last four causes of error is very slight in comparison with the first. In determining the maximum value of the latter, let us consider the maximum value to which the errors in our measurements might attain. Let R be the magnetic rotation for a given gas, submitted to the action of an electric current of the intensity I, and A the wave-length of the luminous rays to which the observations are being directed, let ρ be the rotation which is to be measured, i and λ the wave-length of the light studied. It will be shown afterwards that we can express

$$\frac{\mathrm{R}}{\mathrm{A}^2}=\frac{\rho}{\lambda^2}\,\frac{\mathrm{I}}{i}\,\frac{760}{\mathrm{H}}\,(1+at).$$

By taking the logarithms of the two numbers and differentiating, we have

$$\frac{d\mathrm{R}}{\mathrm{R}}=\frac{d\rho}{\rho}-\frac{2d\lambda}{\lambda}-\frac{di}{i}-\frac{d\mathrm{H}}{\mathrm{H}}+\frac{adt}{1+at}.$$

Each of the terms of the second member represents the relative error due to the variation of each element, and may be positive or negative. It is clear that the most unfavourable case for the measurements will be that where the terms have the same sign and are additive.

Now it has been seen above (p. 395) that $\frac{di}{i}$ cannot exceed 0·001.

The possible variation of λ may be deduced from experiments made with carbon bisulphide (p. 398). It is easily seen that the variations in brilliancy of the incandescent lime through the same coloured screen do not give for the expression $\frac{2d\lambda}{\lambda}$ greater values than 0·05; $\frac{d\mathrm{H}}{\mathrm{H}}$ does not amount to 0·002; and an error of 1° must be made in the temperature of the gases in order that the expression $\frac{adt}{1+at}$ shall be 0·003.

There remains, then, the error $\frac{d\rho}{\rho}$. With a *shaded* apparatus the position of equality of tint is easily determined to nearly 1′. As the rotations sought are deduced from the difference between the two measurements, the maximum error cannot exceed 2′.

Let us admit this limited value $d\rho=2'$, and let us calculate the relative error for two gases—air, which produces the smallest rotations, and olefiant gas, which produces the largest:

$$\text{For air} \ldots \quad \rho = 6'{\cdot}4 \quad \frac{d\rho}{\rho} = 0{\cdot}3, \quad \frac{dR}{R} = 0{\cdot}35,$$

$$\text{For olefiant gas} \quad \rho = 40'{\cdot}0 \quad \frac{d\rho}{\rho} = 0{\cdot}05, \quad \frac{dR}{R} = 0{\cdot}10.$$

Strictly speaking, then, in a single determination it would be possible to commit an error which would vary between the third and the tenth of the rotation, according to magnitude of the rotation.

These numbers are far from giving the precision which may be attained by increasing the observations. For example, we made on the two gases that we have just cited about a hundred and fifty measurements, which mutually tested each other, and enabled us to see which observations were too inexact and ought to be rejected.

Not being able to determine separately the value of the most important causes of error, we attempted to make the precision of each series evident by means of the greater or less agreement in the measurements. Suppose that the average of the numbers for a series of observations is the true value of the rotation sought; the individual measurements can then be compared with this average, and the positive or negative deviations calculated in each case. Giving the same sign to all of them, and taking the average, a number will result that we will call the "average deviation," and which will give an idea of the precision of each of the measurements in a series. In this calculation all the observations were taken into account, even those which presented exceptionally large deviations due to accidental causes. In the following Tables we have put in opposite columns the numbers adopted, and the value of the average deviation calculated in this way for each series; but it must be observed that this deviation is not the error which is probably to be feared. As might be anticipated, it is much smaller, as, indeed, the agreement of the results deduced from different series demonstrates. The average deviations are only superior limits of the errors.

If the only cause of error were that which relates to the optical measurements, the different series ought to give the rotations sought with a precision proportionately greater as the image reflected is of a higher order; for the same error would enter into a multiple of unknown quantity, and the application of the method of least squares would be clearly indicated; but certain perturbations, such as a variation in the wave-length of the light studied, enter into the value of even the unknown rotation. It was therefore necessary to compare each of the rotations with the average wave-length which corresponded to it.

It has been stated that the results obtained with different-coloured rays are very nearly in the inverse ratio of the square of the wave-lengths; and the departures from this simple law are of the order of errors of observation. This observation, then, enabled us to employ, for the determination of the magnetic rotation corresponding to luminous rays of a given wave-length, for example the yellow rays D, all the observations relative to the same gas, which in the case of air and olefiant gas amounted to a hundred and fifty.

Now the average of a hundred observations made with the greatest care possible may be considered as ten times more exact than a single isolated measurement; it is clear, then, that for the numbers in question, an accuracy of $\frac{1}{10}$ of the minute of arc may be relied on.

It will be shown (p. 428) that the numbers deduced, as has just been said, from observations relative to various colours do not differ amongst themselves by more than $\frac{1}{70}$ for air and $\frac{1}{360}$ for olefiant gas.

In the Tables that follow, the numbers are expressed in minutes of arc; and although the first decimal figure cannot always be relied on, I have given the results to two decimal figures, as they resulted from the calculation of the averages.

Experimental Determinations.

It has just been shown what was the relative importance of the different corrections which affect the direct observations. Among them there is one that applies to all the other corrections; this is the variation in the electromagnetic intensity.

It was ascertained that this varied only within restricted limits; and in order to render the numbers comparable, we made this correction first of all. The tables which follow contain only numbers thus corrected. The values of the other corrections are given in the tables themselves, in order that their relative importance may be justly estimated; for they allow of the same causes of error as the observations themselves. We now proceed to examine successively the results obtained with different gases.

Atmospheric Air.—The observations relative to atmospheric air were numerous, first, because the magnetic rotations of this body constituted an important physical datum, and, secondly, because it was possible to effect the determinations without interposing the tube-glasses in the path of the light, and thus to verify the numbers adopted for the magnetic rotations due to the passage of the luminous rays through these glasses. Besides the determinations which follow, a great number of others were made before the apparatus had received its definite and final form. However, the average of the results found previously is identical with that which is deduced from the following numbers:—

Atmospheric Air.

Series.	Average temperature, *t*.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
WHITE LIGHT (incandescent lime).—4th image (9 passages).						
1st. Measurements without the tube-glasses. (Former arrangement of the apparatus.)						
May 13, 1879.	30°·7	millim. 764·0	1·100	6'·17	6'·78	1'·1
Final arrangement.						
Jan. 16, 1880.	10·0	763·2	1·033	6·56 Mirrors 0·28 Diff. 6·28	6·48	1·8
March 19, 1880.	16·0	768·0	1·048	6·44 Mirrors 0·28 Diff. 6·16	6·45	1·0
2nd. Measurements with the tube-glasses.						
Jan. 19, 1880.	16·5	762·8	1·056	10·74 Corr. 4·58 Diff. 6·16	6·50	1·0
March 11, 1880.	14·0	801·3	1·032	10·92 Corr. 4·58 Diff. 6·34	6·54	1·6
3rd image (7 passages).						
March 6, 1880.	21·0	791·8	1·033	8·77 Corr. 3·79 Diff. 4·98	5·14	1·1
YELLOW LIGHT (yellow glass).—4th image (9 passages).						
1st. Measurements without the glasses.						
March 19, 1880.	20·7	767·9	1·064	6·15 Mirrors 0·28 Diff. 5·87	6·18	0·8
„ (2nd series).	35·0	766·3	1·119	5·05 Mirrors 0·28 Diff. 4·77	5·34	0·4

Table (*continued*).

Series.	Average temperature, t.	Average pressure, H.	Correction, $(1+at)\frac{760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
2nd. Measurements with the glasses.						
March 11, 1880.	33°·0	millim. 820·3	1·038	10′·95 Corr. 4·58 Diff. 6·37	6′·61	1′·2
Red light (red glass No. 2).—4th image (9 passages). Measurements without the glasses.						
March 19, 1880.	30·6	766·5	1·102	5·88 Corr. 0·25 Diff. 5·63	6·19	0·9
3rd image (7 passages). Measurements without the glasses.						
March 19, 1880.	27·2	766·6	1·077	4·23 Mirrors 0·25 Diff. 3·98	4·28	0·5
Measurements with the glasses.						
March 11, 1880.	33·0	821·4	1·037	7·50 Corr. 3·19 Diff. 4·31	4·47	1·0
Green light.—4th image (9 passages). Measurements without the glasses.						
March 19, 1880.	21·4	767·05	1·071	7·54 Mirrors 0·38 Diff. 7·16	7·67	0·9
„ „	38·0	766·2	1·130	6·66 Mirrors 0·38 Diff. 6·28	7·10	0·9
3rd image (7 passages). Measurement without the glasses.						
March 19, 1880.	26·2	766·9	1·086	6·38 Mirrors 0·38 Diff. 6·00	6·51	1·0
Measurement with the glasses.						
March 6, 1880.	33·6	821·0	1·038	11·39 Corr. 4·85 Diff. 6·54	6·78	0·8
March 11, 1880.	35·0	822·1	1·040	11·33 Corr. 4·85 Diff. 6·48	6·73	1·9

Table (*continued*).

Series.	Average temperature, t.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
2nd image (5 passages). Measurement with the glasses.						
March 6, 1880.	33°·6	millim. 821·0	1·038	7′·20 Corr. 3·47 Diff. 3·73	3′·97	0′·6
Blue light.—1st image (3 passages). Measurement without the glasses.						
March 19, 1880.	36·9	766·1	1·126	3·10 Mirrors 0·43 Diff. 2·67	3·00	0·3

Oxygen.—The oxygen was prepared by mixing chlorate of potash with a little binoxide of manganese. The greatest care was taken to purify the gas from the chlorine always contained in it: this was done by passing it several times very slowly through a solution of potash, and then over pumice moistened with a concentrated solution of potash. The gas was dried by passing over fragments of pumice moistened with sulphuric acid and then over calcium chloride.

In this way several hundreds of litres of gas were prepared in bags which had been used for some time; and the oxygen was slowly introduced into the tube by causing it to pass once more through the series of purifying and drying tubes. The oxygen displaced the air, so that eventually the tube was filled with a mixture containing only a few hundredths of impurities. Several methods were employed for analyzing the oxygen, hydrochloric acid and copper being principally used. In this way we obtained for the mixture studied as below:—

On the 27th of February, 1880, out of 134 vol. of gas, 3·5 vol. of residuum, which we may admit to be nitrogen (say 0·9739 of oxygen and 0·0261 of nitrogen). After having made the determinations with this gas, a fresh analysis, made on the 3rd of March, yielded out of 121 vol., a residue of 3·4 vol. (say 0·972 of oxygen and 0·028 of nitrogen).

It is to be noted that, between the two series of experiments, a fresh quantity of gas was passed into the tube. It may be admitted that we had an average of 0·973 of oxygen and 0·027 of nitrogen. The following numbers were obtained:—

Oxygen, 0·9730 + 0·027 Nitrogen.

Series.	Average temperature, t.	Average pressure, H.	Correction, $\frac{(1+at)760}{\text{H}}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
White light.—3rd image (7 passages).						
		millim.				
Feb. 28, 1880.	12°·0	797·5	0·995	8′·55 Corr. 3·79 Diff. 4·76	4′·73	2′·9
March 3, 1880.	24·5	810·3	1·022	8·12 Corr. 3·79 Diff. 4·33	4·42	2·9
Yellow light.—3rd image.						
Feb. 28, 1880.	24·0	831·0	0·996	8·58 Corr. 3·79 Diff. 4·79	4·77	2·3
Red light.—3rd image.						
March 3, 1880.	19·2	796·9	1·022	8·69 Corr. 3·19 Diff. 5·50	5·62	2·3
,, ,,	22·8	807·5	1·023	8·14 Corr. 3·19 Diff. 4·93	5·04	2·7
,, ,,	29·7	819·0	1·029	8·39 Corr. 3·19 Diff. 5·20	5·34	2·3
Green light.—3rd image.						
Feb. 28, 1880.	25·3	834·0	0·995	9·65 Corr. 4·85 Diff. 4·80	4·77	2·5
March 3, 1880.	33·0	830·7	1·025	10·63 Corr. 4·85 Diff. 5·78	5·92	3·2
,, ,,	24·7	807·8	1·026	8·83 Corr. 4·85 Diff. 3·98	4·08	1·2
,, ,,	34·8	833·6	1·028	10·05 Corr. 4·85 5·20	5·34	1·2

The average of all the observations made March 3rd on the third image, by alternating the measurements relative to the red rays and to the green rays, produced:—

	Third image.	
	Red.	Green.
Rotation	8·55	9·61
Correction	3·19	4·85
Difference ...	5·34	4·76

These numbers will be discussed later. From the beginning of the experiments the series relative to the oxygen presented anomalies upon which I shall dwell presently. I also made a further series of observations with this gas, some at the beginning of the experiments in 1879, others at the end of January 1880, and, finally, the determinations which I have just recorded.

The latter correspond to a purer gaseous mixture than the previous series. I judged it useless to reproduce the numbers relative to the former series; they, however, completely confirmed those just given.

Nitrogen.—The nitrogen was prepared by passing air very slowly through two tubes, 0·50 metre long, filled with copper turnings, heated to a red heat in porcelain tubes. The gas was received in a large gasometer, and afterwards made to pass, after drying, into the tube previously full of air.

The gaseous mixture was analyzed by means of cold phosphorus; and out of 150 vol. of gas a residuum of 136·5 was produced. The following are the results obtained:—

Nitrogen, 0·91 + 0·090 Oxygen.

Series.	Average temperature, t.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
WHITE LIGHT.—4th image (9 passages).						
Jan. 16, 1880.	20°·0	millim. 762·6	1·069	11′·00 Corr. 4·58 Diff. 6·42	6′·86	2′·6

The numbers obtained for air and oxygen ought to enable us to find the number relative to the nitrogen. By correcting the number above obtained by the 0·09 vol. of oxygen which it contains, we obtain for the pure nitrogen the number 6·95.

Let us now calculate the rotation of the nitrogen by means of those of the air and oxygen. Let us take, for example, the numbers relating to the third white image. We have:—

	Air, 0·208 O + 0·792 N.........	5′·14
	0·208 oxygen..............	0·95
whence		
	0·792 N.....................	4·19
and	N (3rd image)	5·29
	N (4th image)	6·80

This number agrees sufficiently well with the direct determinations; for we cannot count on a greater precision.

Nitrogen Monoxide.—Nitrogen monoxide was prepared by the decomposition of ammonium nitrate, and collected over water saturated with this gas. It was subsequently passed into the tube, which was then full of a mixture of 0·70 of oxygen and 0·30 of nitrogen.

The gas was analyzed by absorbing it by water; in this analysis account must be taken of the air dissolved in the water, and which is evolved on the solution of the gas which is more soluble. In this way we obtained for the mixture:—

Nitrogen monoxide	0·691
Nitrogen	0·219
Oxygen...........................	0·090

Cold phosphorus was employed for the analysis of the mixture not absorbed by the water.

The gas, as we see, was very impure; and this disadvantage must be attributed to the air dissolved in the water of the gasometer, and which was evolved, either by a partial vacuum being produced in the upper part of the gasometer, or by the solution of nitrogen monoxide in the water.

The following numbers were obtained for the preceding mixture:—

Series.	Average temperature, t.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
White light.—4th image.						
Jan. 30, 1880.	23°·0	millim. 772·0	1·067	17′·10 Corr. 4·58 Diff. 12·52	13′·36	1′·7
Green light.—4th image.						
Jan. 30, 1880.	26·9	774·0	1·067	21·12 Corr. 6·13 Diff. 14·99	15·99	4·2

The rotations which would be given by protoxide in a pure state can be deduced from the preceding numbers; in fact, we have for the fourth image (white light):—

$$\begin{array}{lll} & \text{Mixture}\ \ldots\ldots\ldots\ldots\ldots\ldots & 13'{\cdot}36 \\ & \left.\begin{array}{ll} 0{\cdot}219\ \text{N} & 1'{\cdot}51 \\ 0{\cdot}09\ \ \text{O} & 0'{\cdot}53 \end{array}\right\}\ldots\ldots & 2{\cdot}04 \\ \cline{3-3} \text{whence} & 0{\cdot}691\ N_2O\ \ldots\ldots\ldots\ldots\ldots & 11{\cdot}32 \end{array}$$

From this is deduced:—

$$N_2O\ \text{(4th white image)}\ldots\ 16'{\cdot}38$$

The same calculation can be made for the green light; indeed we have for the 4th image (green light):—

$$\begin{array}{lll} & \text{Mixture}\ \ldots\ldots\ldots\ldots\ldots\ldots & 15'{\cdot}99 \\ & \left.\begin{array}{ll} 0{\cdot}219\ \text{N} & 1'{\cdot}88 \\ 0{\cdot}99\ \ \text{O} & 0'{\cdot}53 \end{array}\right\}\ldots\ldots & 2{\cdot}41 \\ \cline{3-3} & 0{\cdot}691\ N_2O\ \ldots\ldots\ldots\ldots\ldots & 13{\cdot}58 \\ \text{whence} & N_2O\ \text{(4th green image)} & 19{\cdot}61 \end{array}$$

Carbonic Acid.—The carbon dioxide was prepared by the action of hydrochloric acid on white marble. A current of this gas, previously dried, was passed for a very long time into the tube, which contained at the time the mixture of nitrogen monoxide, oxygen, and nitrogen above mentioned.

This gas was analyzed by absorbing it by water; and out of 154 vol. of gas I found a residuum of 3 vol. (say 0·0195 of impurities and 0·9805 of carbonic acid). This gas may be considered pure; and the numbers obtained can only admit an inappreciable correction; for it happens that the carbonic acid has very nearly the same magnetic rotatory power as the mixture which constitutes the 0·019 of impurities. The results obtained are given in the following table:—

Series.	Average temperature, t.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
			WHITE LIGHT.—4th image.			
Feb. 6, 1880.	17°·5	millim. 808·4	1·000	16'·90 Corr. 4·58 Diff. 12·32	12'·32	2'·7
			GREEN LIGHT.—4th image.			
Feb. 6, 1880.	20·0	815·8	1·000	21·50 Corr. 6·13 Diff. 15·37	15·37	2·6

Sulphurous Acid.—This gas was prepared by the volatilization of liquefied anhydrous sulphurous acid, for which I am indebted to the kindness of M. Raoul Pictet. The gas thus obtained was perfectly dry. It was introduced into the tube which contained dry air.

In order to analyze this gas, it was, on issuing from the tube, received in small glass tubes which were hermetically closed; subsequently one of the ends of these tubes was broken over boiled water containing potash, and the gas was analyzed by absorption. By exactly gauging the volume of the small tube as well as the volume of the gaseous residuum not absorbed by the potash, it was ascertained that the mixture enclosed in the tube contained 0·984 of sulphurous acid and 0·016 of air. With this gas the following numbers were obtained:—

Sulphurous acid, 0·984 SO^2+0·016 Air.

Series.	Average temperature, *t*.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
			White light.—4th image (9 passages).			
July 24, 1880.	25°·0	millim. 819·6	1·012	32'·12 Corr. 4·58 Diff. 27·54	27'·87	2'·3
" "	37·5	854·3	1·012	34·80 Corr. 4·58 Diff. 30·22	30·58	2·4
			3rd image.			
" "	35·0	847·2	1·012	23·98 Corr. 3·79 Diff. 20·19	20·43	2·3
			Red light.—3rd image.			
" "	39·0	858·15	1·012	22·14 Corr. 3·19 Diff. 18·95	19·17	2·3
			2nd image.			
" "	44·8	875·0	1·012	16·23 Corr. 2·35 Diff. 13·88	14·04	1·9

Table (*continued*).

Series.	Average temperature, t.	Average pressure. H.	Correction, $\frac{(1+at)760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
			GREEN LIGHT.—4th image.			
July 24, 1880.	34°·8	millim. 845·9	1·012	42′·41 Corr. 6·13 Diff. 36·28	36′·71	2′·1
			3rd image.			
„ „	35·0	847·2	1·012	34·07 Corr. 4·85 Diff. 29·22	29·51	4·0
			BLUE LIGHT.—1st image.			
„ „	41·7	864·3	1·012	17·75 Corr. 2·58 Diff. 15·17	15·34	1·6

By taking into account the 0·016 of air in the gaseous mixture, we may calculate by means of the preceding numbers the rotations that we should have with the gas absolutely pure. We should find thus:—

4th white image	29′·90
4th green image	37·19

Olefiant Gas.—A preliminary series of experiments performed upon a mixture of 0·72 of olefiant gas and 0·27 of carbonic acid, proved that this body was endowed with a relatively great magnetic rotatory power; and we took extreme precautions to obtain in the tube gas in its purest possible condition. More than 300 litres of olefiant gas were passed into the tube. This gas was prepared by the action of sulphuric acid on alcohol, purified from ether by passing through sulphuric acid, and from the sulphurous acid (which was produced in rather large quantities) by passing through potash. By remaining in the gasometer the gas became completely freed from the small quantity of sulphurous acid which might remain; but it was always mixed with a little air.

The mixture was analyzed by passing a known volume of the gas into a small graduated tube and then introducing chlorine, bubble by bubble, so as to form ethylene chloride; the excess of the chlorine was subsequently absorbed by potash.

