

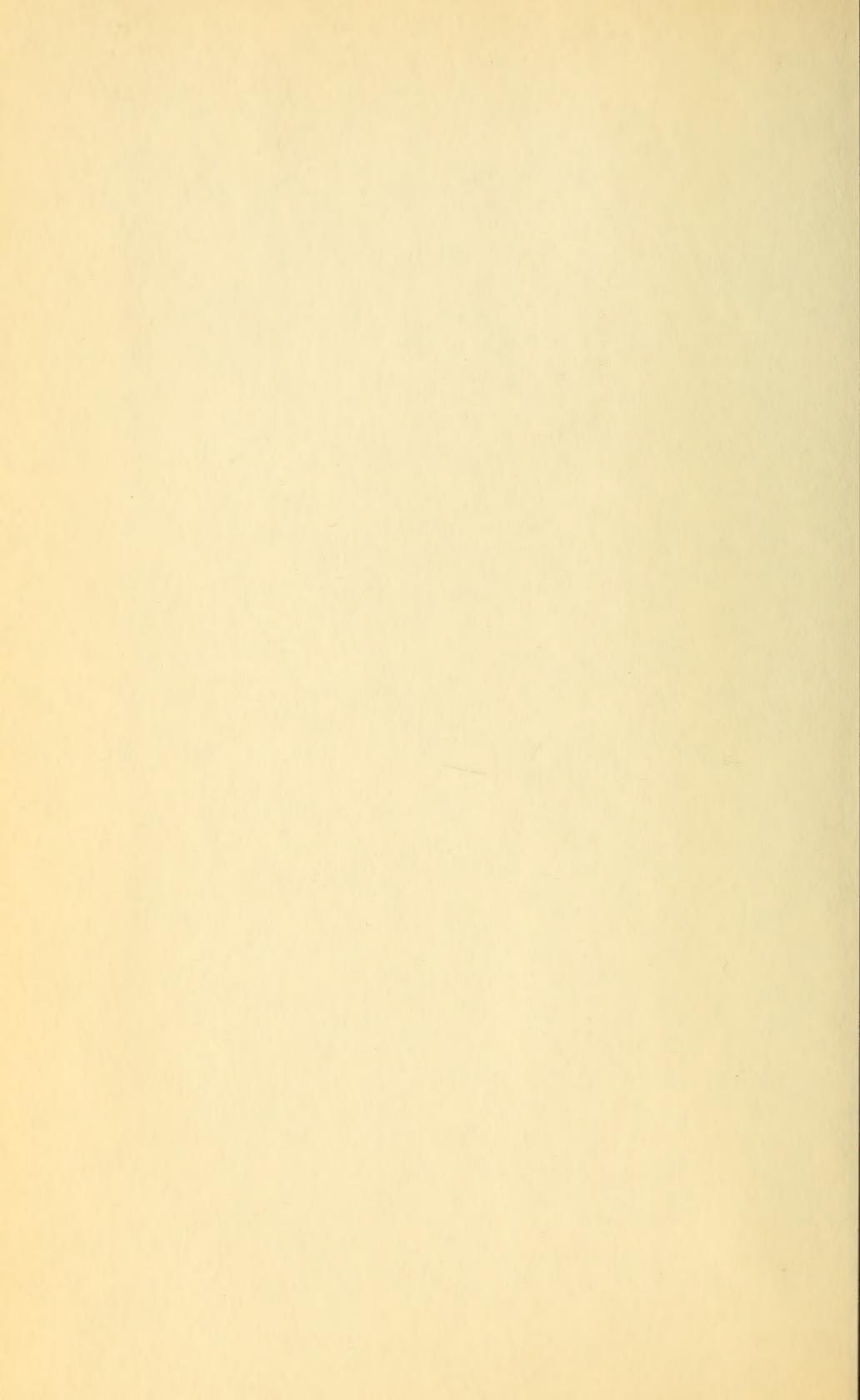
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THE
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PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

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JOHN TYNDALL, F.R.S. &c.