In this analysis account must be taken of a small quantity of air that may be introduced by the chlorine, as well as of the air which is evolved from the liquid by absorbing the chlorine through the potash.

From careful experiments I found, before commencing the first series recorded below, that the gas contained 0·044 of air. In the interval between the first and second series, the tube being very hot, a little gas was lost by the closing of the glasses. At the end of the second series two closely agreeing analyses gave 0·061 of impurities. It may be assumed that we had on an average a mixture containing 0·95 of olefiant gas and 0·05 of air.

A great number of measurements for the different images and colours were made with this body, because the rotations, relatively great, were to serve as verification of our experimental method.

A direct determination made with the yellow light of soda gave, for the first image, a number very close to those which would be deduced from the rotation obtained with the fourth white image. There is no use in giving this number, because, on account of the feeble intensity of the light, it was not possible to ensure the same precision as in the case of the other measurements.

We subjoin the results of our measurements with olefiant gas.

Olefiant Gas, 0·95 C_2H_4 + 0·05 Air.

Series.	Average temperature, *t*.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
			WHITE LIGHT.—4th image (9 passages).			
Feb. 17, 1880.	19°·3	millim. 804·5	1·012	35′·23 Corr. 4·58 Diff. 30·65	31′·01	5′·0
Feb. 20, 1880.	23·0	776·2	1·061	34·36 Corr. 4·58 Diff. 29·78	31·56	5·2
			3rd image (7 passages).			
" "	32·0	797·9	1·064	27·86 Corr. 3·79 Diff. 24·07	25·61	2·3

Table (*continued*).

Series.	Average temperature, t.	Average pressure, H.	Correction, $(1+at)\frac{760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
2nd image (7 passages).						
Feb. 20, 1880.	32°·0	millim. 797·9	1·064	20′·49 Corr. 2·86 Diff. 17·63	18′·82	0′·9
1st image (3 passages).						
" "	35·6	805·7	1·065	12·24 Corr. 1·82 Diff. 10·42	11·10	0·9
Yellow light (yellow glass).—4th image.						
" "	29·7	783·2	1·075	35·23 Corr. 4·58 30·65	32·85	5·3
3rd image.						
" "	32·0	787·6	1·078	28·30 Corr. 3·79 Diff. 24·51	26·42	3·3
Yellow light (yellow glass).—2nd image.						
" "	34·0	787·8	1·080	18·53 Corr. 2·80 Diff. 15·73	16·99	1·7
1st image.						
" "	36·0	795·2	1·082	10·74 Corr. 1·82 Diff. 8·92	9·65	1·7
Red light (red glass No. 2).—3rd image.						
Feb. 17, 1880.	35·0	847·6	1·012	24·78 Corr. 3·19 Diff. 21·59	21·85	3·3
Feb. 20, 1880.	39·5	801·7	1·085	23·53 Corr. 3·19 Diff. 20·34	22·07	4·6
2nd image.						
" "	39·5	801·7	1·085	15·53 Corr. 2·35 Diff. 13·18	14·30	1·4

Table (*continued*).

Series.	Average temperature, t.	Average pressure, H.	Correction, $\frac{(1+at)760}{H}$.	Numbers observed and corrections.	Corrected magnetic rotations.	Mean deviations.
1st image.						
Feb. 20, 1880	39°·5	millim. 801·7	1·085	9′·88 Corr. 1·51 Diff. 8·37	9′·08	1′·8
1st image (red glass No. 1, deeper red).						
" "	39·5	801·7	1·085	9·30 Corr. 1·50 Diff. 7·80	8·46	0·9
Green light.—4th image.						
Feb. 17, 1880.	24·4	819·2	1·012	44·72 Corr. 6·13 Diff. 38·59	39·05	7·5
3rd image.						
" "	32·2	839·5	1·012	34·12 Corr. 4·85 Diff. 29·27	29·62	4·5
2nd image.						
Feb. 20, 1880.	41·4	803·7	1·089	24·61 Corr. 3·47 Diff. 21·14	23·02	2·9
1st image.						
" "	40·5	800·8	1·090	12·99 Corr. 2·22 Diff. 10·77	11·73	1·5
Blue light.—2nd image.						
Feb. 17, 1880.	40·9	849·5	1·012	25·64		5·3
" "	40·9	849·5	1·012	25·53 Correction of the glasses uncertain.		5·4
1st image.						
Feb. 20, 1880.	42·3	793·4	1·100	15·14 Corr. 2·58 Diff. 12·56	13·90	1·5

The slight correction due to the presence of 0·05 of air can be calculated with great precision. For example, we have for the fourth image—

	4th image.	
	White.	Green.
Mixture..........................	31′·28	39′·05
0·05 of air.......................	0·32	0·38
Difference = 0·95 olefiant gas	30·96	38·67
Pure olefiant gas	32·59	40·70

DISCUSSION OF THE RESULTS OBTAINED.

I. *Verifications of the Method of Observation.*

The principle of the method employed by me in this work consisted, as we have seen above, in amplifying the phenomenon by causing the luminous rays to pass several times through the tube full of gas. It is evident that the numbers obtained for each reflected image must be proportionate to the number of passages made by the luminous rays through the apparatus. These numbers should therefore mutually test each other; and by dividing them by 3, 5, 7, and 9 we ought to obtain numbers which would represent the magnetic rotation corresponding to a single passage through the tube. With the same luminous source and the same coloured screen these numbers should be constant; but we have seen above that the composition of the light which reaches the eye in order to form the different images varies with them; we ought, then, to find numbers regularly varying with the order of the images.

Strictly speaking, all the measurements ought to be effected at the distance which separates the mirrors; and when any other gas than air is in question, the column studied has not exactly the same length as the distance from the mirrors. But we have seen that, in the interval of 0·06 metre which separates the end of the tube from the mirrors, the magnetic intensity is very feeble; and the effect due to this slight difference in length must be regarded as absolutely unimportant, so that the verifying calculations can be applied even when the tube is full of any gas whatever. The slight differences between numbers which, for the same gas, correspond to a single passage of a pencil of rays of the same colour, are partly due to variations in the colour of the images, rather than to errors in the measurements.

Summary of the Results obtained.

Atmospheric Air.										
	Red.		Yellow.		White incandescent lime.		Green.		Blue.	
	Rotations.	Rotation for one passage.	Rotations.	Rotation for one passage.	Rotations.	Rotation for one passage.	Rotations.	Rotation for one passage.	Rotations.	Rotation for one passage.
4th image	6́·19	0́·68 ?	6́·39	0́·710	6́·49	0́·721	7́·67	0́·850		
3rd image	4·37	0·624	......	......	5·14	0·730	6·67	0·95		
2nd image	......	......	......	......	......	......	3·97	0·79		
1st image	......	......	......	......	......	......	......	......	3́·00	1́·00
Oxygen.										
3rd image	5·33	0·762	4·77	0·68	4·57	0·63	4·76	0·68		
Nitrogen.										
4th image	......	......	......	......	6·90	0·766				
Carbonic Acid.										
4th image	......	......	......	......	12·32	1·370	15·37	1·700		
Nitrous Oxide.										
4th image	......	......	......	......	16·38	1·82	19·61	2·18		

Sulphurous Acid.

4th image	……	……	……	……	29′·59	3′·288	37′·19	4′·132		

Mixture of 0·984 SO^2 and 0·016 Air.

4th image	……	……	……	……	29·22	3·247	36·71	4·080		
3rd image	19′·17	2′·738	……	……	20·43	2·920	29·51	4·215		
2nd image	14·04	2·808								
1st image	……	……	……	……	……	……	……	……	15·34	5·11

Olefiant Gas.

4th image	……	……	……	……	32·59	3·62	40·70	4·52		

Mixture of 0·95 $C^4 H^4$ and 0·05 Air.

4th image	……	……	32·85	3·65	31·28	3·47	39·05	4·34		
3rd image	21·95	3·13	26·42	3·77	25·61	3·66	29·62	4·23		
2nd image	14·30	2·86	16·99	3·40	18·82	3·76	23·02	4·60	13·90	4·63
1st image	9·08	3·02	9·65	3·22	11·10	3·70	11·71	3·90		
1st image, Red no. 1	8·46	2·82								

Almost the same results are obtained as with the glasses placed inside the first coil, especially with the white light. For example, if the rotations are compared with that which is deduced from the fourth image, we shall find that the rotations for a single passage of the white light are expressed by the following numbers:—

	4th image.	3rd image.	2nd image.	1st image.
Olefiant gas . .	1	1·054	1·083	1·066
St.-Gobain glass .	1	1·046	1·058	1·061

We see, then, that the divergences obtained between numbers which should be constant are partly due to the same cause —that is, to the difference in the colour of the various images.

If this is the case, the successive images with the white light ought to become more and more yellow, and the rotations should diminish from the first to the fourth. For the green light, on the contrary, the first image ought to be more yellow than the others, and the corresponding rotation more feeble. We find, in point of fact, that this deduction is confirmed by experiment. The absorption of the luminous rays by the coloured screens renders observations on the third and fourth images very difficult. It is necessary to increase the pressure of the oxygen from the blowpipe: the temperature of the line rises; but as the brilliancy increases it becomes more and more variable for very slight differences in temperature.

The want of regularity in the value of numbers deduced from the different images apprises us of these variations; nevertheless by taking the average of the numbers obtained, we see that we have results comparable with those deduced from observations made for the same colours with the carbon bisulphide.

II. *Dispersion of the Planes of Polarization of Luminous Rays having different Wave-lengths.*

The study of the phenomena of magnetic rotatory polarization in bodies comprises two parts:—

1st. The relative measurements of the rotations of the planes of luminous rays having different wave-lengths and traversing the same substance.

2nd. The comparison of the magnetic rotatory powers of various bodies for rays having the same wave-length.

I have demonstrated in the course of this memoir that the want of homogeneity in the luminous source compelled me to begin with the first of these studies—that is to say, to determine the relation of the rotations of the planes of polarization of rays of different colours.

It has been shown that the luminous rays studied had not in each case the same wave-length, but that their average wave-length could be deduced from the magnetic rotation of the carbon bisulphide.

As the phenomenon has been exhaustively studied with regard to liquid bodies, such as carbon bisulphide, we can limit ourselves to comparing the relations of the magnetic rotations for the same colours with this body and with the different gases studied. The St.-Gobain glass, which has been made the object of special determinations in this memoir, can also serve for comparison.

By taking the average of the results obtained with each of the coloured screens employed, the results indicated in the following table were obtained; the unit to which the rotations for each substance were referred was the rotation obtained with the yellow light D, or the rotation deduced from the fourth white image.

Dispersion of the Planes of Polarization of Rays of different Wave-lengths.

Bodies.	Magnetic rotations.				
	Red.	Yellow D, 4th white image.	White.	Green.	Blue.
Carbon bisulphide (liquid)	0·836	1·000	1·066	1·290	1·480
St.-Gobain glass	0·845	1·000	1·055	1·290	1·51
$\frac{1}{\lambda^2}$	0·850	1·000	1·060	1·250	1·390
Oxygen					
Atmospheric air	0·860	1·000	......	1·250	1·39
Nitrogen	......	1·000			
Carbonic acid	......	1·000	......	1·246	
Nitrous oxide	......	1·000	......	1·197	
Sulphurous acid	0·853	1·000	......	1·256	
Olefiant gas	0·849	1·000	1·066	1·235	1·33

We see, first of all, that the statements of the rotations for two self-colours are nearly the same for liquids, gases, and solids. Oxygen, to which we shall return later, must be excepted. If the numbers contained in the summary (pp. 422, 423) be compared with those which were obtained by the glasses of the tube, it will be found that the numbers relative to the glasses of the tube are very nearly ten times those obtained for the air, and that in the two tables the same figures are sometimes reproduced for the same colours. I would again refer to the comparison made between the numbers obtained with the different white images.

These considerations enable us to see that the precision of the measurements can be carried very far, since it enables the variations in coloration of the successive images to be followed with gases as well as with a mass of glass. The average wave-length of the luminous pencil can then be approximately determined in each case and compared with the rotations obtained. The most simple means consists in making a graphical representation.

If the numbers inversely proportional to the squares of the wave-lengths distinguishing each image be taken as the abscissæ, and the magnetic rotations observed for the ordinates, we see that the typical points of the determinations relative to the same substance approach very closely to a right angle passing by the origin of the coordinates, which proves that the rotations are very nearly in the inverse ratio of the square of the wave-lengths, as may be verified by reference to the preceding table.

The numbers produced by the different images with different colours may each serve for the determination of an average right angle which, within the limits of the observations, represents the phenomenon with sufficient precision.

Briefly summarizing, we see that, for five of the gases studied, the magnetic rotation of the planes of polarization of luminous rays having different wave-lengths is very nearly in the inverse ratio of the squares of the wave-lengths of these rays. The slight value of the rotations observed did not allow me to ascertain whether these gases, like solids and liquids, diverge a little from this simple law in proportion as the rays become more refrangible.

The nitrous oxide gives a rather less deviation than the other gases. We may remark that, in order to make the numbers agree with those given by the other bodies, it would suffice to multiply the results by 1·05; and as this ratio 1·05 is exactly the ratio of the rays of white light to the rays of yellow light, we may conclude that the inequality is due to an increase of brilliancy in the luminous source at the moment of the experiment with the white light. This explanation appeared to me to be admissible; but if such were not the cause of the divergence observed, we may associate this fact with that presented by oxygen, and which I will now discuss.

Oxygen showed a remarkable anomaly. Previously, while studying the magnetic rotatory powers of various bodies with the yellow light, I had discovered that the presence of oxygen in a combination tended to diminish the rotatory power of the compound. I now find that gaseous oxygen has a feeble positive magnetic rotatory power, and that, in addition, it does not disperse the planes of polarization of rays of different colours as the other gases do. The numbers which relate to this body are very small, and the precision of the observations is much less than for the other gases. It is noticeable that this want of precision was characteristic of the experiments made with oxygen—although I always took the measurements with the same care, and although I studied this gas several times at different periods during our series of experiments. If the numbers found directly for the various colours be not corrected by the rotation of the glasses, it will be seen that they are very nearly equal to each other. In order to prove this fact, I took the precaution of making a large number of series, operating alternately with red and green rays; and I always found that the rotations were essentially the same.

Now, as the magnetic rotation due to the glasses is greater for the green rays than for the red rays, we should conclude from this that the oxygen would deflect the plane of polarization of the green rays less than that of the red rays, in opposition to the other gases studied up to this time. This body presents, then, a most curious exception; and it would be very important to establish this fact irrefragably. The numbers that I obtained do not as yet allow of it; but I anticipate soon being able to complete the first experiments required to do so.

However, it is easy to see that oxygen, if it does not present an inverse magnetic rotatory dispersion, would have at least a dispersion which is very nearly *nil*. The present experiments do not allow us to decide whether the rotations slightly augment or decrease with the refrangibility.

It is not uninteresting to consider the exceptional phenomenon of the magnetic properties of oxygen.

I have shown in previous researches that very magnetic bodies endowed with a negative magnetic rotatory power disperse the planes of polarization of light according to a different law from that which governs positive rotations. The ratios of the negative rotations are obviously the squares of the ratios which correspond to the positive magnetic rotations for the same luminous rays.

Now, it is extremely interesting to remark that the exception relative to oxygen coincides with the special magnetic properties of this gas. At present we cannot demonstrate that the coincidence which the two phenomena appear to show connects them one with the other. By referring to my former researches, it will be seen that it is possible to conceive the existence of a mixture of two substances, the one magnetic and the other diamagnetic, which would give rise to the same phenomenon as oxygen. This, however, is pure hypothesis; but it is interesting to consider it, especially when it relates to a body so singular as oxygen, which, on the one hand, is endowed with exceptional magnetic properties, and, on the other, gives rise to the very peculiar modification known as ozone. However, I propose to investigate more fully the cause of the anomaly just noticed.

III. *Magnetic Rotations of Gases compared with Carbon Bisulphide for the Yellow Rays of Soda.*

The relation above established between the magnetic rotations of the planes of polarization of rays of different colours and the wave-length of the corresponding light, enables us to determine the magnetic rotation relative to the yellow light of soda which corresponds to the rays D in the solar spectrum. In order to effect this determination, use may be made of all the numbers obtained for the different colours, either by means of graphical representation, or by dividing each of them by the inverse ratio of the squares of the wave-lengths. In this way the following results have been obtained for the different gases (oxygen, which has already been considered, is excluded from the following table):—

Rotations for the Yellow Light D, deduced from Observations with Rays of different Colours.

Colour of the rays.	Numbers obtained.	Mean.	Mean deviation.
	Atmospheric Air.		
Red rays	0·73	0·720	$\frac{1}{70}$
Yellow rays	0·72		
White „	0·70		
Green „	0·72		
	Nitrogen.		
White rays	0·729		
	Carbonic Acid.		
White rays	1·37	1·365	$\frac{1}{130}$
Green „	1·36		
	Nitrous Oxide.		
White rays	1·82	1·78	$\frac{1}{40}$
Green „	1·74		
	Sulphurous Acid.		
Red rays	3·302	3·300	$\frac{1}{330}$
Yellow rays	3·288		
Green „	3·306		
	Olefiant Gas.		
Red rays	3·626	3·625	$\frac{1}{360}$
Yellow rays	3·621		
White „	3·636		
Green „	3·616		

These numbers correspond to a single passage of the luminous rays through the tube; we see with what precision they mutually test each other. By dividing these numbers by the rotation obtained for carbon bisulphide under the same conditions, and which was found to be equal to 4520′, the following table is obtained:—

Magnetic Rotations of Gases compared with liquid Carbon Bisulphide (yellow light D).

Gas.	Rotation for one passage.	Rotations compared with carbon bisulphide.
Oxygen	0·663	0·000146
Air	0·720	0·000159
Nitrogen	0·729	0·000161
Carbonic acid	1·365	0·000302
Nitrous oxide	1·780	0·000393
Sulphurous acid	3·300	0·000730
Olefiant gas	3·625	0·000802

In the course of the experiments an occasion offered of verifying the preceding numbers.

We have seen how the determination of the magnetic rotation of the glasses in the tube inside the first coil was effected. This rotation may be compared with that of the gases. Now it happens that for air the comparison is immediate; the rotation of the glasses (say 1 centim. of crown glass) is equal to ten times the rotation of 3 metres of air (say to 3000 centim. of air submitted to the same magnetic influence). The relation of the rotations is then

$$\frac{1}{3000}=0·00033.$$

Now in my former researches I found that the magnetic rotatory power of crown glass for the yellow light was 0·48 of that of carbon bisulphide. By adopting this number, which is independent of our present experiments, we should find for the rotatory power of air 0·000158*.

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* Since the time when I began these researches, MM. Kundt and Röntgen have published numbers relative to several gases. The only ones common to our researches are oxygen and air. The numbers given by these authors in their first memoir (Wiedemann's *Annalen*, t. viii. p. 278) are too large, because they have compared the rotations obtained directly with white light for gases, and with yellow light D for carbon bisulphide. For oxygen the number that I find is identical with that which they have given for high pressures. The agreement is, no doubt, owing to the fact that oxygen has no sensible rotatory dispersion, and that the errors which they committed respecting the refrangibility of light became then unnoticed. In a second memoir the same authors have

IV. *Relation between the Magnetic Rotatory Powers of Gases and their Indices of Refraction.*

I have demonstrated that a remarkable relation exists between the rotations of the planes of polarization of light traversing non-magnetic solid and liquid bodies and the indices of refraction of these bodies for the same luminous rays. It was of the greatest importance to prove whether such a relation still existed in the gaseous state; and I took the opportunity of saying at the beginning of this memoir, that I was guided by these considerations in the construction of the apparatus employed in the researches. The experiments just described have justified my expectations.

When the magnetic rotations obtained for the different gases are compared with the indices of refraction of the latter for the yellow light of soda, it is seen that the rotations increase regularly with the indices of refraction. In the table which follows, I have placed opposite to the magnetic rotations of the gases their indices of refraction:—

Magnetic Rotations of Gases compared with liquid Carbon Bisulphide.

Gas.	1. Magnetic rotatory powers, R.	2. Magnetic rotations.	3. Indices of refraction, n.	4. $(n-1)^2$.	5. $\frac{R}{n^2(n^2-1)}$.
Oxygen	0·000146	0·918	1·0002706 (Mascart)	0·850	0·269
Air	0·000159	1·000	1·0002936 (Mascart)	1·000	0·277
Nitrogen	0·000161	1·012	1·0002977 (Mascart)	1·027	0·274
Carbonic acid ...	0·000302	1·900	1·0004544 (Mascart)	2·393	0·332
Nitrous oxide ...	0·000393	2·471	1·0005159 (Mascart)	3·086	0·381
Sulphurous acid...	0·000730	4·591	1·0006650 (Dulong)	5·130	0·548
Olefiant gas	0·000802	5·044	1·0006780 (Dulong)	5·327	0·590

In order to make a comparison between the numbers in the

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given smaller numbers than those previously published; and the results at which they arrived do not agree any better with mine. The number that they attribute to air (0·000127) is identical with that which I had previously published in a work on atmospheric polarization (*Annales de Chimie et de Physique*, t. xix.). I observed at the time that this number only approximated within two or three tenths of its value; it results from the present memoir that it was too little by 0·00003.

first column and those in the third, we may construct a curve by taking the indices as the abscissæ and the rotations as the ordinates: in this way a curve of great regularity is obtained, whose form recalls that which we obtained under analogous conditions with liquid and solid bodies (fig. 5).

By seeking a simple function of the index, which varies proportionately to the rotations observed, we easily see that the ratios of the function $(n-1)^2$, n being the index of refraction, follow very nearly those of the magnetic rotations. These appear in columns 2 and 4 of the preceding table. However, it does not seem that this formula is the complete expression of the relation between the magnetic rotatory powers of bodies and their indices of refraction. We had already observed that it did not suffice for the various peculiarities of the phenomenon in solids and liquids; and it is easy to see that it does not account for the relation of the magnetic rotatory powers of bodies in the liquid and gaseous conditions.

In my previous researches, I had been led to adopt the expression $n^2(n^2-1)$ as approximately representing the statements of the magnetic rotatory powers of the various solid and liquid bodies that had been studied. I had discovered that the relation of the magnetic rotation R to the function $n^2(n^2-1)$ varied very little when compared with the great variations of the magnetic rotations of one body and another.

The unit adopted was the magnetic rotation of liquid carbon bisulphide; and the values of the expression $\frac{R}{n^2(n^2-1)}$ (generally about 0·25) varied between 0·10 and 0·50*. These values are indicated in column 5 of the preceding table. The numbers obtained are the same as for various solid and liquid bodies. Thus, although the magnetic rotations are 10,000 times smaller than in liquid bodies, the variations in the function $n^2(n^2-1)$ are always of the same relative order of size as those of the magnetic rotations.

It ought, however, to be observed that the numbers in the fifth column of the preceding table increase regularly with the indices of refraction, which tends to prove that the formula $n^2(n^2-1)$ is only an approximate expression of the phenomenon, which answers sufficiently well for the properties of non-magnetic solid and liquid bodies, but which might diverge from experiment when the values of the index of refraction became very small, as in the case of gases.

The variations of the expression $\frac{R}{n^2(n^2-1)}$ with gases do

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* See the researches quoted above.

not, however, exceed the limits of those which are presented by solid and liquid bodies; and this study must be extended to a much greater number of gases before we are able to decide whether these variations are not simply characteristics of the physical and chemical constitution of gases, as we have discovered in regard to other substances.

Some interesting remarks may be made on this subject. The numbers given above enable us to compare the magnetic rotatory powers of gaseous sulphurous acid with that which it possesses in a liquid state. M. de la Rive found for liquid sulphurous acid a magnetic rotatory power variable with the temperature, and which at about 12° is nearly 0·382. But the index of refraction of this body has not been accurately measured. Faraday merely says that the index is the same as that of water; on this hypothesis we should find that the ratio $\frac{R}{n^2(n^2-1)}$ is 0·277—that is to say, exactly the half of the number found with the gaseous body. Is this statement a mere conjecture, or is it the expression of a more general fact?*

An analogous result is observed on comparing with the number obtained for oxygen the numbers given by certain highly oxygenated liquids, such as nitric and sulphuric acids, &c. $\frac{R}{n^2(n^2-1)}$ is about 0·11 for these bodies, while it is 0·27 with gaseous oxygen. Finally, we may connect with these facts an observation relative to various salts (chloride of sodium and chloride of potassium) endowed with a positive rotatory power. Their magnetic rotatory power is less in a crystalline condition than in solution.

These various remarks would then tend to show that the positive rotatory power of a body is so much the greater in relation to its index of refraction as the particles of the bodies are further apart, and that it increases on passing from a liquid to a gaseous state.

This question requires a special study, which could not form part of the present work, but to which I intend to return at some future time. If we are not absolutely authorized to assert that the function $n^2(n^2-1)$ plays the same role for the magnetic rotations of gases as for other substances, this re-

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* In a recent article (*Journal de Physique*, August 1880) M. E. Bichat has studied sulphurous acid, both liquid and gaseous. He found for the magnetic rotation of the liquid a number very near to that of M. de la Rive; for the index of refraction of the same body he gives the number 1·34; but the magnetic rotatory power that he attributes to the gas differs very much from that deduced from my experiments.

markable fact is nevertheless established,—that the magnetic rotatory powers of bodies are closely connected with their index of refraction n, and that the variations of the function $n^2(n^2-1)$ are of the same order and magnitude as those of the magnetic rotations of bodies in the solid, liquid, and gaseous conditions.

In concluding this memoir, I think it necessary to lay stress on the theoretical interpretation of the formula $\frac{n^2(n^2-1)}{\lambda^2}$, which, according to these researches, seems to intervene in the expression of the magnetic rotatory power of bodies. It can be written $\frac{n^2-1}{\left(\frac{\lambda}{n}\right)^2}$; under this form the denominator represents the wave-length of the luminous vibratory movement inside the body studied. The rotation of the plane of polarization of the light would be therefore in the inverse ratio of the square of this wave-length, and would be proportionate to the expression n^2-1, which, according to the theoretical views of Fresnel and the experiments of M. Fizeau, measures the partial influence of the undulatory movement of a body on the luminous waves.

It is interesting to note that experiment leads to this same expression to measure the action of magnetism on the propagation of luminous vibrations through bodies, thus establishing a new relation between a mechanical effect and the effects of electromagnetic influence. Perhaps this influence of luminous waves on the vibratory movement so well studied is of a kind to inform us as to the mode of motion which manifests itself to us under the form of electricity and magnetism.

CONCLUSION.

The results established in this memoir are as follows:—

1st. Bodies in a gaseous condition possess, like solid and liquid substances, the property of deflecting the plane of polarization of luminous rays passing through them when they are submitted to the influence of magnetism.

2nd. The magnetic rotations of the planes of polarization of rays of different wave-lengths traversing the same gas (oxygen excepted) are generally very nearly in the inverse ratio of the squares of the wave-lengths of the luminous rays considered.

3rd. The magnetic rotatory powers of gases can be compared with that of liquid carbon bisulphide, and consequently with those of other solid and liquid bodies.

We have discovered in gaseous bodies, as it had been formerly observed for other substances, a remarkable relation between the indices of refraction and the magnetic rotatory power of rays having the same wave-length.

4th. Finally, oxygen presents an anomaly which seems connected with the exceptional magnetic properties of this gas.

LII. *On the Beats of Mistuned Consonances.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your November number, in a paper by Professor S. P. Thompson on Binaural Audition, there occur some references to my recent work. While it is gratifying that the work should have attracted Prof. Thompson's attention, it is rather disheartening to find that he has failed for the most part to grasp its purport. I am obliged to him for his remarks, as, if he has failed to understand it, I dare say a good many other people will have done the same; and it affords me the opportunity of putting two or three points in a clear light. I will take the passages I have to observe upon in order.

Page 352. Halfway down, the word *subjective* is underlined and a note appended. In the note there is first an objection to my use of the word. I have never believed much in the advancement of clearness by the employment of words very strictly defined. In speaking of any particular matter, it seems to me generally necessary and sufficient to give such descriptions as shall leave no doubt of the meaning. This I have amply done. Over and over again I have described the subjective phenomena as being such as originate within the ear itself. Whether this is the strictly philosophical sense is to me immaterial. I took the use of the word from Helmholtz (Ellis's 'Helmholtz,' p. 234, foot of page; 4th German edition, p. 259, top of page). I think it is there used in the sense in which I use it*. I may put the matter otherwise thus:—In a binary combination $a \sin A + b \sin B$ I find the term lower in pitch ($a \sin A$) subject to a variation of the coefficient a. Does this variation arise before or after the arrival of the combination at the gates of the ear? If before, I call the variation objective; if after, subjective. I will not repeat the argument from resonators; it is sufficiently stated in my paper.

Prof. Thompson then makes an objection which precisely

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* Viz. as opposed to the objective origin of combination-tones in instruments described a little further on.

misses the whole point of my paper. He objects—"the beats are objective; they can be seen in a manometric flame if the primary tones are sufficiently loud" &c. That is to say, my exposition of the existence in air of the Smith's beats, or variations of resultant displacements, and demonstration by difference of properties that they are not the same as the beats we hear, is all thrown away.

To take a simple case: suppose the two notes are represented by $a\,(\sin \mathrm{A} + \sin \mathrm{B})$. Then the Smith's beats, which you can see in the manometric flame, are given by

$$2a \sin \frac{\mathrm{A}+\mathrm{B}}{2} \sin \frac{\mathrm{A}-\mathrm{B}}{2};$$

which we may regard as a harmonic curve $\sin \frac{\mathrm{A}+\mathrm{B}}{2}$, with a varying coefficient $2a \sin \frac{\mathrm{A}-\mathrm{B}}{2}$. That is to say, as I have pointed out in my paper, the pitch of the note which varies to make these beats is about halfway between the pitches of the primaries; whereas, in the beats we actually hear, the pitch of the note which varies is very nearly that of the lower primary note itself. Further, the very existence of the resultant beats depends on the maintenance of the unvaried primaries; whereas the beats we hear consist of variations of one of the primaries. This and the argument from resonators are entirely distinct; they converge on the same result.

The rest of the note I need not answer; it appears to depend on the use of the word "subjective," about which I do not care. I am only solicitous about certain questions of fact, which can be quite well stated without the use of any particular word; though I maintain that, in the present case, my use of the word subjective is amply justified by authority.

The other point with which I have to trouble you is again on p. 352, just below the first. It is given as a statement in my paper—"(2) that the beats consist, *as König discovered in* 1875, of variations in the intensity of the lower of the two interfering tones."

This is really too bad. The whole of my work was originated by the fact that König never analyzed his beats, and never enunciated any opinion whatever as to the notes of whose variation the beats consist—except in the one case of the octave, which I have observed upon in my paper, where he says that fundamental and octave are heard alternately. The law above mentioned was unquestionably first obtained by me. I knew König's papers intimately at the time, and there was no vestige of guidance in them on the subject; so I object to have the credit of this law taken away by a careless sentence like the above.

I do not think there is any thing I need add, except that I do not follow the argument by which Prof. Thompson arrives at the result that the beats of the lower notes are not subjective. There seems to me to be no proof whatever of this result.

Yours truly,
R. H. M. Bosanquet.

LIII. *On* Wheatstone *and* Brewster's *Theory of Binocular Perspective. By* W. Le Conte Stevens[*].

In 1838 Sir Charles Wheatstone[†] announced his invention of the reflecting stereoscope, by means of which slightly dissimilar pictures of the same object could be simultaneously viewed in such a manner as to produce the illusion of binocular relief. The essential object attained by using this instrument he expresses by saying[‡], "the two pictures (or rather their reflected images) are placed in it at the true concourse of the optic axes." The apparent distance of the combined image is thus determined by the intersection of these axes. In a subsequent paper[§], published in 1852, Wheatstone described a number of experiments with his stereoscope, by which he had investigated the effect of varying the convergence of visual lines. The result he expressed by saying[‖], "The perceived magnitude of an object diminishes as the inclination of the axes becomes greater while the distance remains the same; and it increases, when the inclination of the axes remains the same while the distance diminishes." In a subsequent paragraph, however, speaking of the change in apparent magnitude of the image produced by varying the optic angle, he says[¶], "and yet, if we attentively regard it in any fixed position, it is perceived to be at a different distance." That Wheatstone had not given up the idea of apparent distance expressed in his first paper, however, is shown by his subjoining[**] a table of inclinations of the optic axes which correspond to different distances; it also shows the angular positions of the camera required to obtain binocular pictures

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[*] Abstract of a memoir read before the New York Academy of Sciences, Oct. 24, 1881. Communicated by the Author.

[†] Wheatstone, "Contributions to the Physiology of Vision," Phil. Trans. 1838, part ii. Also reprinted in the Philosophical Magazine, April 1852, p. 241.

[‡] Ibidem, p. 245 (1852).

[§] Ibidem, p. 504.

[‖] Ibidem, p. 507.

[¶] Ibidem, p. 508.

[**] Ibidem, p. 512.

which shall appear at a given distance in the stereoscope in their true relief." This table is constructed from the following formula* ("a denoting the distance between the two eyes, and θ the inclination of the optic axes"):—

$$D = \tfrac{1}{2} a \cot \tfrac{1}{2}\theta.$$

From this formula it is obvious that if the axes become parallel, D becomes infinite; if divergent, D becomes negative. Wheatstone makes no reference to the attainment of optic divergence; but his stereoscope was constructed in such manner that the visual lines might be made parallel. Of the images thus produced he notes as a peculiarity†, "although the optic axes are parallel, or nearly so, the image does not appear to be referred to the distance we should, from this circumstance, suppose it to be, but it is perceived to be much nearer."

These apparently contradictory statements of Wheatstone show how difficult it is to interpret our sensations when the conditions attendant upon normal vision are disturbed. Wheatstone notices the distortion of perspective produced by making the angle between the camera axes different from that between the visual lines when the stereograph is viewed‡; but in this connexion he does not mention any apparent change in distance of the picture as a whole.

In 1844, Sir David Brewster published a paper§ "On the Knowledge of Distance given by Binocular Vision," in which he elaborated the idea that apparent distance is determined by intersection of visual lines, giving an interesting geometric discussion of the subject, and describing many experiments performed without the aid of the stereoscope. The intersection of the visual lines he calls the binocular centre; and for its distance from the card on which the conjugate pictures are drawn he deduces a formula, the discussion of which leads to results identical with those deduced from Wheatstone's formula. In his paper relating to the lenticular stereoscope‖, and other forms of the instrument devised by him, he frequently refers to the external intersection of axes coincident in direction with the rays after refraction or reflection, but makes no reference to divergence of visual lines.

The theory of apparent distance of objects perceived in the stereoscope, to which Wheatstone, and especially Brewster, gave prominence, has been abundantly reproduced in our text-books on Physics; indeed it is expressed either directly

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* Phil. Mag. p. 512 (1852). † Ibidem, p. 514. ‡ Ibidem, p. 512.

§ Edinburgh Transactions, vol. xv. p. 663 (April 1844).

‖ Phil. Mag. Jan. 1852, p. 16.

or implicitly in every explanation of the stereoscope to which I have had access, where a diagram is employed in tracing the course of rays entering the eyes. That it is misleading and should be abandoned, is shown by the fact that the perception of binocular relief and the judgment of apparent distance in the stereoscope are easy when conditions are such as to make the visual lines slightly divergent. If their intersection determines the point of sight, this would then be behind the observer's head. In taking the pictures composing the stereograph, each point in the field of view is at the intersection of a pair of secondary camera axes; and thus far, but no further, Wheatstone's formula is strictly applicable. No diagram can give more than an approximation to the truth when the pictures are viewed in the stereoscope; and not one hitherto published has exhibited any provision for the possibility of optic divergence. I have elsewhere* shown how such a diagram may be constructed by application of Hering's theory of a binocular eye (*œil de cyclope imaginaire*)†, assuming that we know the angle of convergence between the camera axes for a given point in the field of view.

The fact that optic divergence is possible with distinct binocular fusion of retinal images is not new. The use of prisms for testing the rectus muscles of the eyeballs has long been known; and Helmholtz not only mentions the use of stereographs for the same purpose, but observes the increase in apparent distance of the point of sight when divergence of visual lines is induced‡. No analysis of the visual phenomena seems to have been made, however; and their important bearing upon the theory of stereoscopic perspective seems to have escaped notice. Helmholtz says§ in regard to such illusions of sight, "If these images are such as could not be produced by any normal kind of observation, we judge them according to their nearest resemblance;" but comparison is scarcely satisfactory if direct analysis is possible.

I have examined many stereoscopes and stereographs in testing Brewster's theory||, and have found that optic divergence is often necessary; hence it is unconsciously practised by nearly every one who uses them. What is really requisite is not that the visual lines shall converge, but that the eyes shall receive upon corresponding retinal points the images of

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* American Journal of Science, Nov. and Dec. 1881.

† Hering, *Beiträge zur Physiologie*; or Helmholtz, *Opt. Physiologique*, p. 777 (edit. 1867).

‡ *Opt. Phys.* pp. 616 & 828.

§ 'Popular Lectures on Scientific Subjects,' p. 307 (tr. 1873).

|| American Journal of Science, Nov. and Dec. 1881.

corresponding points on the stereograph. The relative positions of the different parts of the externally projected binocular image is determined mainly by the stereoscopic displacement in the pair of pictures viewed. This in turn is determined by the degree of convergence of the camera axes; while the relation between the visual lines of the observer in the stereoscope may be either convergence, parallelism, or divergence. It is not, however, a matter of indifference which of these relations is assumed. Theoretically the visual lines should meet at an angle the same as that between the camera axes, when a given point is examined; practically this is rarely ever the case. The distance between corresponding stereograph points is adjusted to secure, if possible, comfort to an average pair of eyes in viewing the picture; and the same stereograph may necessitate convergence of visual lines for one observer, divergence for another, in order that binocular fusion be attained.

In studying the effect of varying the optic angle while a given stereograph is examined, I have found a modification of Wheatstone's reflecting stereoscope to be valuable. The arms were made to glide under a divided circle in such manner that, from a given point on either of the conjugate pictures, the angle of incidence at the surface of each mirror could be varied from 0° to 60°. When its value is 45° the visual lines must be parallel, if the image is seen binocularly; if less than 45°, they must converge; if greater, they must diverge. Calling angles of convergence positive, I was able to vary the optic angle from $-7\frac{1}{2}°$ to $+80°$, though clear and steady vision was not possible through so wide a range. Placing the conjugate pictures each at a fixed distance, such as 50 centim., this being the sum of the lengths of the incident and reflected rays, the apparent distance of the binocular image should be 50 centim. when the arms of the instrument are so disposed as to make the optic angle about equal to 7° 20′. The effect upon the combined image may then be noted for any change in the relation between the visual lines.

Helmholtz* has shown that the visual lines are not coincident with the true optic axes of the eyeballs; but since each is practically axial in its relation to the crystalline lens, it will involve no confusion to call them visual axes, while the angle, α, between them is still called the optic angle. If an isosceles triangle be constructed whose base, a, is the interocular line, then for the distance, D, of its vertex from either optic centre, we have

$$D = \tfrac{1}{2} a \operatorname{cosec} \tfrac{1}{2}\alpha.$$

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* *Opt. Phys.* p. 93 (edit. 1867).

The optic vertex is simply Brewster's "binocular centre." In normal binocular vision of near objects, the distance of the point of sight is sufficiently nearly given by this formula, while the subjective effect is that of the superposition of the two retinal images in a central binocular (cyclopean) eye*, the two visual axes being combined into a single median line, along which distance is estimated. These lines in turn have no objective existence; and their intersection really cannot determine any external point. Any effect due to axial convergence is merely the interpretation which experience has taught the observer to put upon the sensation of muscular tension, as the internal rectus muscles contract. This interpretation may be strengthened or antagonized by what other conditions suggest; and the estimate of distance on the combined median line is modified accordingly. The direction of the combined image is along this median, whether the visual axes be convergent, parallel, or divergent.

If the conjugate pictures selected be the simplest possible, so that all kinds of perspective are eliminated except that due to stereoscopic displacement, there are still left three elements to consider, which can never be entirely dissociated. These are:—

I. The optic angle, interpreted by the sensation of tension in the rectus muscles of the eyeballs.

II. The focal adjustment, interpreted by the tension in the ciliary muscle encircling the crystalline lens.

III. The visual angle, interpreted by recognition of the retinal area impressed.

In normal vision, these elements conduce to the same visual effect without becoming separately manifested to the consciousness of the individual. In the stereoscope they may conflict among themselves; and the result is modification of judgment, attended with some uncertainty.

Let us now assume the arms of our stereoscope to be so turned upon their pivot as to change the value of α from 7° 20′ to 37° 20′, the interocular distance, *a*, being taken as 64 millim. Applying our formula, we find $D=10$ centim. The focal adjustment, however, must still be for an image 50 centim. distant, if distinct vision is attained. Strong contraction of the internal rectus muscles is habitually associated with nearness of the point of sight, and accompanied with strong ciliary contraction. But in the present case the effort to secure distinct vision implies relaxation of the ciliary muscles, the effect of which is to suggest greater distance of the point

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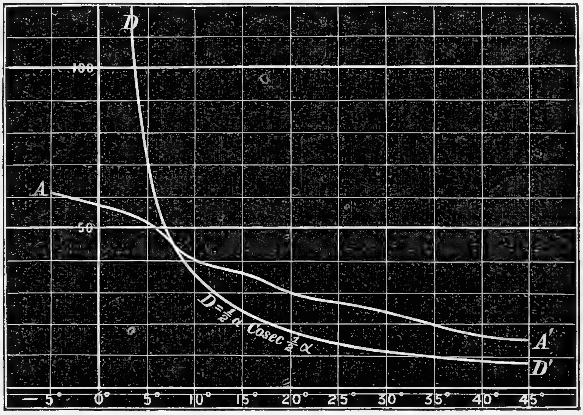
* Le Conte, 'Light,' New York, 1881, pp. 213-269; or American Journal of Science, ser. 3, vol. i. p. 33 *et seq.*

of sight than is suggested by the tension of the rectus muscles; and there is partial dissociation between actions generally associated. Finally, the visual angle has remained constant; and this tends to produce the idea of constancy in distance. Of the three elements, however, the first is found most important; and the combined effect is that the image appears 18 or 20 centim. distant, the judgment being quite uncertain. The apparent area and depth of the external image are diminished approximately in the ratio of the diminished distance.