“Nec araneorum 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.*

VOL. XXV.—FOURTH SERIES.

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THE
LONDON, BIRMINGHAM AND GLASGOW
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE
"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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- II. III. IV. Illustrative of Prof. Maskelyne and Dr. Lang's Mineralogical Notes.
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- VI. VII. VIII. IX. Illustrative of Prof. Maskelyne and Dr. Lang's Mineralogical Notes.

ERRATA.

- Page 16, line 8, and *passim*, for objective tube read stage.
- 16, — 17, for bright inscription on dark ground read dark inscription on bright ground.
- 16, — 42, for rectangular read perpendicular.
- 17, — 16 for objective stand read stage, and for objective holder read clamping-piece.
- 17, — 36, for is in read must be placed at.
- 19, — 13, for observed read absorbed.
- 19, — 18, for horizontally read perpendicularly.
- 20, — 23, for will read does.
- 23, — 21, read from the conductor of one electrical machine to another, observed directly under the microscope, gives, &c.
- 32, in equation (5), for 770 millims. read 760 millims.
- 33, line 7 from the bottom, for theoretically read theoretically.
- 36, Table II., heading of third column from the right, for 0.0205 read .00205.
- 165, line 13, for tubes read tube.
- 166, last line, for $\frac{1000P\sigma}{P_0}$ read $\frac{P\sigma}{1000P_0}$.
- 170, line 2, for $A + w$ read $X + w$.
- 293, — 10, for $(\alpha + 1)$ read $(\beta + 1)$.
- 293, — 19, for $\frac{V_0}{V_1}$ read $1 : \frac{V_0}{V_1}$.
- 293, last line but one, last word, read arithmetical.
- 294, line 3, for $2p_0$ read $2p_0'$.
- 297, — 2, for v_a read p_a .

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JANUARY 1863.

I. *On the Secular Cooling of the Earth.*

By Professor WILLIAM THOMSON, LL.D., F.R.S., F.R.S.E.*

[With a Plate.]

1. **F**OR eighteen years it has pressed on my mind, that essential principles of thermo-dynamics have been overlooked by those geologists who uncompromisingly oppose all paroxysmal hypotheses, and maintain not only that we have examples now before us, on the earth, of all the different actions by which its crust has been modified in geological history, but that these actions have never, or have not on the whole, been more violent in past time than they are at present.

2. It is quite certain the solar system cannot have gone on, even as at present, for a few hundred thousand or a few million years without the irrevocable loss (by dissipation, not by *annihilation*) of a very considerable proportion of the entire energy initially in store for sun heat, and for plutonic action. It is quite certain that the whole store of energy in the solar system has been greater in all past time than at present; but it is conceivable that the rate at which it has been drawn upon and dissipated, whether by solar radiation, or by volcanic action in the earth or other dark bodies of the system, may have been nearly equable, or may even have been less rapid, in certain periods of the past. But it is far more probable that the secular rate of dissipation has been in some direct proportion to the total amount of energy in store, at any time after the commencement of the present order of things, and has been therefore very slowly diminishing from age to age.

3. I have endeavoured to prove this for the sun's heat, in an

* From the Transactions of the Royal Society of Edinburgh, vol. xxiii. part 1. Communicated by the Author.

article recently published in 'Macmillan's Magazine'*, where I have shown that most probably the sun was sensibly hotter a million years ago than he is now. Hence geological speculations assuming somewhat greater extremes of heat, more violent storms and floods, more luxuriant vegetation, and hardier and coarser-grained plants and animals in remote antiquity, are more probable than those of the extreme quietist or "Uniformitarian" school. A "middle path," not generally safest in scientific speculation, seems to be so in this case. It is probable that hypotheses of grand catastrophes destroying all life from the earth, and ruining its whole surface at once, are greatly in error; it is impossible that hypotheses assuming an equability of sun and storms for 1,000,000 years can be wholly true.