Let the arms of the stereoscope be now pushed back till $\alpha = -5°$. Relaxation of the internal rectus and contraction of the external rectus muscles is habitually associated with recession of the point of sight; and in this case it is carried beyond the limit of axial parallelism. But, again, the focal adjustment is for a point only 50 centim. in front; and the visual angle is constant. Of the three elements the last is now most important; and the combined effect is that the image appears about 60 or 70 centim. distant, with corresponding increase in apparent area and depth.

In the accompanying figure (fig. 1) I have shown the result

Fig. 1.

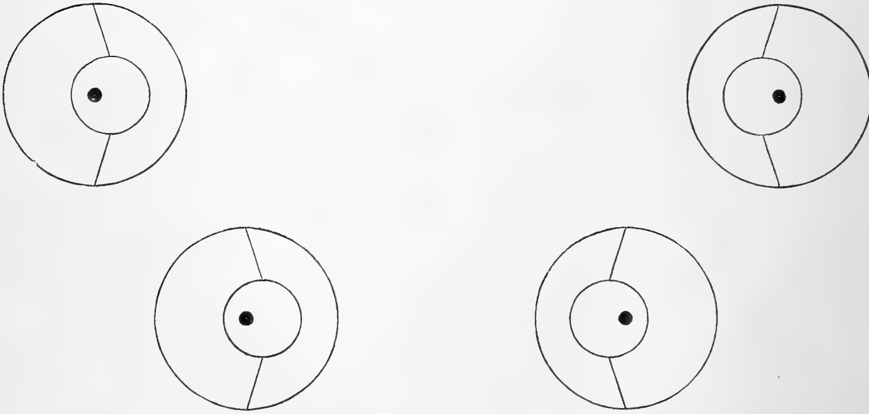


of experiments conducted at different times during the last few months. The stereoscope was manipulated by an assistant, who varied the optic angle in the most irregular order, recording its values and my corresponding estimates of distance, while I was kept ignorant of these records until the entire list of estimates had been completed. Six independent series, of eighteen estimates each, were made; and the curve A A′ expresses the mean result, the values of the optic angle being taken as abscissas and estimated distances as ordinates, the true distance of the stereograph being 50 centim. D D′ is the curve whose equation is $D = \frac{1}{2} a \operatorname{cosec} \frac{1}{2} \alpha$, the value of a being my interocular distance, 60 millim. As might be expected, the probable error is large, being about ±8 millim.; and the curve A A′ is by no means regular, though its general import is unmistakable. It cuts the theoretic curve near the point corresponding to $\alpha = 7° 20'$, but rather lower than might have been anticipated.

The stereograph employed was one of the full moon, the two pictures being of course cut apart, and so arranged as to produce conversion of relief, thus destroying any resemblance to known bodies; while objects for comparison were excluded from the binocular field of view. All estimates of distance were for the foreground. For very large positive optic angles the dissociation between axial and focal adjustments becomes extremely difficult; and distinct vision at the distance of 50 centim. is then impossible. For negative values focalization is comparatively easy; but beyond $-6°$ the unaccustomed strain upon the external rectus muscles produces unsteadiness of vision. As limits, therefore, $-5°$ and $+45°$ were selected.

Although such employment of the muscles of the eyes is unusual when carried to the extent implied in these experiments, and the disturbance of natural coordination is at first confusing, the use of the stereoscope nearly always necessitates such disturbance, to a limited extent, and is hence not unfrequently productive of discomfort. Most persons, however, who possess healthy eyes will find it possible to secure binocular combination of the conjugate diagrams in fig. 2, if viewed in the stereoscope. For this purpose they are placed at the bottom of the page, which may be rested at the proper

Fig. 2.



distance in front of the semi-lenses of the stereoscope. When the lower pair are seen, by axial convergence, as a truncated cone, the upper pair are seen separately and monocularly, without the appearance of relief. For binocular combination of the upper pair divergence of visual lines is required, assuming an average pair of eyes and an average lenticular stereoscope, at least as found in New York. The truncated cone now appears larger, deeper, and more distant. It is unnecessary to explain why the background dot should appear double when the foreground circle is seen single, and *vice versâ*.

In the course of these experiments, extending over many months, it has been found that, although the coordination of muscular actions in the eyes is commonly directed only to the attainment of perfect vision, their dissociation is largely under the control of the will. Not only is it possible directly to diverge the visual lines without employing any external points of fixation, but I find it not difficult to contract the ciliary muscle strongly, thereby destroying the distinctness of distant vision, while the relation between the visual lines undergoes scarcely any noticeable variation. Certain peculiar visual effects result from this; but the discussion of them must be reserved at present.

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LIV. *Notices respecting New Books.*

Conic Sections treated Geometrically. By S. Holker Haslam, *B.A.*, *and* J. Edwards, *B.A.* London: Longmans. 1881. Pp. 137.

THE authors start from the usual Focus and Directrix definition of the curve, and call to their aid what they call the *Auxiliary Circle of a Point.* This is the circle first used, so far as we are aware, by Boscovich in his *Sectionum Conicarum Elementa* (1754), but without a name: of its use he writes:—"Mirum sane quam fœcunda est hæc constructio, quam tyroni exercendo apta. Plurima quidem ex eâ inferri possunt theoremata et pleraque utilissima, ac iterum fœcunda." Rediscovered by George Walker (1794), and named by him the *Generating Circle*, it has again come to the front in Mr. Charles Taylor's book, and there poses as the *Eccentric circle of a point.* It certainly furnishes a very neat basis for operating upon the Sections; and its usefulness is extended in a neat way by our Authors to what they call *Focal Projection.* The first five chapters give the familiar properties of these curves; the sixth treats of curvature; the seventh of the Right circular cone; the eighth of Transversals leading up to the above-named Focal Projection in chapter nine, and Orthogonal Projection in chapter ten. Six sections are devoted to Exercises at the end of the book. The proofs have a freshness about them dependent upon the introduction of the Auxiliary circle, and are concisely and clearly exhibited. We have come across a few errata, which, however, the student will, we believe, easily be able to correct.

Exercises in Analytical Geometry. By J. M. Dyer, *M.A., Senior Mathematical Master in the Classical Department of Cheltenham College. With Illustrations.* Macmillan and Co. 1881. Pp. 152.

The bare title of this book by no means does it justice. It is certainly not a heterogeneous collection of examples, were it nothing more; but besides an excellent progressive arrangement and grouping of exercises under the different sections of Analytical Geometry, there are some most valuable hints for solving the difficult problems interspersed among the answers, together with a number of figures. As a supplement to the best and well-known modern treatises on Coordinate Geometry, it will be useful to both beginners and more advanced students.

LV. *Intelligence and Miscellaneous Articles.*

AN AMUSING EXPERIMENT WITH LIQUID FILMS.

BY J. PLATEAU.

I HAVE sought to derive a little amusement from thin liquid films. I have had constructed of iron wire of about 0·5 millim. thickness the outline of a flower with six petals of oval form; these are each 24 millim. in length, and 19 millim. in their greatest width. The central ring, made of the same iron wire, to which they are attached, has a diameter of 14 millim.; it is supported beneath by a little fork, which is fastened to a thicker iron wire, forming the stalk of the flower; this last is implanted in a small board which serves as a support. All the petals are in the same plane; and when the board rests on the table the flower is horizontal.

The whole contour of the flower was first slightly oxidized by keeping it for a few moments in dilute nitric acid; then, after washing it, it was immersed horizontally in the glycerine liquid, but only to a very little depth, in order to avoid the formation of a film in the fork; it was drawn out again, also horizontally, and was then turned up and the board placed upon the table. Finally the whole was covered over with a glass bell, to shelter the films from the slight agitations of the air. The apparatus was placed before a window so that the sky was seen by reflection from the films.

I thought that the petals would soon present uniform tints, which would gradually change, in accordance with the attenuation of the films; but it was not so. During the first moments the flower of course appeared colourless; then, upon each petal and the central circle, shades of rose and green of the last orders were seen to appear, which gave place to irregularly distributed bands and spots presenting tints of higher orders. The appearance was then that of a flower streaked with the most lively colours. These were afterwards slowly modified; and ten hours after the formation of the flower some small black spots were distinguished upon the metals. The observation had to be discontinued on account of the lateness of the hour; and the next morning several of the petals had burst.

I ought to mention that the glycerine liquid which was used for this experiment was of very indifferent quality, perhaps because the oleate employed was not prepared with the requisite care, or because Price's glycerine is nowadays not so pure as it was formerly. But this may have been an advantage; for with an excellent liquid the modifications of the flower would have been too slow.

If after withdrawing the flower from the liquid, and while all the petals are still colourless, they be successively burst in the middle by a wire heated in the flame of a spirit-lamp, a numerous series of brilliant little liquid masses are instantly seen attached to the entire metallic contour, which threads them like the beads of a necklace: their arrangement is very nearly regular; and there are fourteen of them to each petal.

I seize this occasion to insist on two precautions to be taken in making the oleate of soda, precautions indispensable if we wish to form good glycerine liquid. In the first place, the oleic acid obtained at one stage of the preparation must be most carefully protected from the action of the oxygen of the air; in the second place, the sea-salt employed to separate the oleate at the end of the operations must have been perfectly purified. Let us add that, when the oleate is prepared, we must take care not to dissolve it in alcohol to render it more pure; I believe I have proved that, on the contrary, this spoils it.

Let us, in conclusion, with respect to the experiment of the flower, mention a curious instance of persistence of impressions, an instance moreover which is not unprecedented*. My wife, whose eyes are very sharp, had observed the flower at different times during the day. Now, at her waking on the morrow, casting her eyes upon the white curtains of the bed, she saw the image of the flower: one of the petals was so clearly delineated that she distinguished even the wire forming its outline; the interior was striped with white and yellow; the other petals were more or less indistinct. Changing the direction of her gaze caused the image to disappear.—*Extrait des Bulletins de l'Académie Royale de Belgique*, sér. 3, t. ii. no. 7 (1881).

PHOTOMETRIC COMPARISON OF SOURCES OF LIGHT OF DIFFERENT TINTS. BY A. CROVA.

The exact measurement of the ratio of the intensities of two lights of which the emission-temperatures are very different presents great difficulties, on account of the difference of composition of the two lights which are to be compared. The equality of intensity of either the shadows or the illuminated regions of the photometric screen cannot be exactly determined, because of the differences

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* See Stevelley, *L'Institut*, 1859, no. 1309, p. 38: Gorini, *Ann. di Ottalm.* t. iii. p. 164.

of their tints. The employment of a spectrophotometer permits this difficulty to be surmounted.

Let us suppose the simple radiations which constitute the light emanating from an electric regulator and that from a standard Carcel lamp spread out in two contiguous spectra. If the distances of the two sources from the photometer be such that their mean illumination is the same, the two spectra will be far from presenting the same aspect: that of the carbon points is more intense towards the violet extremity, and less luminous towards the red, than that of the lamp. The temperature of emission of the electric light being much higher than that of the lamp, the ratio of the intensities of the simple radiations of the electric light to the intensities of the corresponding lights of the spectrum of the flame of the lamp, will with equality of mean illumination, be represented by a fraction greater than unity towards the violet; but if the red be approached, the ratio will gradually diminish, following the law of continuity, and will be less than unity at the red end.

There exists, then, *a determinate simple radiation* (whose wavelength depends on the nature of the lights compared) for which that ratio is exactly equal to unity. If this radiation be precisely known, the measurement of the ratio of its intensities in the two spectra will give *exactly* the ratio of the total intensities of the two sources.

I have constructed a very simple apparatus which permits the practical realization of the theoretic conception of which I have just spoken. Let us illuminate the half-disks of the screen of a Foucault's photometer with an electric light and that of a standard Carcel lamp: the two illuminated regions being placed tangentially, it is very difficult to judge of the equality of the illumination, one of the moieties having a bluish tint in comparison with the other, which appears to be of an orange-yellow; and the standard lamp can be displaced within pretty wide limits without the eye being able to judge if equality of illumination is really obtained.

Let us look at the screen, placing in front of the eye two Nicol prisms the cross sections of which are rectangular, and between which is placed, perpendicular to the axis, a quartz plate of 9 millim. thickness. The eye then sees the two fields coloured of a green tint washed with white; and if we vary the distance of the standard lamp, a moment arrives when, equality of intensity being obtained, the line of demarcation of the two fields disappears. A very slight variation of the distance of the lamp then suffices to give rise to a very sharp contrast between the two regions.

The thickness of the quartz plate has been calculated so that its interposition between the two rectangular nicols give rise, in the spectra of the two sources, to two large interference-bands situated in the two extremities of the spectrum. On passing from these two bands towards the middle of the spectrum, the intensity of the different preserved radiations varies as the square of the cosine of the angle made by the cross section of the second nicol with the planes of polarization of the different radiations, which have undergone rotatory dispersion in the quartz plate. There is therefore one of them for which the square of the cosine is equal to unity, and which suffers no weakening. It is conceived that it is possible empirically to displace the second nicol (and thereby the two bands) so as to preserve the maximum of intensity to the simple radiation the comparison of which would give the same result as that of the total intensity. The neighbouring radiations are more and more rapidly weakened in proportion as their distance from this maximum increases; and this circumstance diminishes the influence of their differences of intensity (which increase with their distance from the maximum) to such a degree as to render then negligible.

In practice the system of nicols and quartz plate is fixed in the body of a small telescope placed opposite to the screen of the photometer, upon the cone through the apex of which we view it. This instrument, constructed with much precision by M. Duboscq, tried with the Drummond light, an electric regulator or sun-light, comparatively with the standard Carcel, immediately gave very precise results in spite of the difference of tint of those different lights.—*Comptes Rendus de l'Académie des Sciences,* Sept. 26, 1881, t. xciii. pp. 512, 513.

A NEW DEMONSTRATION OF RIEMANN'S THEOREM.

BY M. CROULLEBOIS.

M. Clausius* has presented Riemann's theorem as a consequence of Green's formula; that important proposition can be demonstrated more directly. The theorem is thus enunciated by Clausius:—

Given two conducting bodies A *and* B, *which may either be insulated or connected with the earth by a conductor; in the vicinity of these let there be any number whatever of other conducting bodies put into communication with the earth by means of conductors. While* B *is in communication with the earth and* A *is insulated, this latter body is charged with electricity up to a certain potential-level* K. *Let* Q_b^a *be the quantity of electricity accumulated by influence upon* B *under these circumstances.* A *is afterwards put into communication with the earth, while* B *is insulated and charged with electricity to the same potential-level* K. *Let* Q_a^b *be the quantity of electricity accumulated upon* A *by influence under these circumstances. We shall have*

$$Q_a^b = Q_b^a.$$

Let the two conductors A and B be initially accompanied by any number of conductors, all connected with the earth—that is to say, at the potential zero. Let us insulate A and bring it to the potential V_1; the effect of this increase of potential will be to raise the charge of A from 0 to Q_a^a, and to induce charges upon the other conductors, and in particular upon B the charge Q_b^a; we can put

$$Q_a^a = C_a^a V_1, \text{ and } Q_b^a = C_b^a V_1,$$

C_a^a being the capacity of A, and C_b^a the coefficient of induction of A upon B.

Let us discharge A by putting it into communication with the earth, and bring it to the potential V_2; there comes in like manner, in this second state of equilibrium, for the charges acquired upon B and induced upon A:—

$$Q_b^b = C_b^b V_2, \quad Q_a^b = C_a^b V_2,$$

---

* *Théorie Mécanique de la Chaleur,* t. ii. pp. 38–44.

C^b_b being the capacity of B, and C^b_a the coefficient of induction of B upon A. We have to demonstrate that $C^b_a = C^a_b$. Let us superpose these two states of equilibrium, characterized by the charges M_b and M_b upon the conductors A and B:—

$$\left.\begin{aligned} M_a &= C^a_a V_1 + C^b_a V_2 ; \\ M_b &= C^a_b V_1 + C^b_b V_2 . \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (1)$$

The actual electrical energy W of the system is, by equations (1) of the form

$$W = \tfrac{1}{2}\Sigma MV = r(V_1)^2 + t \,.\, V_1 V_2 + s(V_2)^2 . \quad \ldots\ldots \quad (2)$$

Let us now subject the system to an infinitesimal deformation by producing, for example, a very slight relative displacement of the conductors. The charges will not change; the potentials alone will vary; and the corresponding variation of the energy will be

$$\delta W = \tfrac{1}{2} M_a \delta V_1 + \tfrac{1}{2} M_b \delta V_2 .$$

Therefore $\tfrac{1}{2} M_a$ and $\tfrac{1}{2} M_b$ are respectively the partial differential coefficients of the function W with respect to the potentials V_1 and V_2. Differentiating, therefore, (2) with respect to V_1, one finds for the development of M_a

$$2rV_1 + tV_2 ;$$

in the same way, differentiating with respect to V_2, one finds for the development of M_b

$$tV_1 + 2sV_2 .$$

Finally, comparing these results with the equations (1), it is seen that

$$C^b_a = C^a_b ,$$

which was to be demonstrated.—*Comptes Rendus de l'Académie des Sciences*, Nov. 7, 1881, t. xciii. pp. 719, 720.

ANOTHER CONFIRMATION OF PREDICTION.

BY PLINY EARLE CHASE, LL.D.

On the 4th of October, 1878, I presented a communication to the American Philosophical Society*, in which I showed that the position of Watson's first intra-Mercurial planet, as computed by Gaillot and Mouchez, represented the third intra-Mercurial term of my harmonic series. At the last meeting of the British Association, Prof. Balfour Stewart read a paper, in which he gave indications of sun-spot disturbances by a planet revolving in 24·011 days, and consequently having a semiaxis major of 163. This confirmation, both of my own prediction† and of the calculations of the French astronomers, is the more interesting, because the first confirmation of my series was contained in a communication which was made to the Royal Society by Messrs. De La Rue, Stewart, and Loewy, forty-one days after I had announced the series to the Philosophical Society and published it in the 'New York Tribune'‡. The accordances are as follows:—

Prediction.		Confirmation.	
1st interior harmonic term	·267	De La Rue, Stewart, and Loewy	·267
3rd ,, ,,	·165	Gaillot and Mouchez	·164
		Stewart	·163

---

* Procceedings, xviii. pp. 34–36. † Ibid. xiii. p. 238. ‡ Ibid. p. 470.

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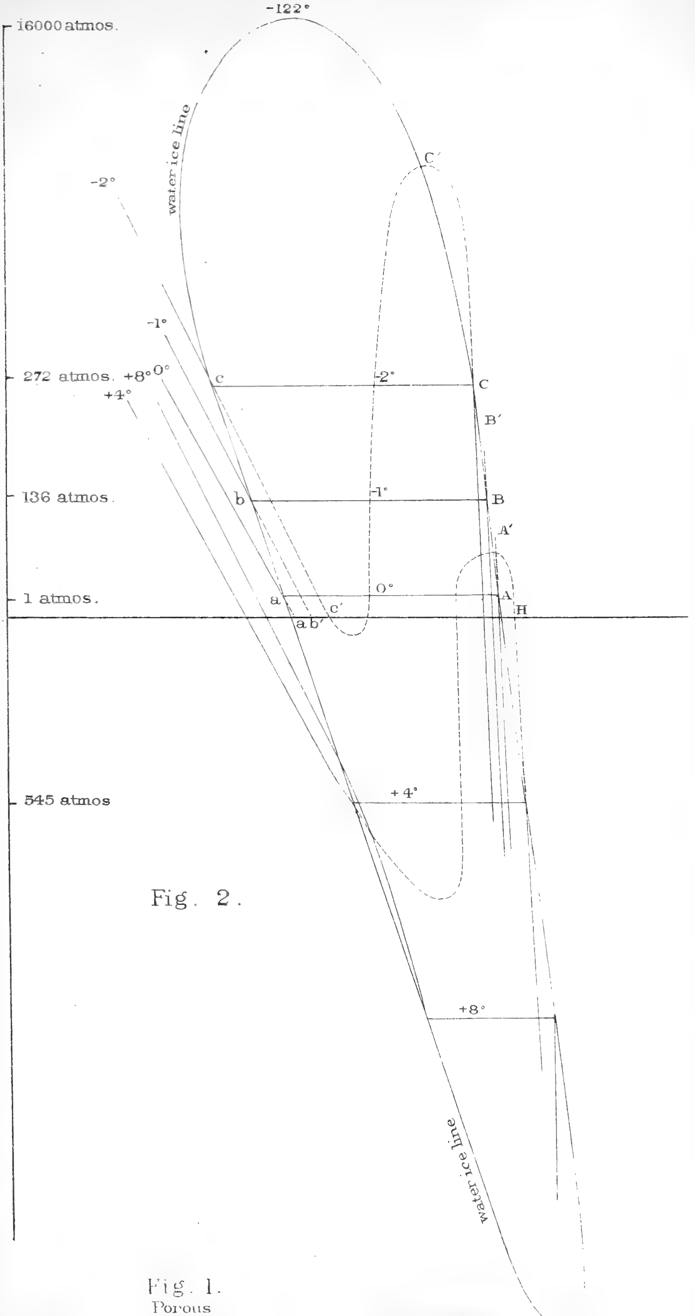

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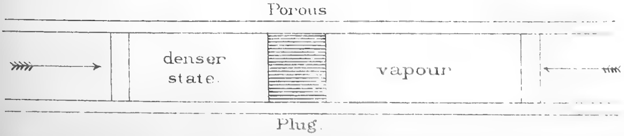

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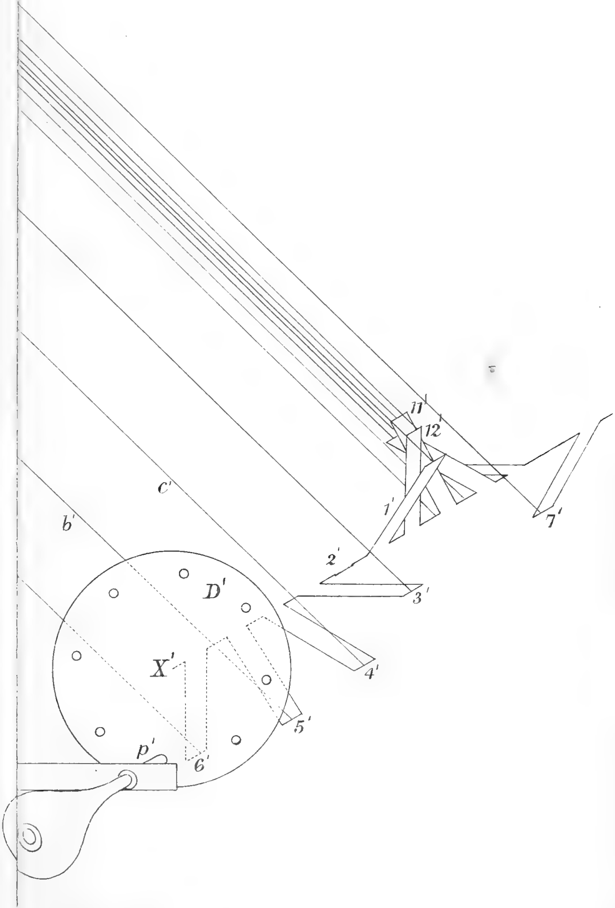
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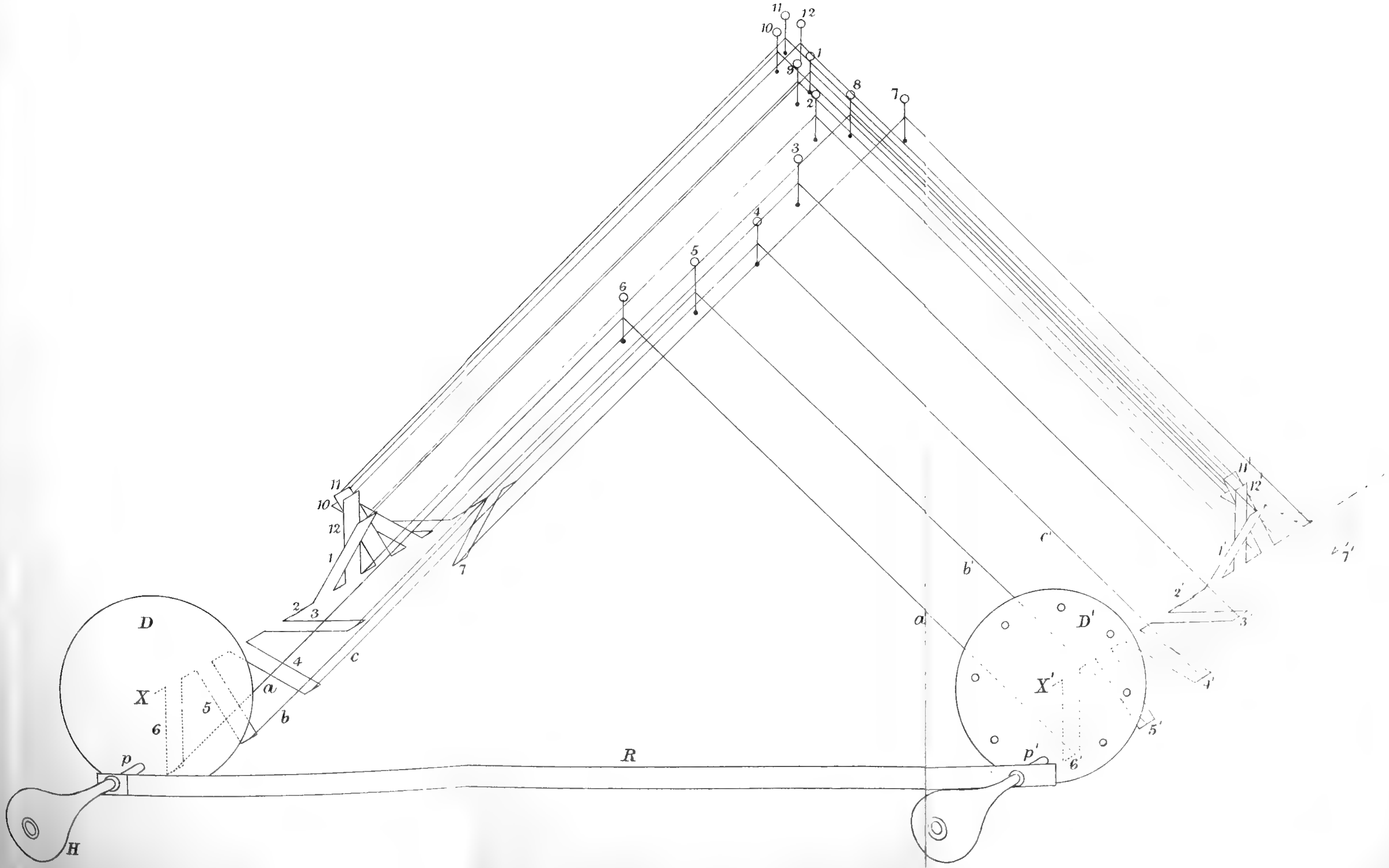

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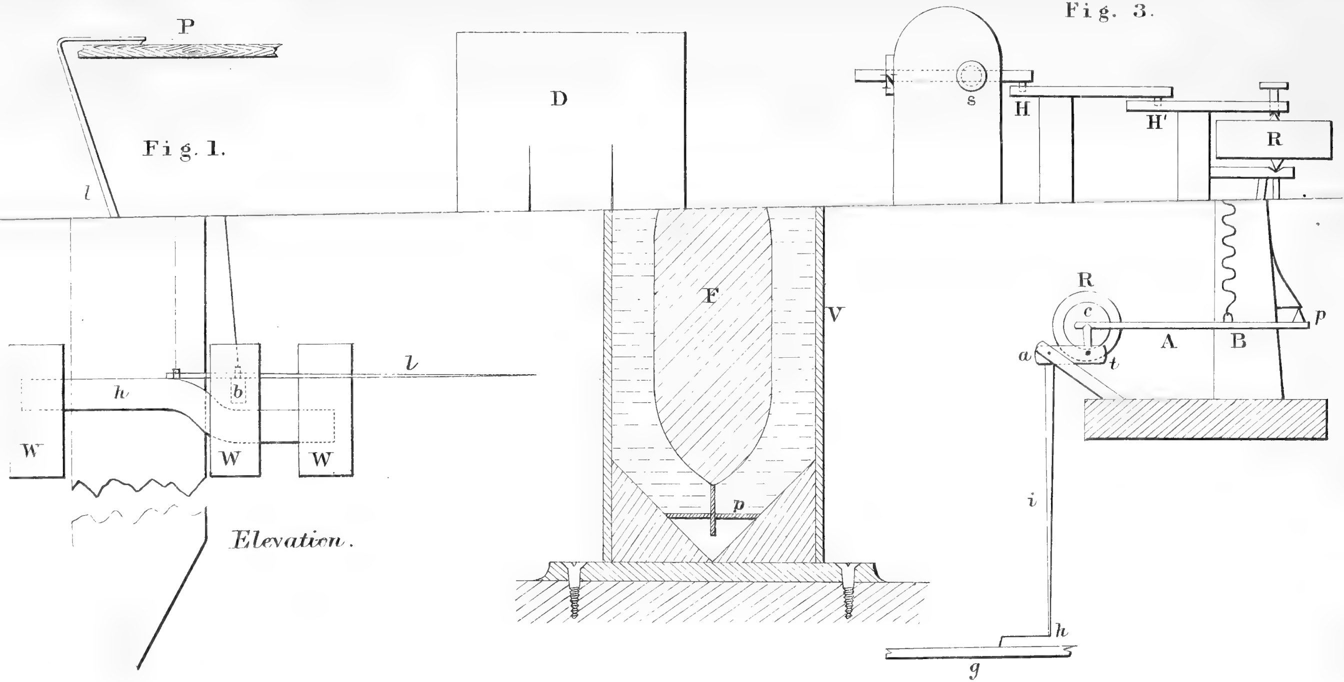

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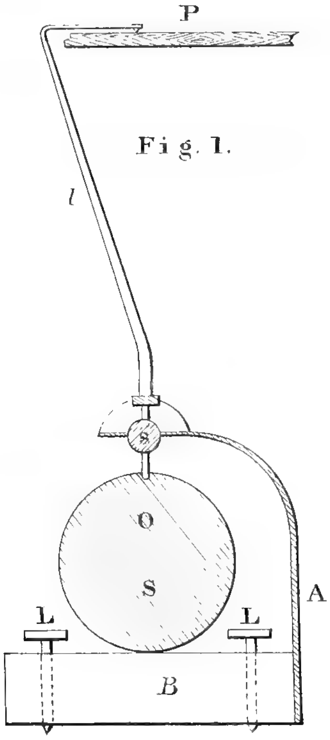

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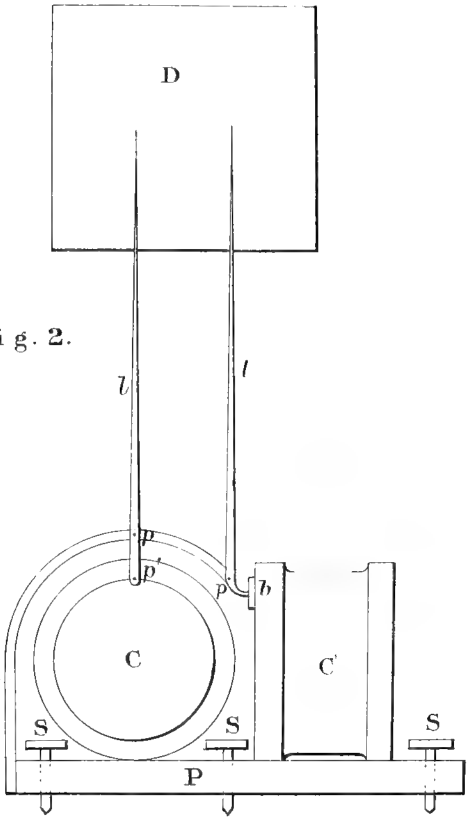

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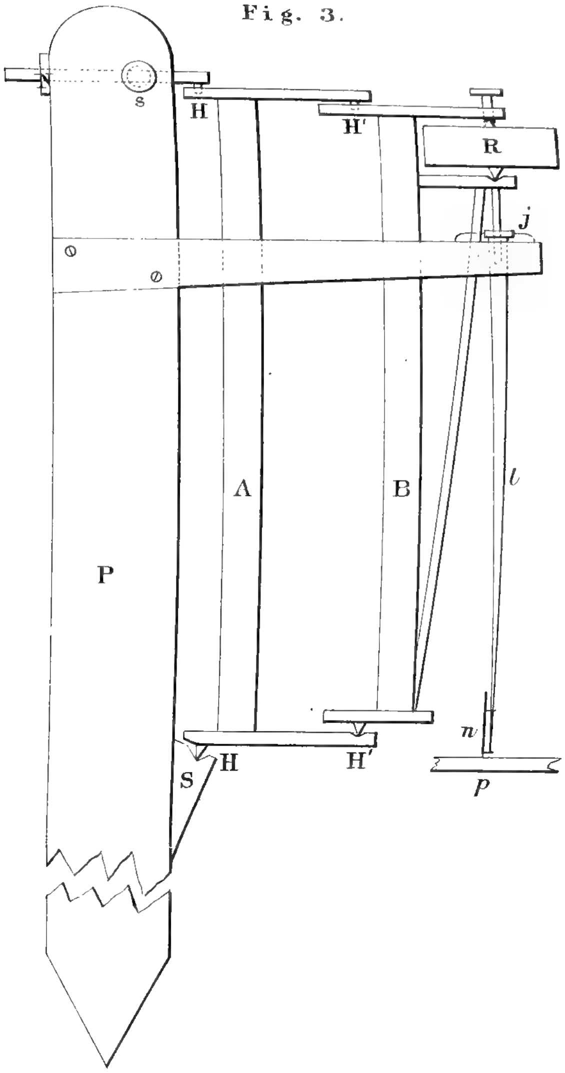

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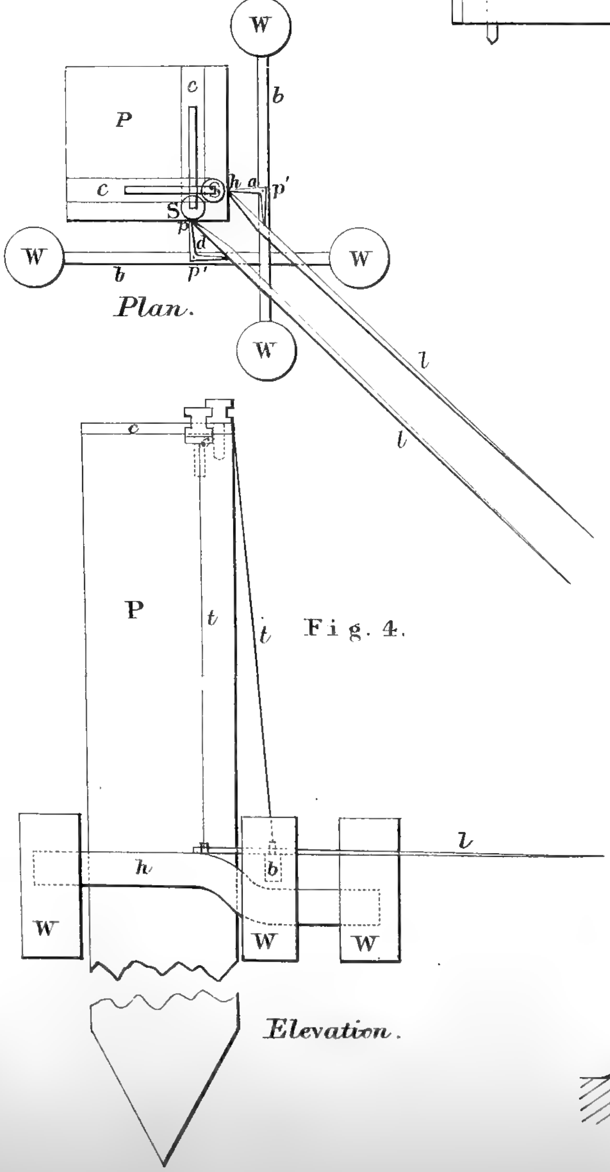

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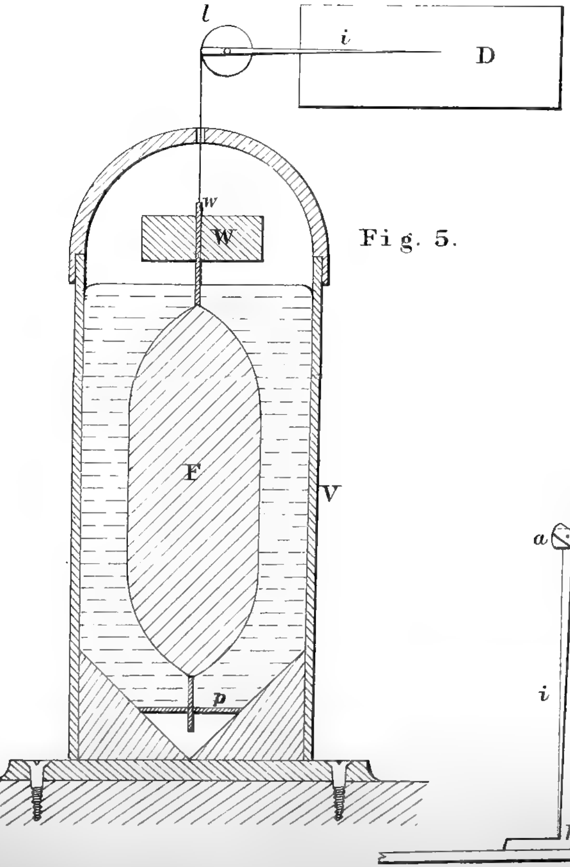

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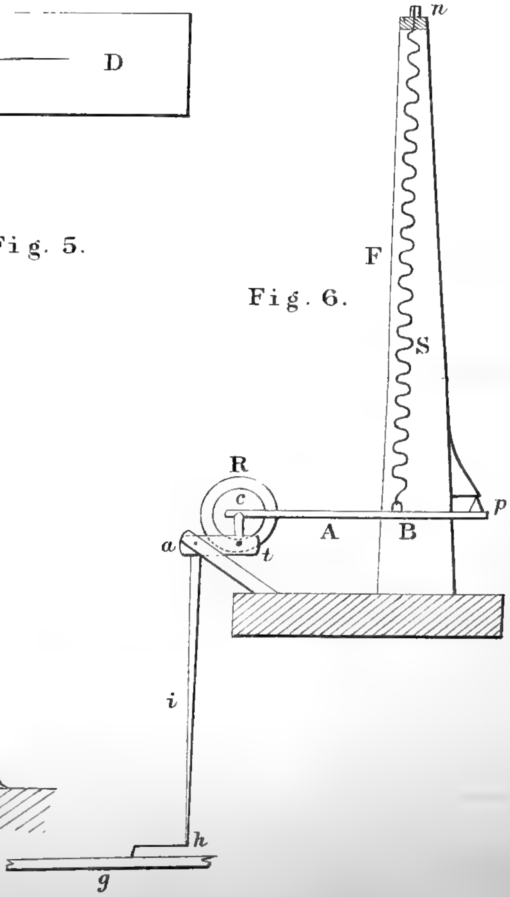

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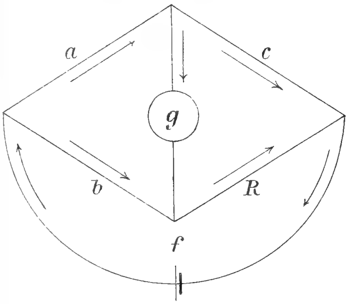

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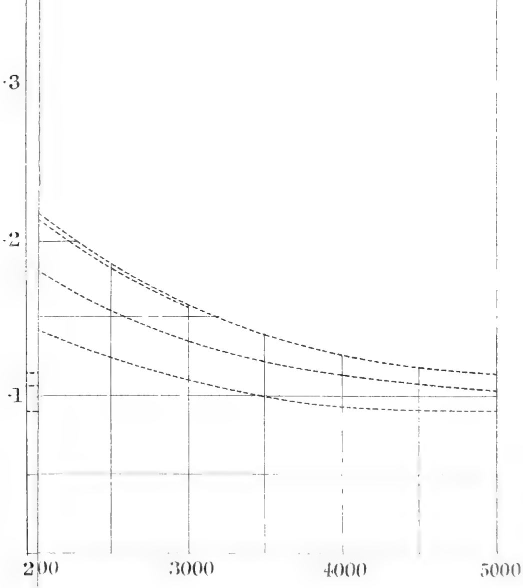
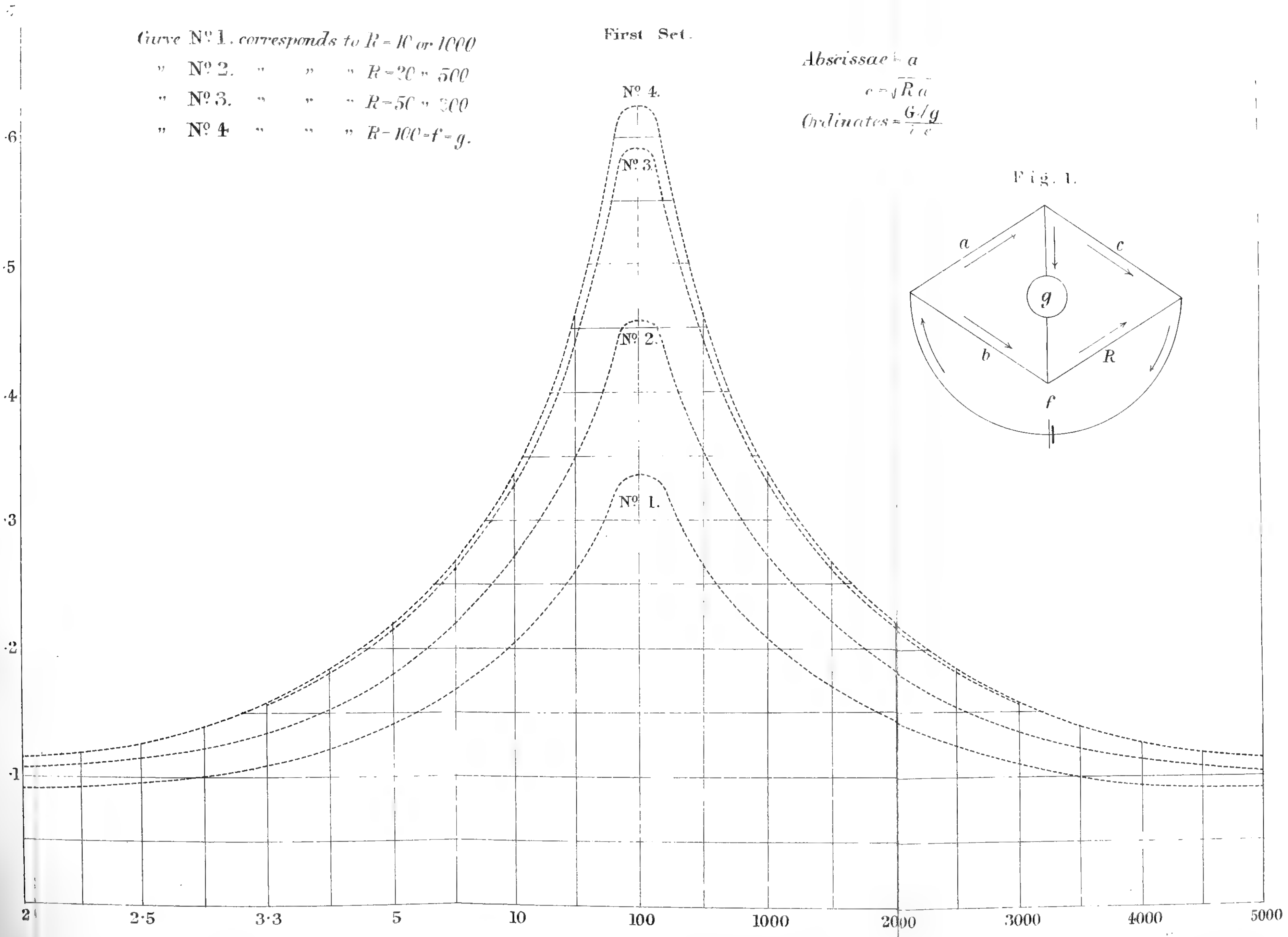


CURVES SHEWING THE EFFECT OF USING DIFFERENT RESISTANCES IN THE WHEATSTONE'S BRIDGE.