4. Fourier's mathematical theory of the conduction of heat is a beautiful working out of a particular case belonging to the general doctrine of the "Dissipation of Energy"†. A characteristic of the practical solutions it presents is, that in each case a distribution of temperature, becoming gradually equalized through an unlimited future, is expressed as a function of the time, which is infinitely divergent for all times longer past than a definite determinable epoch. The distribution of heat at such an epoch is essentially *initial*, that is to say, it cannot result from any previous condition of matter by natural processes. It is, then, well called an "*arbitrary* initial distribution of heat," in Fourier's great mathematical poem, because it could only be realized by the action of a power able to modify the laws of dead matter. In an article published about nineteen years ago in the 'Cambridge Mathematical Journal'‡, I gave the mathematical criterion for an essentially initial distribution; and in an inaugural essay, "De Motu Caloris per Terræ Corpus," read before the Faculty of the University of Glasgow in 1846, I suggested, as an application of these principles, that a perfectly complete geothermic survey would give us data for determining an initial epoch in the problem of terrestrial conduction. At the meeting of the British Association in Glasgow in 1855, I urged that special geothermic surveys should be made for the purpose of estimating absolute dates in geology; and I pointed out some cases, especially that of the salt-spring borings at Creuznach in Rhenish Prussia, in which eruptions of basaltic rock seem to leave traces of their igneous origin in residual heat§. I hope

* March 1862.

† Proceedings of the Royal Society of Edinburgh, February 1852, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy." Also, "On the Restoration of Energy in an Unequally Heated Space," Phil. Mag. S. 4. vol. v. p. 102.

‡ February 1844, "Note on Certain Points in the Theory of Heat."

§ See British Association Report of 1855, Glasgow Meeting.

this suggestion may yet be taken up, and may prove to some extent useful; but the disturbing influences affecting underground temperature, as Professor Phillips has well shown in a recent inaugural address to the Geological Society, are too great to allow us to expect any very precise or satisfactory results.

5. The chief object of the present communication is to estimate, from the known general increase of temperature in the earth downwards, the date of the first establishment of that "*consistentior status*," which, according to Leibnitz's theory, is the initial date of all geological history.

6. In all parts of the world in which the earth's crust has been examined, at sufficiently great depths to escape large influence of the irregular and of the annual variations of the superficial temperature, a gradually increasing temperature has been found in going deeper. The rate of augmentation (estimated at only $\frac{1}{110}$ -th of a degree Fahr. in some localities, and as much as $\frac{1}{15}$ -th of a degree in others, per foot of descent) has not been observed in a sufficient number of places to establish any fair average estimate for the upper crust of the whole earth. But $\frac{1}{30}$ -th is commonly accepted as a rough mean; or, in other words, it is assumed as a result of observation that there is, on the whole, about 1° F. of elevation of temperature per 50 British feet of descent.

7. The fact that the temperature increases with the depth implies a continual loss of heat from the interior, by conduction outwards through or into the upper crust. Hence, since the upper crust does not become hotter from year to year, there must be a secular loss of heat from the whole earth. It is possible that no cooling may result from this loss of heat, but only an exhaustion of potential energy, which in this case could scarcely be other than chemical affinity between substances forming part of the earth's mass. But it is certain that either the earth is becoming on the whole cooler from age to age, or the heat conducted out is generated in the interior by temporary dynamical (that is, in this case, chemical) action. To suppose, as Lyell, adopting the chemical hypothesis, has done*, that the substances, combining together, may be again separated electrolytically by thermo-electric currents, due to the heat generated by their combination, and thus the chemical action and its heat continued in an endless cycle, violates the principles of natural philosophy in exactly the same manner, and to the same degree, as to believe that a clock constructed with a self-winding movement may fulfil the expectations of its ingenious inventor by going for ever.