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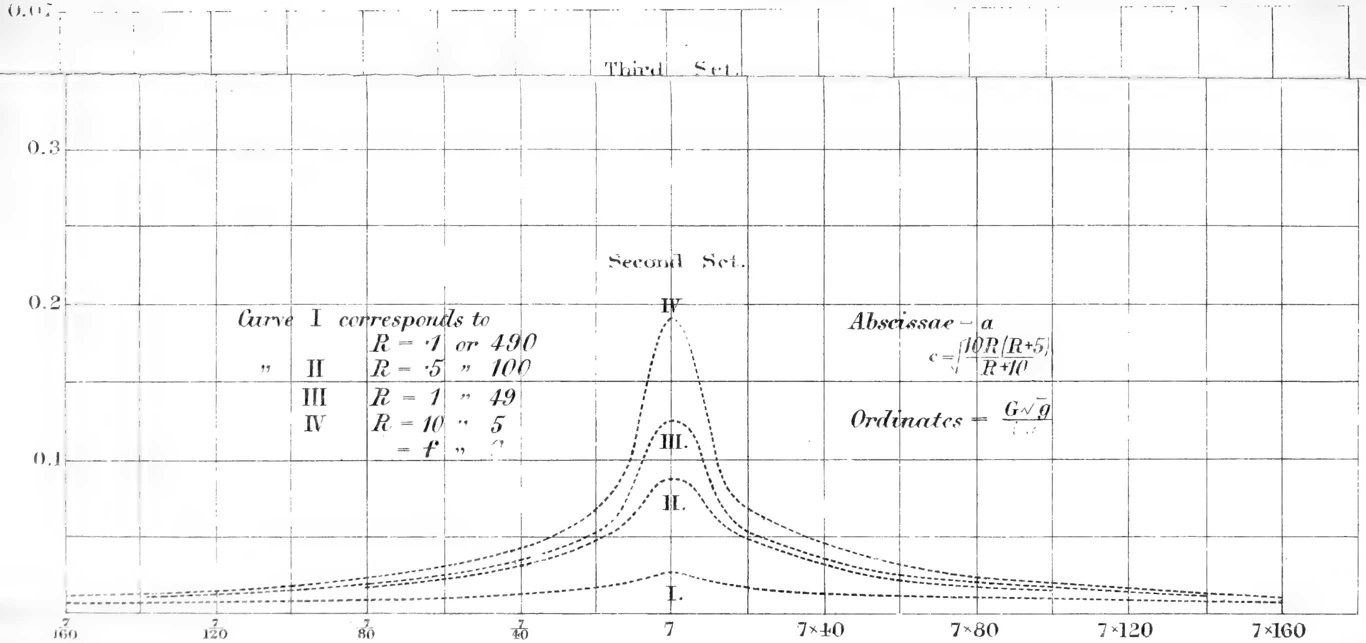

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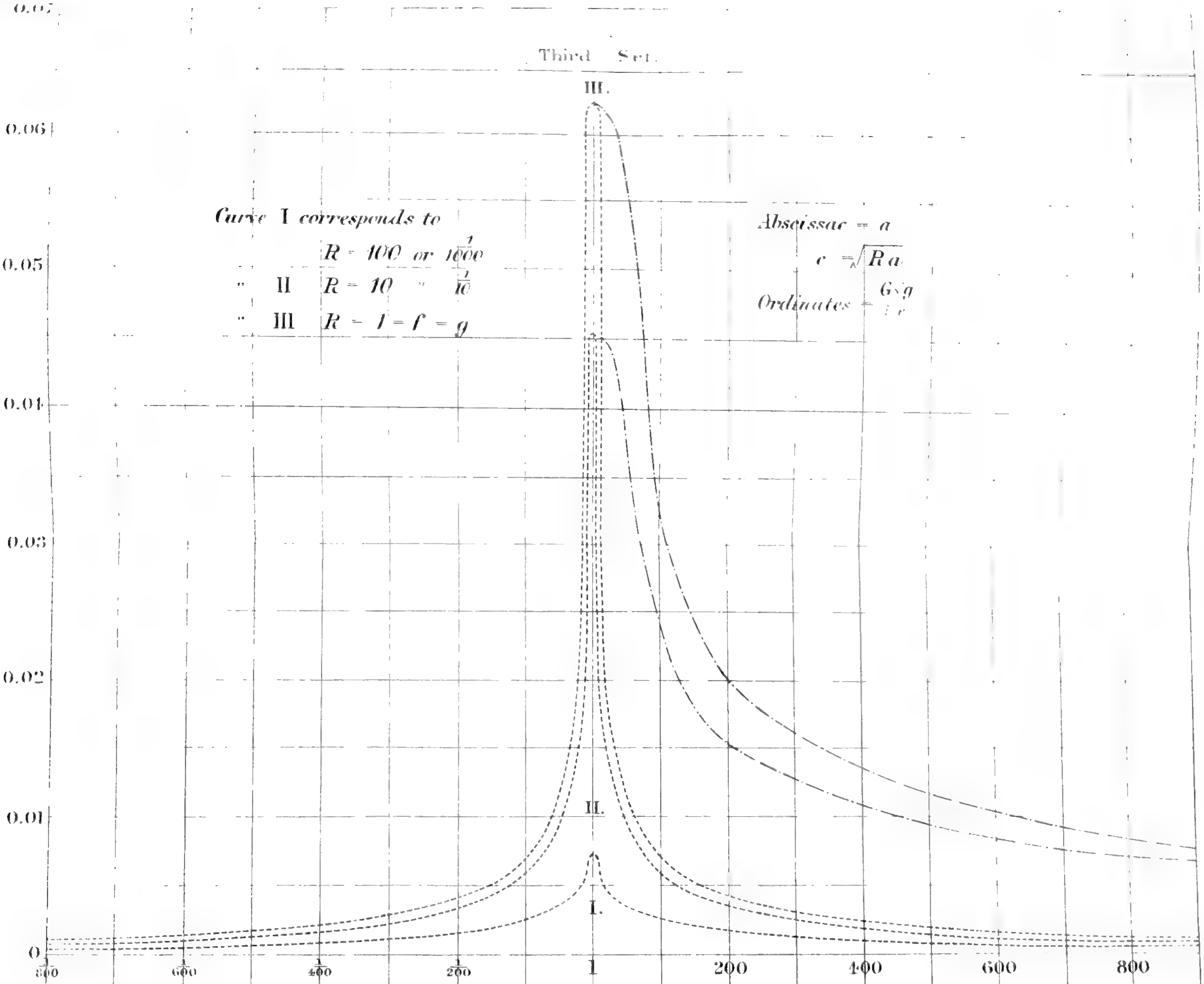

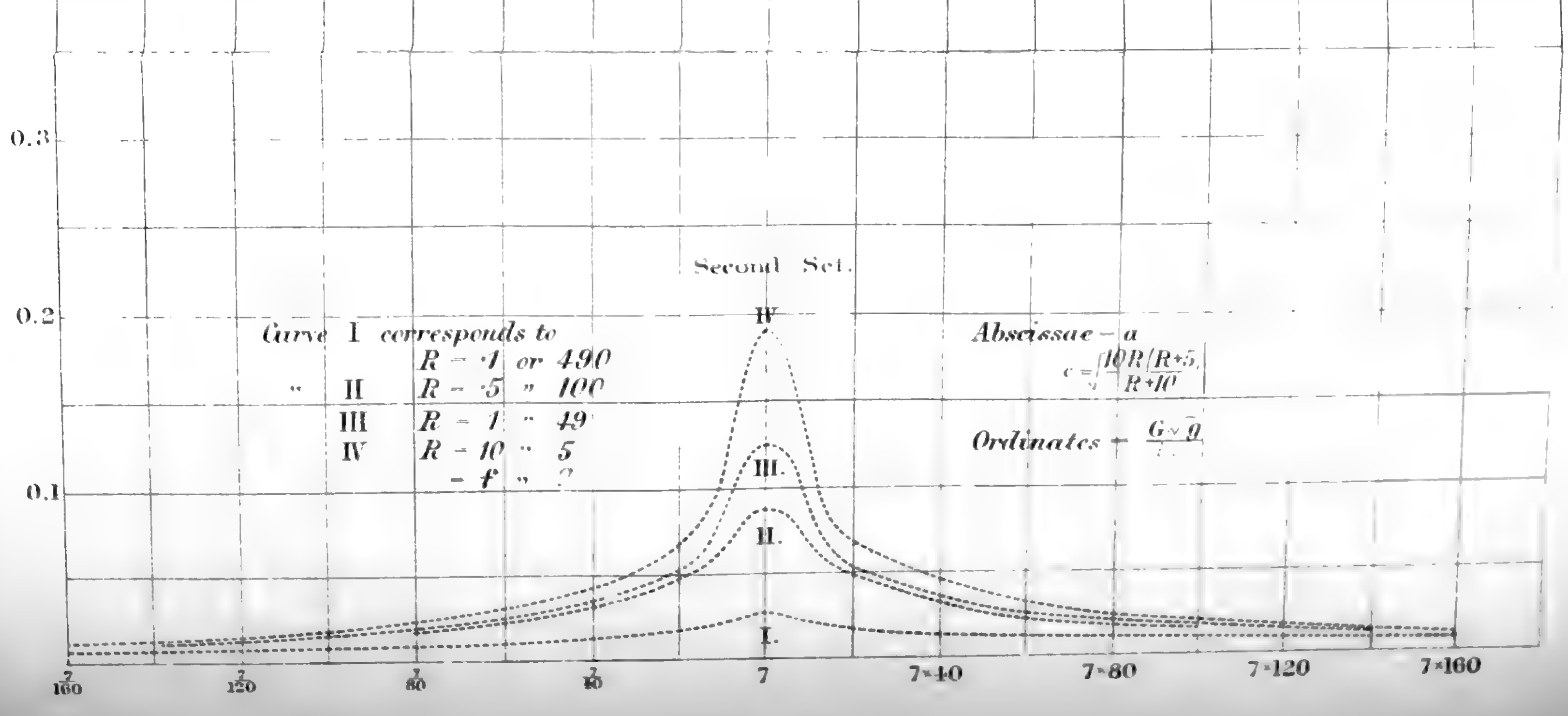

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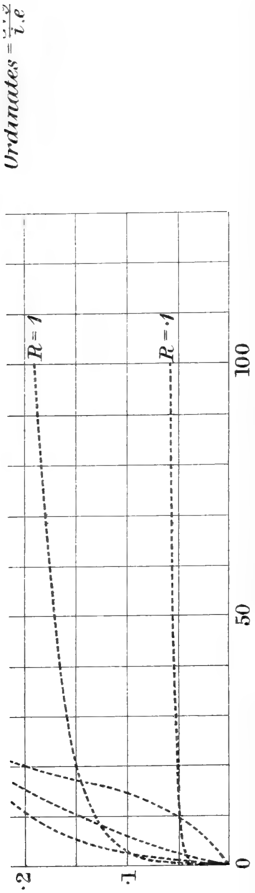

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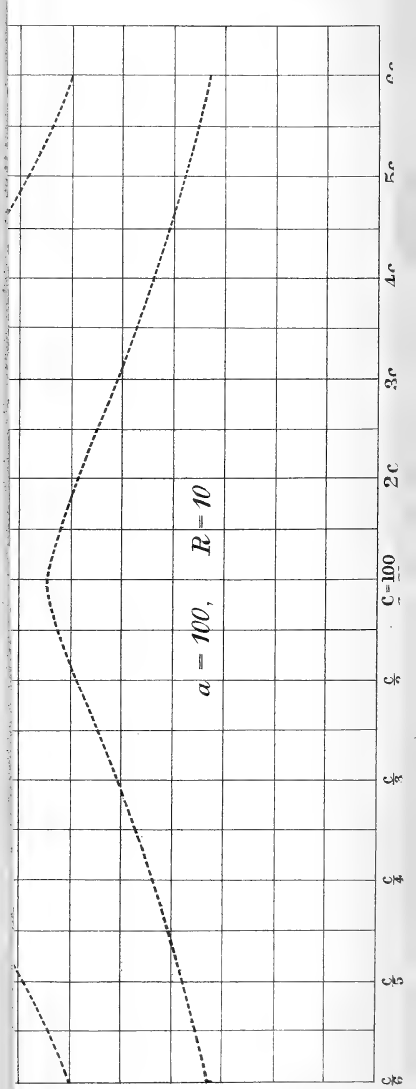

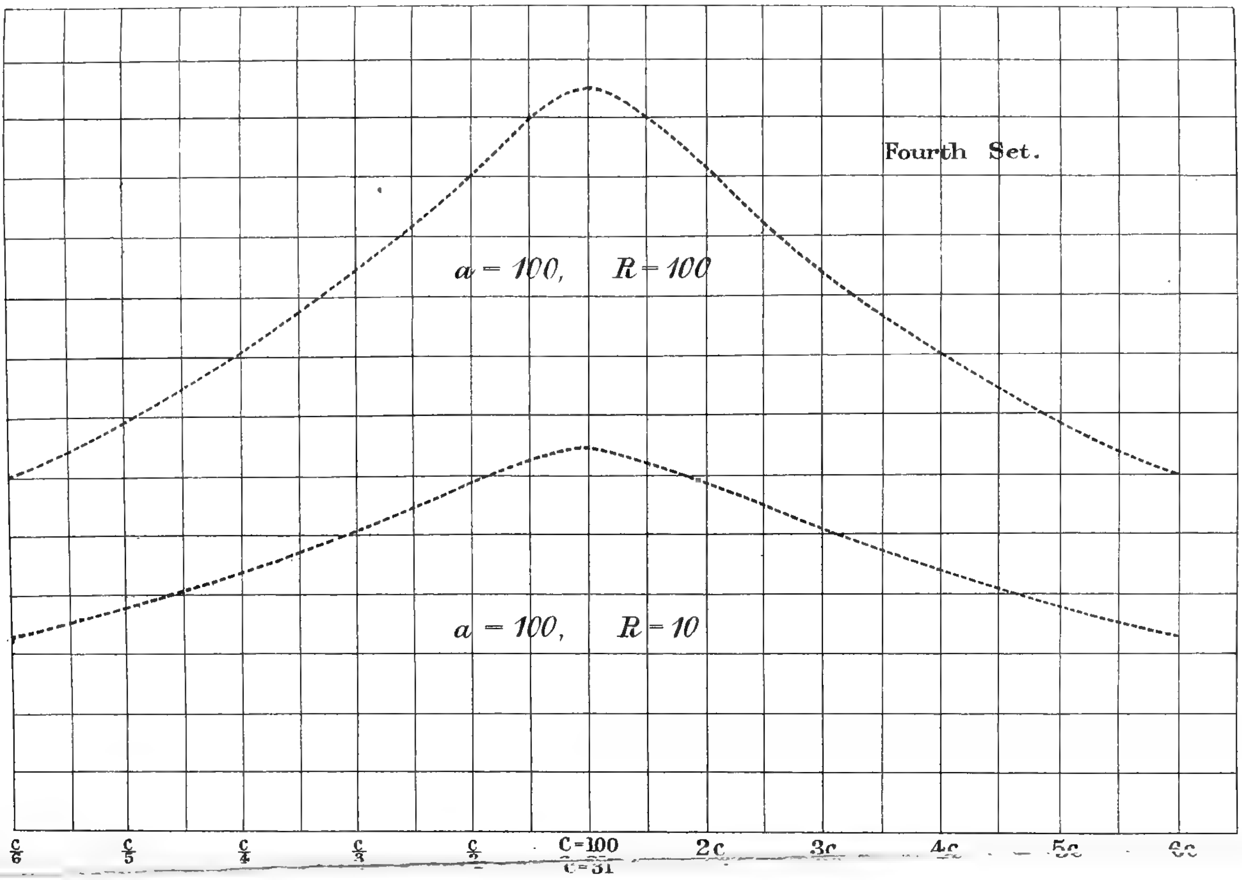

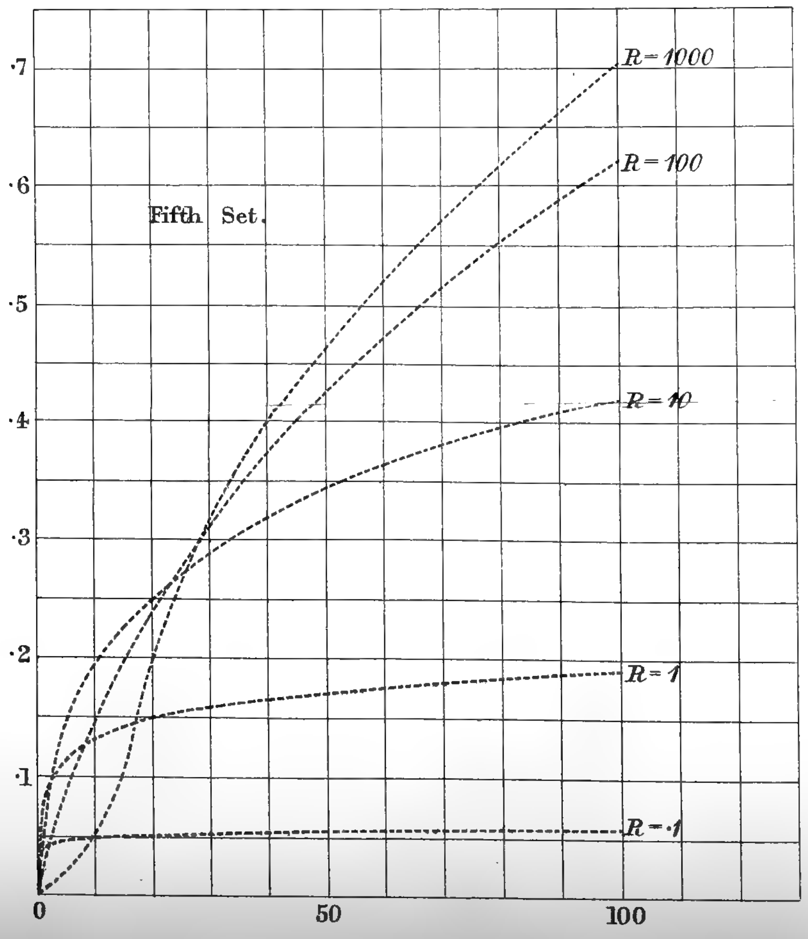

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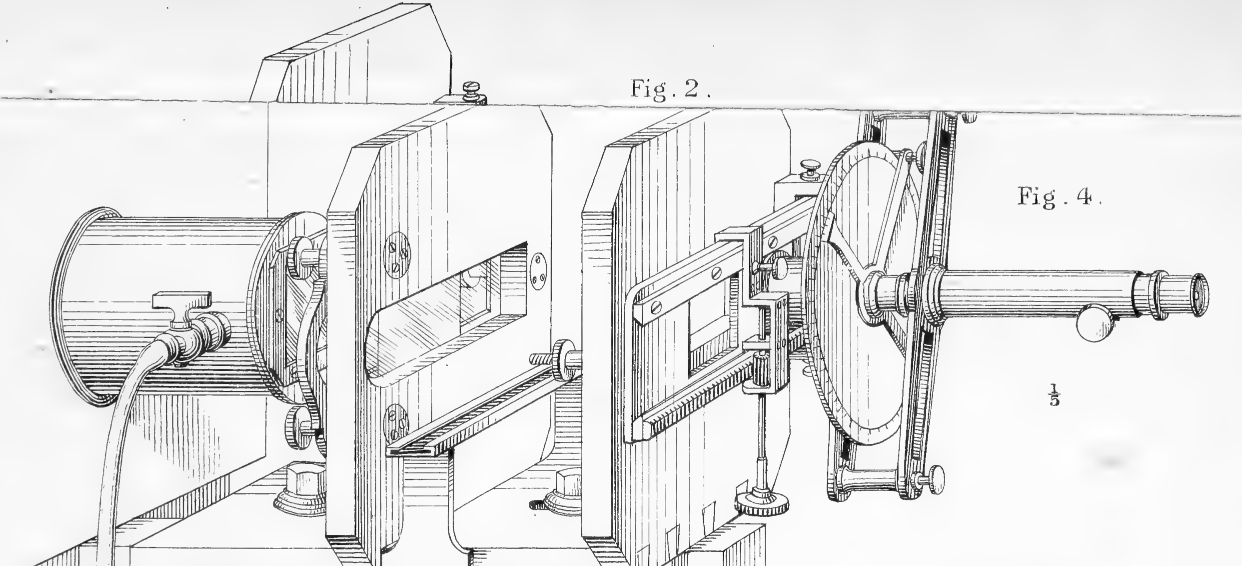

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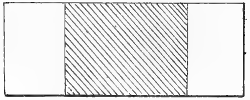

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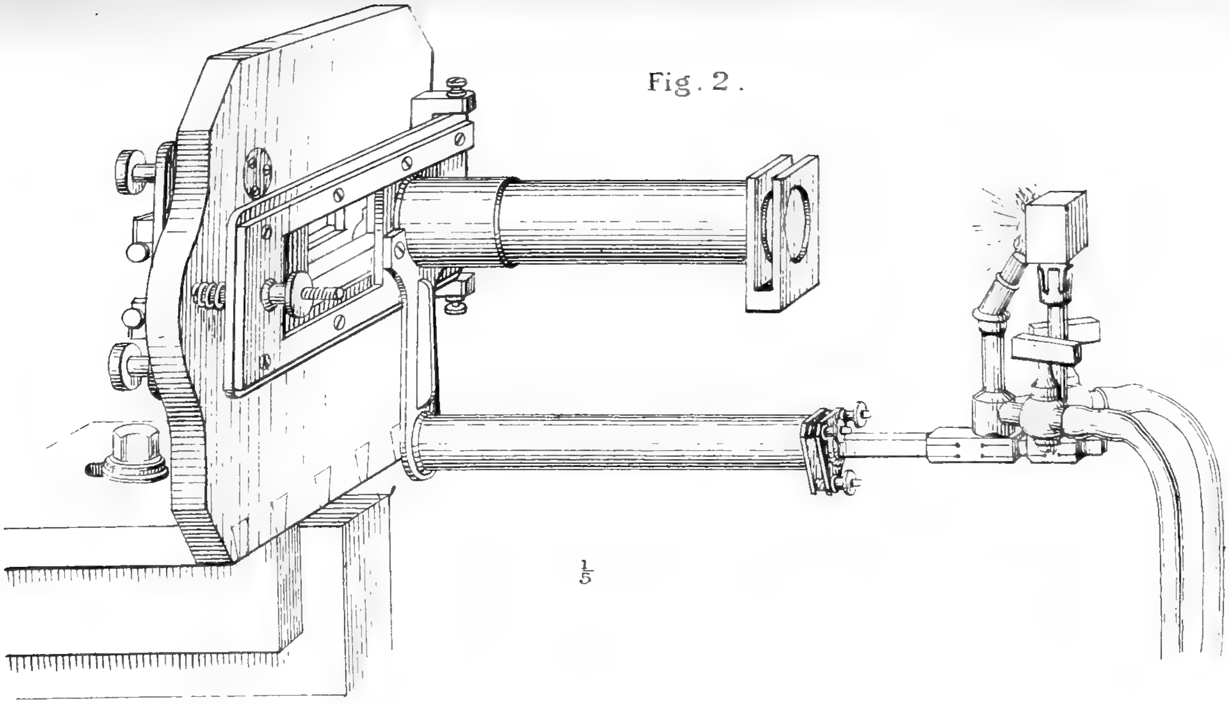

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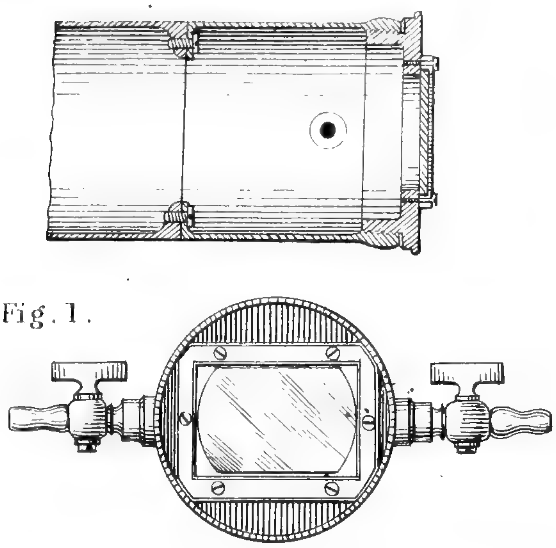

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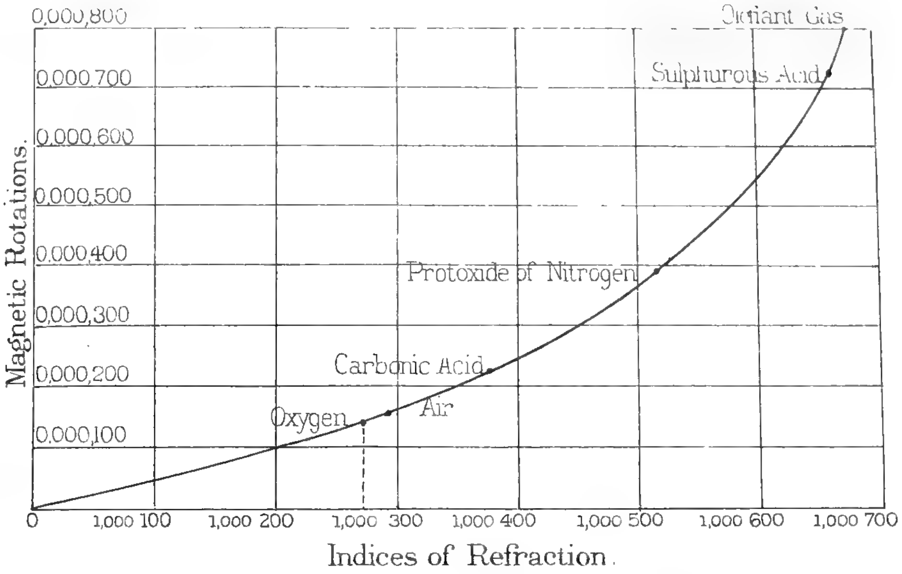

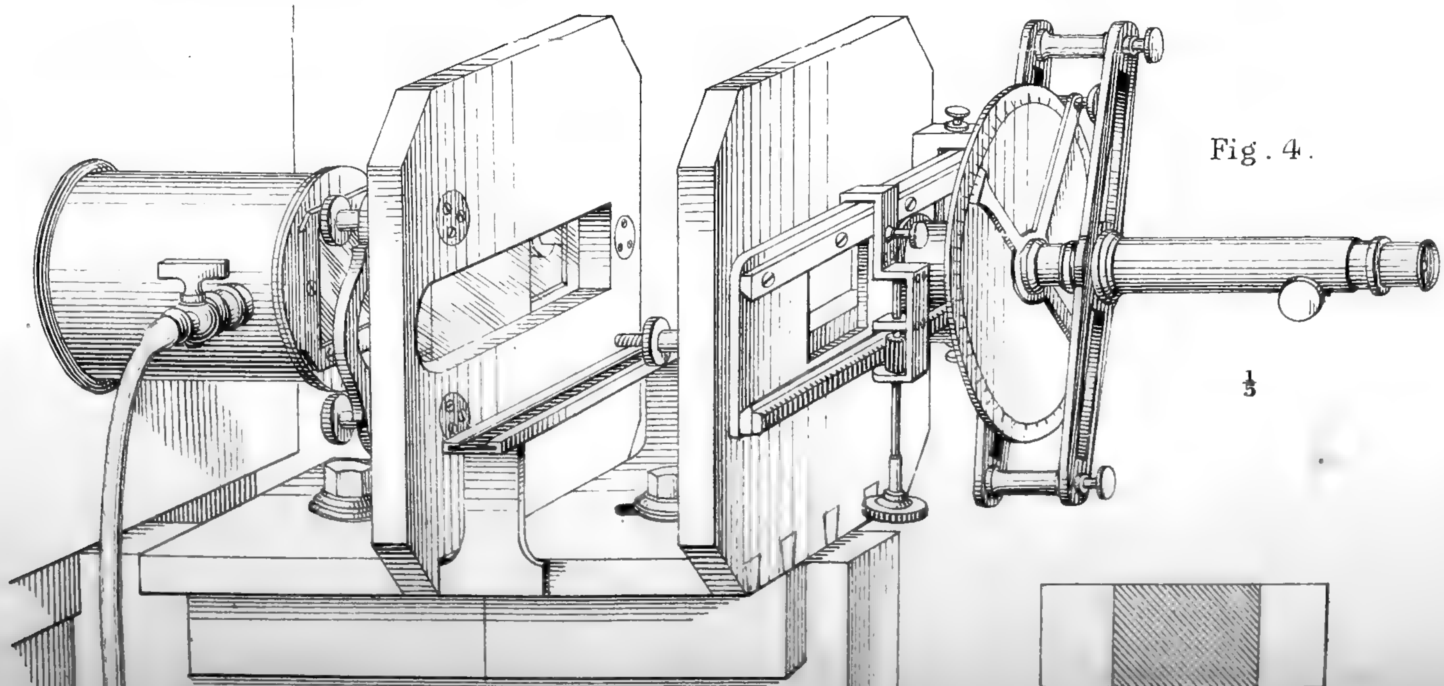

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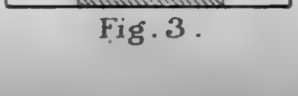

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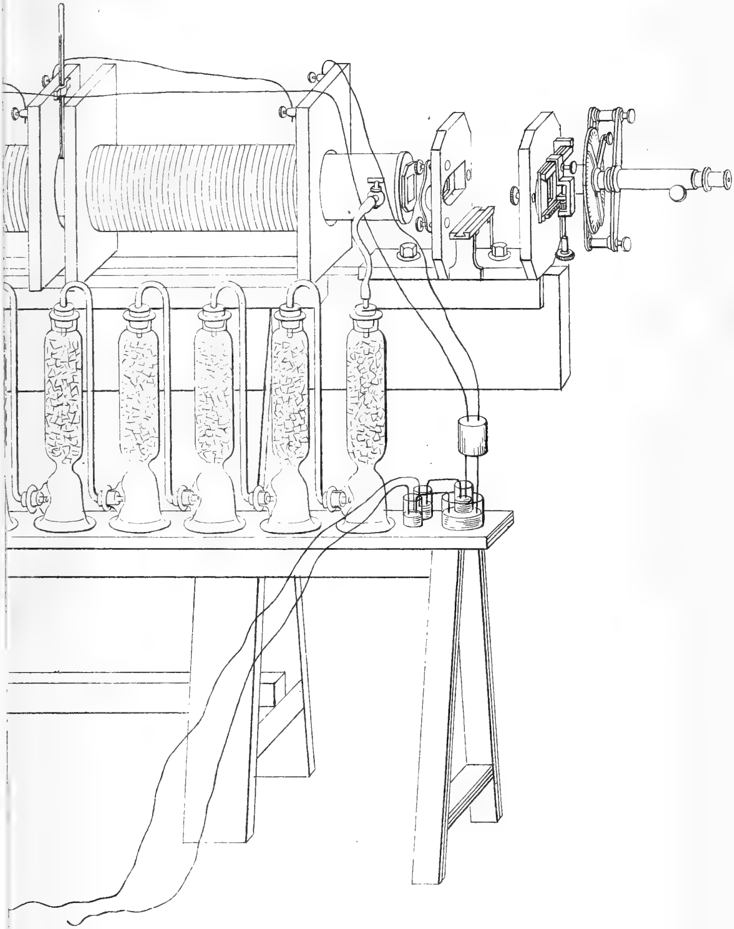

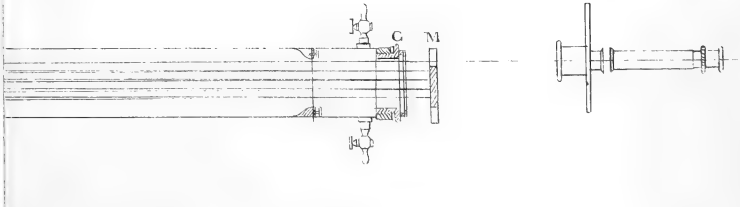
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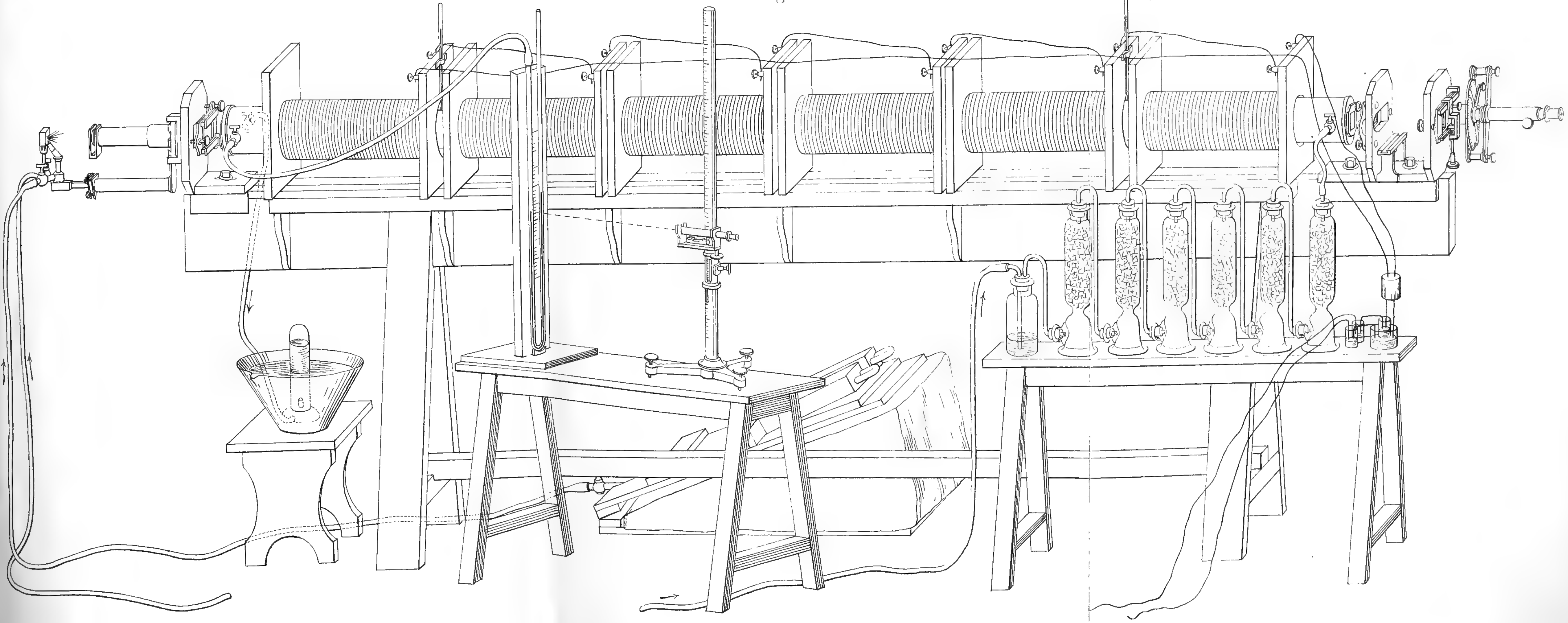

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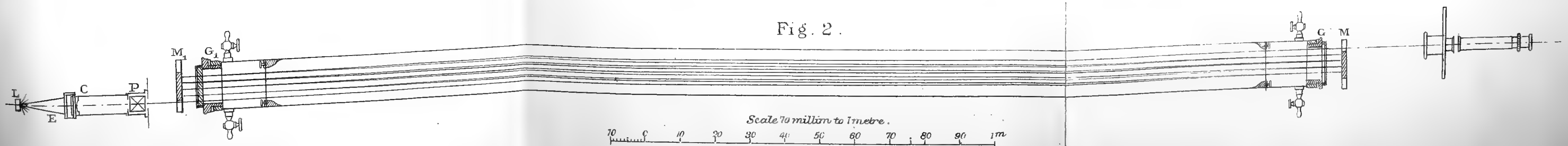

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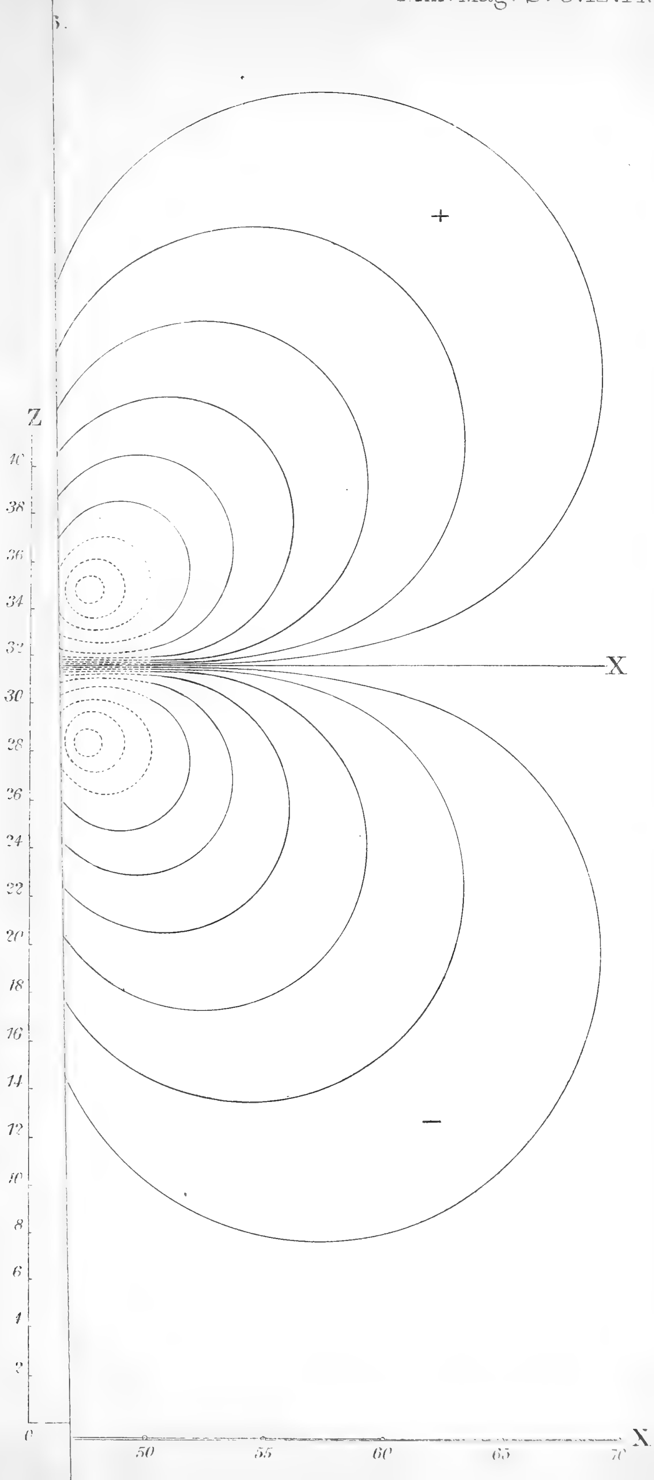
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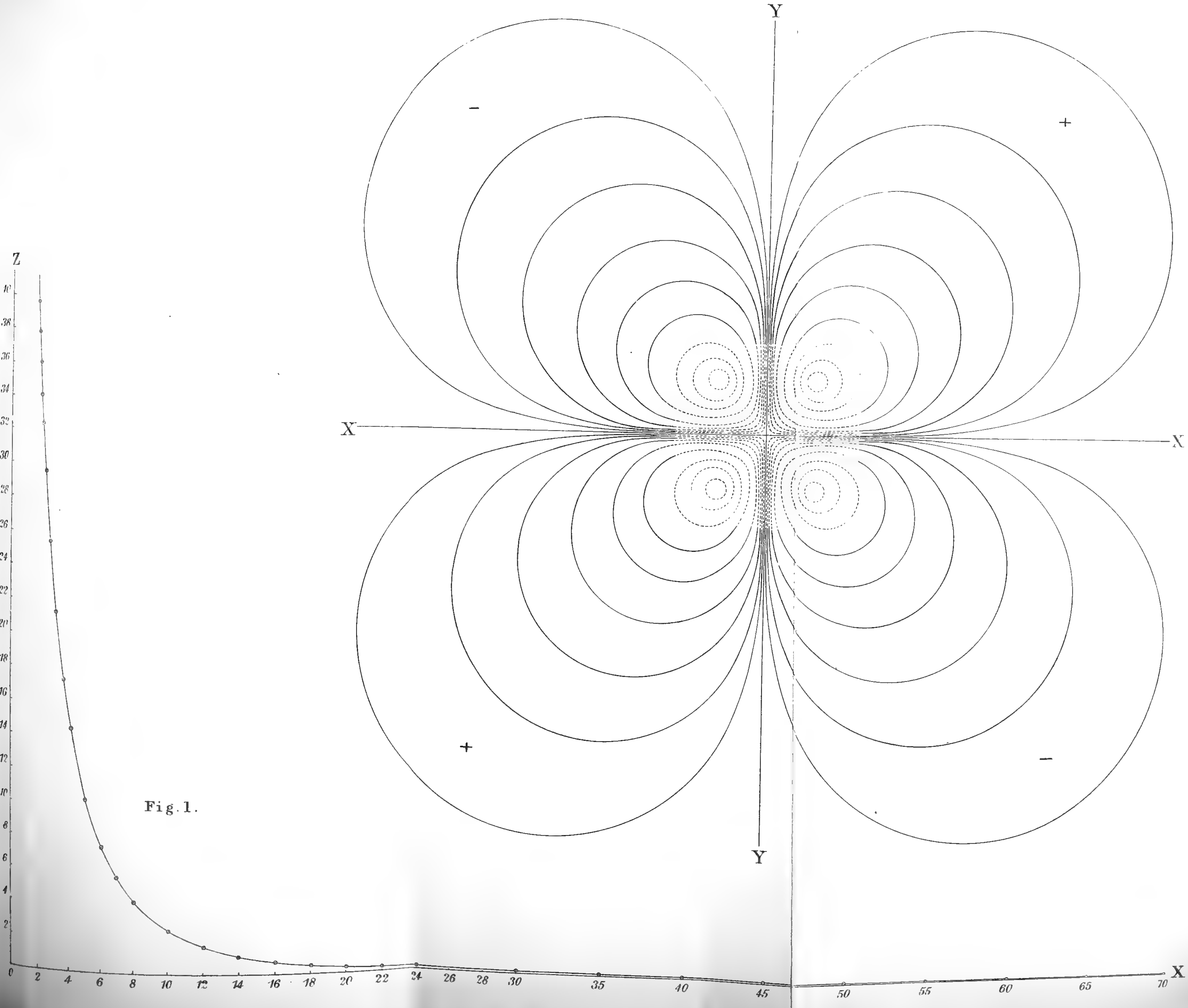