8. It must indeed be admitted that many geological writers

* Principles of Geology.

of the "Uniformitarian" school, who in other respects have taken a profoundly philosophical view of their subject, have argued in a most fallacious manner against hypotheses of violent action in past ages. If they had contented themselves with showing that many existing appearances, although suggestive of extreme violence and sudden change, may have been brought about by long-continued action, or by paroxysms not more intense than some of which we have experience within the periods of human history, their position might have been unassailable, and certainly could not have been assailed except by a detailed discussion of their facts. It would be a very wonderful, but not an absolutely incredible result, that volcanic action has never been more violent on the whole than during the last two or three centuries; but it is as certain that there is now less volcanic energy in the whole earth than there was a thousand years ago, as it is that there is less gunpowder in a "Monitor" after she has been seen to discharge shot and shell, whether at a nearly equable rate or not, for five hours without receiving fresh supplies, than there was at the beginning of the action. Yet this truth has been ignored or denied by many of the leading geologists of the present day, because they believe that the facts within their province do not demonstrate greater violence in ancient changes of the earth's surface, or do demonstrate a nearly equable action in all periods.

9. The chemical hypothesis to account for underground heat might be regarded as not improbable, if it was only in isolated localities that the temperature was found to increase with the depth; and, indeed, it can scarcely be doubted that chemical action exercises an appreciable influence (possibly negative, however) on the action of volcanoes; but that there is slow uniform "combustion," "eremacausis," or chemical combination of any kind going on at some great unknown depth under the surface everywhere, and creeping inwards gradually as the chemical affinities in layer after layer are successively saturated, seems extremely improbable, although it cannot be pronounced to be absolutely impossible, or contrary to all analogies in nature. The less hypothetical view, however, that the earth is merely a warm chemically inert body cooling, is clearly to be preferred in the present state of science.

10. Poisson's celebrated hypothesis, that the present underground heat is due to a passage, at some former period, of the solar system throughout hotter stellar regions, cannot provide the circumstances required for a palæontology continuous through that epoch of external heat. For from a mean of values of the conductivity, in terms of the thermal capacity of unit volume, of the earth's crust in three different localities near Edinburgh, which I have deduced from the observations on underground

temperature instituted by Principal Forbes there, I find that, if the supposed transit through a hotter region of space took place between 1250 and 5000 years ago, the temperature of that supposed region must have been from 25° to 50° F. above the present mean temperature of the earth's surface, to account for the present general rate of underground increase of temperature, taken as 1° F. in 50 feet downwards. Human history negatives this supposition. Again, geologists and astronomers will, I presume, admit that the earth cannot, 20,000 years ago, have been in a region of space 100° F. warmer than its present surface. But if the transition from a hot region to a cold region, supposed by Poisson, took place more than 20,000 years ago, the excess of temperature must have been more than 100° F., and must therefore have destroyed animal and vegetable life. Hence, the further back and the hotter we can suppose Poisson's hot region, the better for the geologists who require the longest periods; but the best for their view is Leibnitz's theory, which simply supposes the earth to have been at one time an incandescent liquid, without explaining how it got into that state. If we suppose the temperature of melting rock to be about $10,000^{\circ}$ F. (an extremely high estimate), the consolidation may have taken place 200,000,000 years ago. Or, if we suppose the temperature of melting rock to be 7000° F. (which is more nearly what it is generally assumed to be), we may suppose the consolidation to have taken place 98,000,000 years ago.

11. These estimates are founded on the Fourier solution demonstrated below. The greatest variation we have to make on them, to take into account the differences in the ratios of conductivities to specific heats of the three Edinburgh rocks, is to reduce them to nearly half, or to increase them by rather more than half. A reduction of the Greenwich underground observations, recently communicated to me by Professor Everett of Windsor, Nova Scotia, gives for the Greenwich rocks a quality intermediate between those of the Edinburgh rocks. But we are very ignorant as to the effects of high temperatures in altering the conductivities and specific heats of rocks, and as to their latent heat of fusion. We must therefore allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have; nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of "emergence" of the "consistentior status" was probably between those dates.