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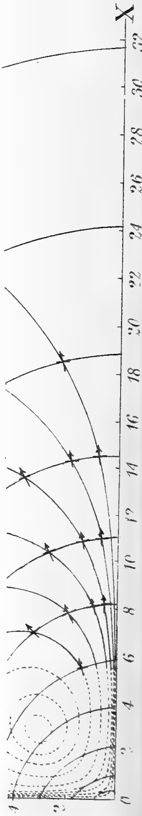
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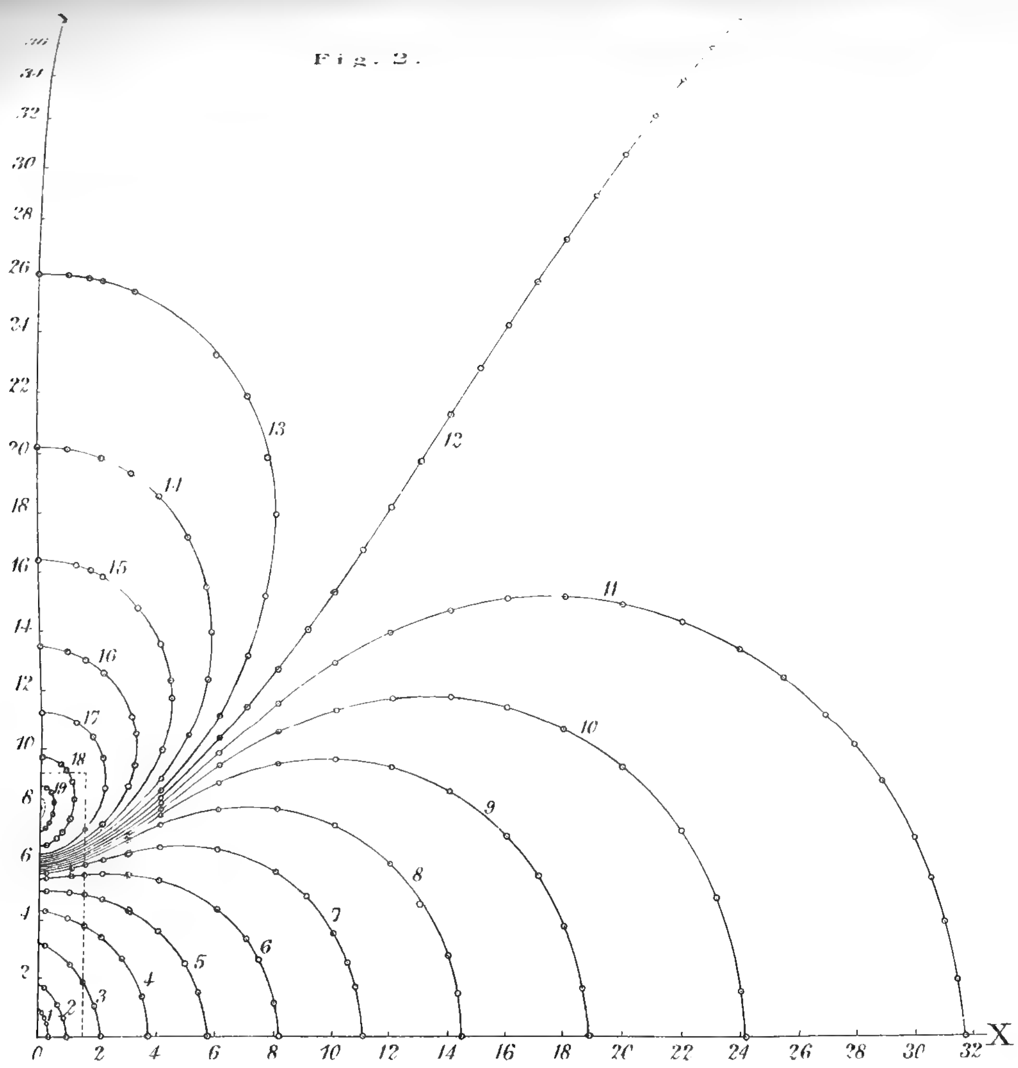

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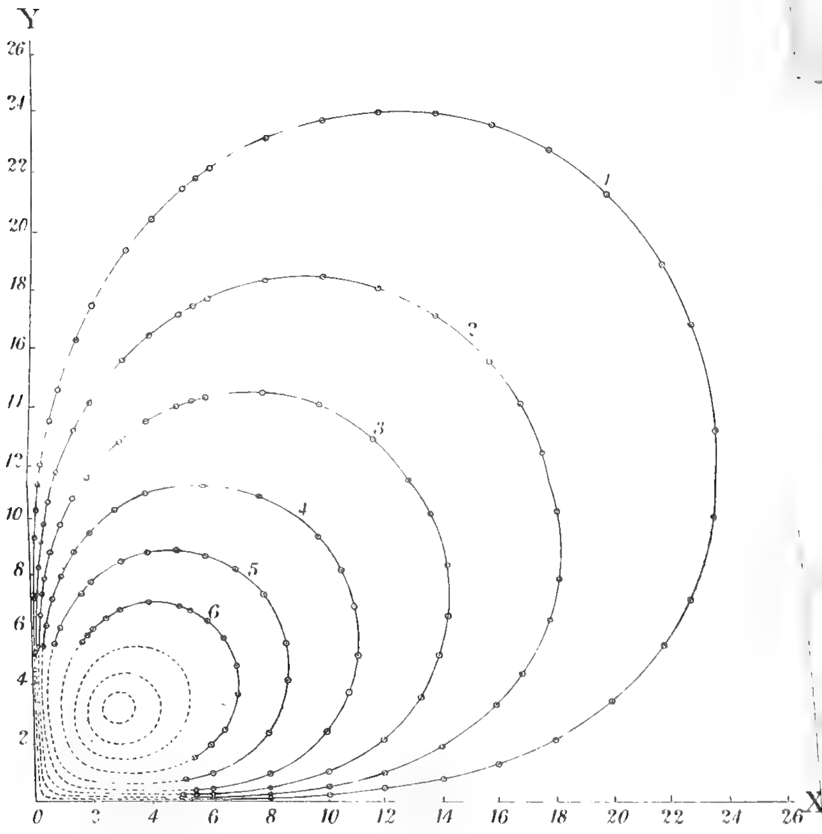

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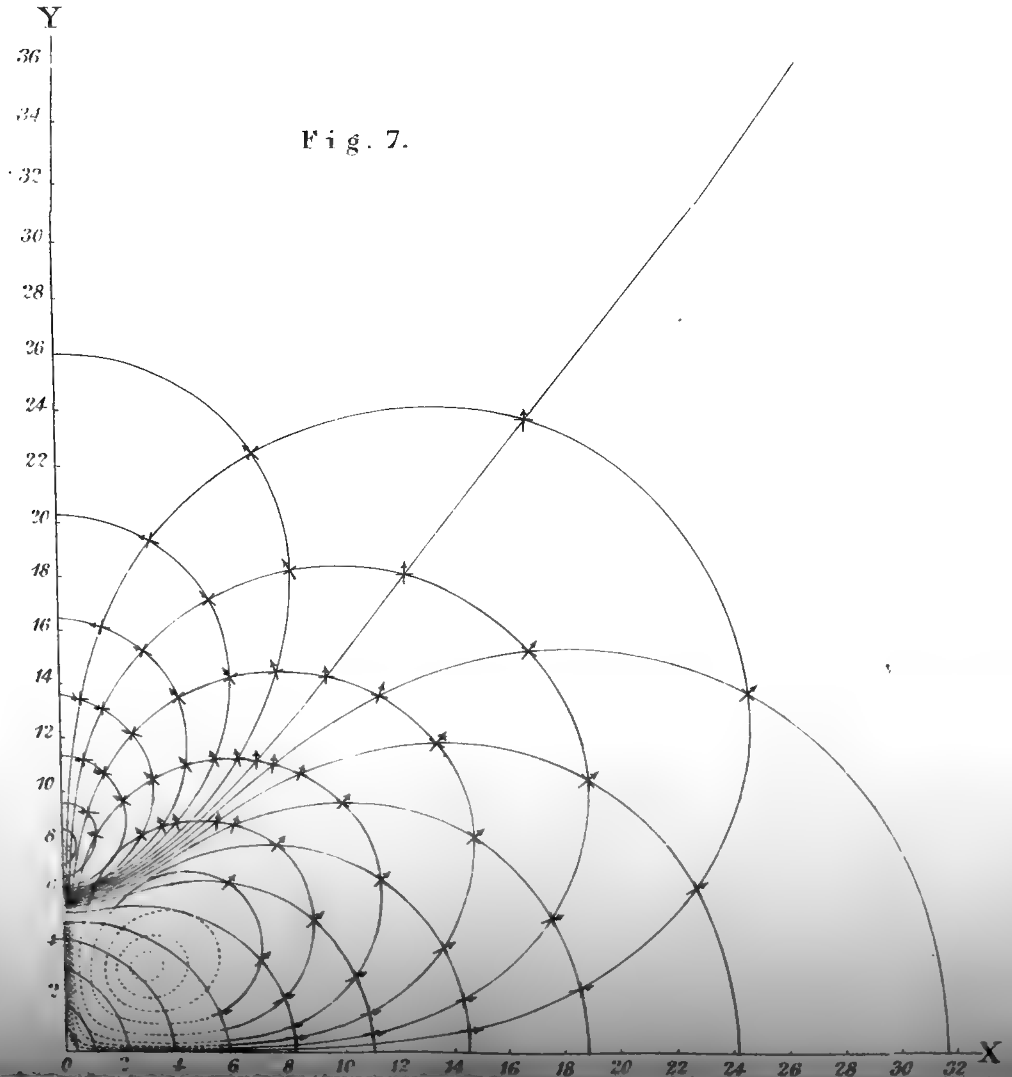

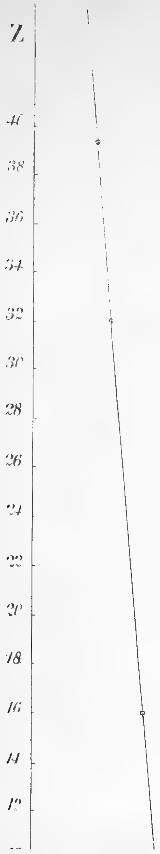

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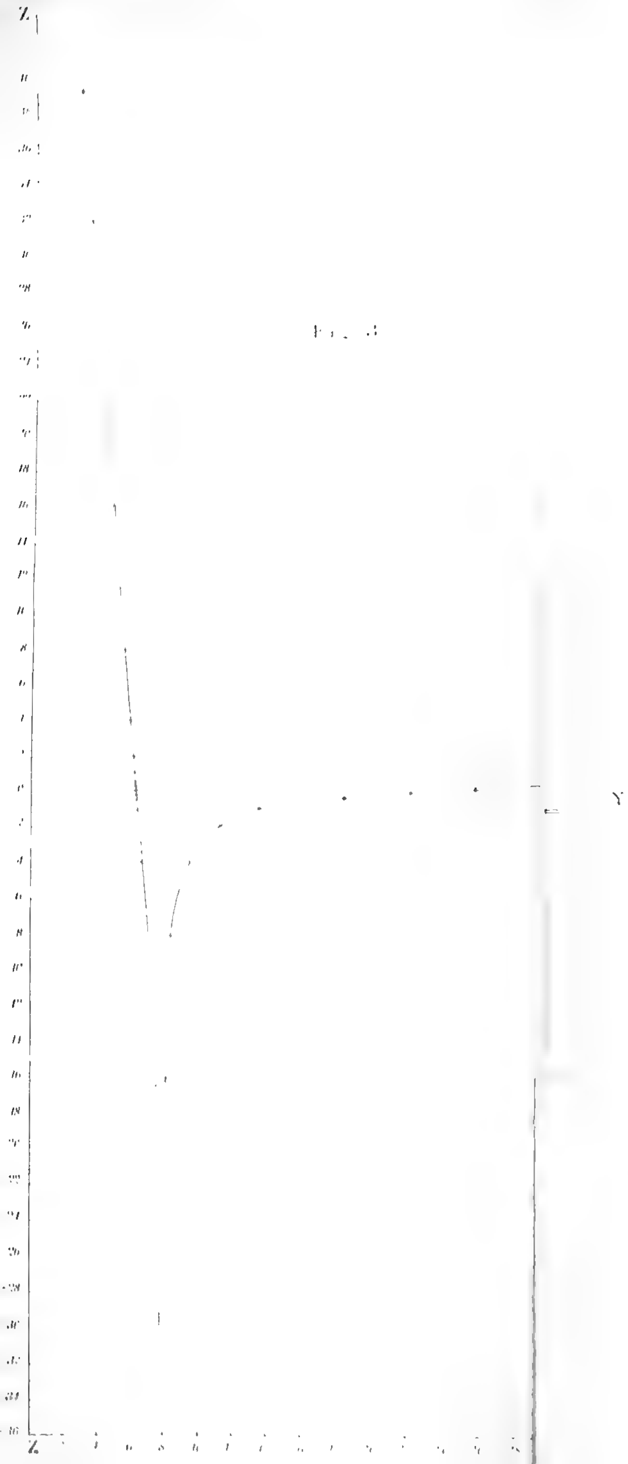

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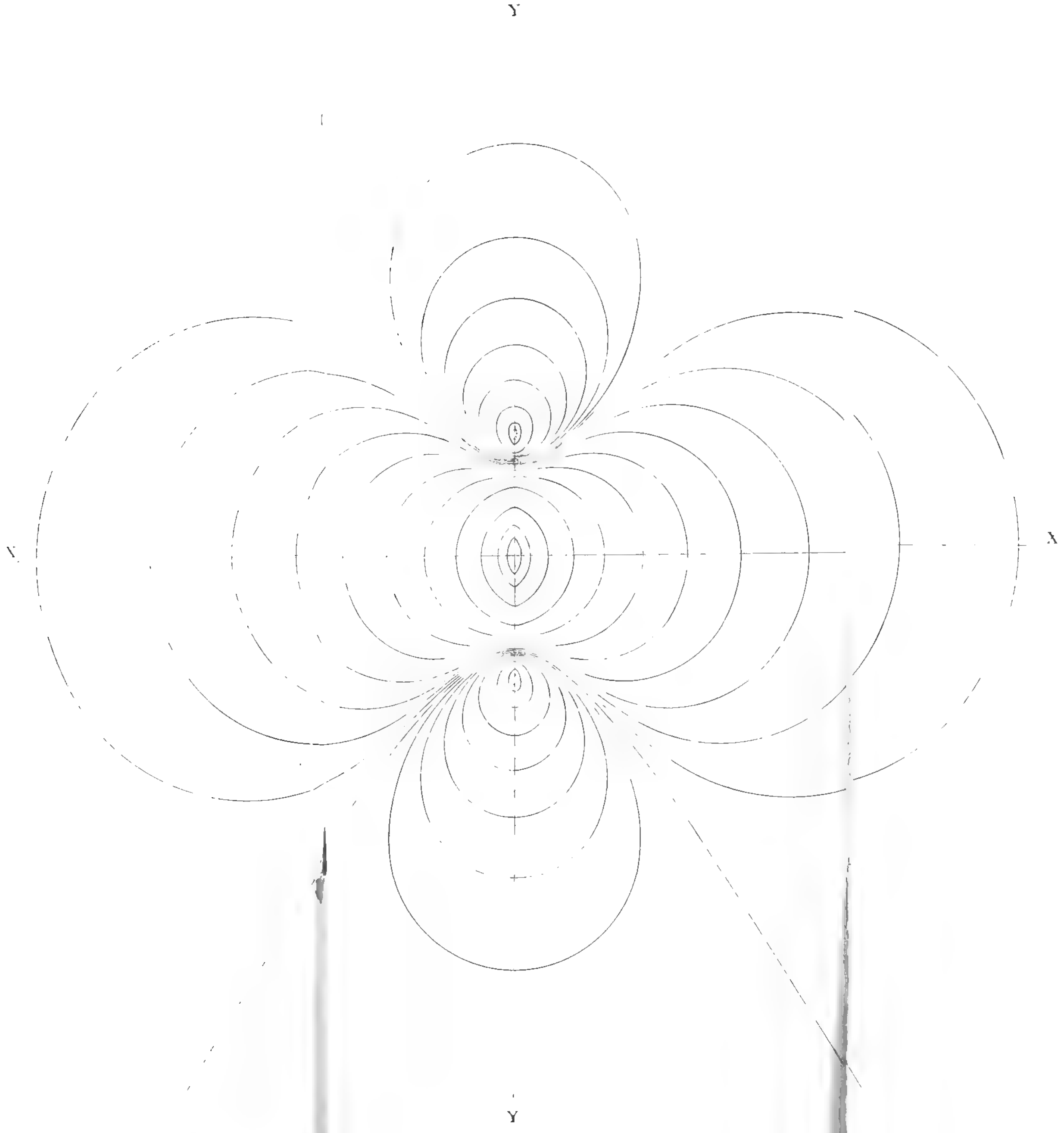

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Errata in No. 70 (June 1881).

Page 415, line 10; insert $1\frac{1}{2}$ in blank space.

Page 432, line 9, for $C_1 : C$ read $C : C$
— — 18, for $C_1 : C$ read C_1
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Errata in No. 74 (September 1881).

Page 189, art. 4, line 2, for x^n read x^a
" " " 4, for tz' read tz
Page 192, art. 17, line 4, *dele* the initial "3"
" 195, art. 29, line 4, for $(1+x^3)$ read $(1+x^3)^2$
Pages 189, 192, 196, arts 1, 15, 36, *for* vol. xiii. *read* vol. xii. [The XIII. at foot of No. 170 of the Proc. L. M. S. is wrong.]

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Errata in No. 75 (October 1881).

Page 301. In the title of Mr. A. Gray's letter, *for* "Number of Electrostatic Units in the Electromagnetic Unit of Electromotive Force," *read* "Number of Electromagnetic Units in the Electrostatic Unit of Electromotive Force."

Page 304, lines 9 to 13. In the values of R tan *a* given in the quotation from Mr. Shida's report, *insert* the factor 10^9.

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