12. The mathematical theory on which these estimates are

founded is very simple, being in fact merely an application of one of Fourier's elementary solutions to the problem of finding at any time the rate of variation of temperature from point to point, and the actual temperature at any point, in a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two different constant values on the two sides of a certain infinite plane. The solution for the two required elements is as follows:—

$$\frac{dv}{dx} = \frac{V}{\sqrt{\pi\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}},$$

$$v = v_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{\kappa t}}} dz \epsilon^{-z^2},$$

where κ denotes the conductivity of the solid, measured in terms of the thermal capacity of the unit of bulk;

V half the difference of the two initial temperatures;

v_0 their arithmetical mean;

t the time;

x the distance of any point from the middle plane;

v the temperature of the point x at time t ;

and, consequently (according to the notation of the differential calculus), $\frac{dv}{dx}$ the rate of variation of the temperature per unit of length perpendicular to the isothermal planes.

13. To demonstrate this solution, it is sufficient to verify (1) that the expression for v fulfils the partial differential equation

$$\frac{dv}{dt} = \kappa \frac{d^2v}{dx^2},$$

Fourier's equation for the "linear conduction of heat;" (2) that when $t=0$, the expression for v becomes v_0+V for all positive, and v_0-V for all negative values of x ; and (3) that the expression for $\frac{dv}{dx}$ is the differential coefficient with reference to x , of the expression for v . The propositions (1) and (3) are proved directly by differentiation. To prove (2), we have, when $t=0$ and x positive,

$$v = v_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\infty} dz \epsilon^{-z^2};$$

or, according to the known value, $\frac{1}{2}\sqrt{\pi}$, of the definite integral $\int_0^{\infty} dz \epsilon^{-z^2}$,

$$v = v_0 + V;$$

and for all values of t , the second term has equal positive and negative values for equal positive and negative values of x , so that when $t=0$ and x is negative,

$$v = v_0 - V.$$

The admirable analysis by which Fourier arrived at solutions including this, forms a most interesting and important mathematical study. It is to be found in his *Théorie Analytique de la Chaleur* (Paris, 1822).

14. The accompanying Plate (Pl. I.) represents, by two curves, the preceding expressions for $\frac{dv}{dx}$ and v respectively.

15. The solution thus expressed and illustrated applies, for a certain time, without sensible error, to the case of a solid sphere primitively heated to a uniform temperature, and suddenly exposed to any superficial action, which for ever after keeps the surface at some other constant temperature. If, for instance, the case considered is that of a globe 8000 miles diameter of solid rock, the solution will apply with scarcely sensible error for more than 1000 millions of years. For if the rock be of a certain average quality as to conductivity and specific heat, the value of κ , as I have shown in a previous communication to the Royal Society*, will be 400, to unit of length of a British foot and unit of time a year; and the equation expressing the solution becomes

$$\frac{dv}{dx} = \frac{V}{35 \cdot 4} \cdot \frac{1}{t^{\frac{1}{2}}} \cdot \epsilon^{-\frac{x^2}{1600t}};$$

and if we give t the value 1,000,000,000, or anything less, the exponential factor becomes less than $\epsilon^{-5 \cdot 6}$ (which being equal to about $\frac{1}{270}$, may be regarded as insensible) when x exceeds 3,000,000 feet or 568 miles. That is to say, during the first 1000 million years the variation of temperature does not become sensible at depths exceeding 568 miles, and is therefore confined to so thin a crust that the influence of curvature may be neglected.

16. If, now, we suppose the time to be 100 million years from the commencement of the variation, the equation becomes

$$\frac{dv}{dx} = \frac{V}{354000} \epsilon^{-\frac{x^2}{160000000000}}.$$

The diagram (Pl. I.) therefore shows the variation of temperature which would now exist in the earth if, its whole mass being first solid and at one temperature 100 million years ago, the temperature of its surface had been everywhere suddenly lowered by

* "On the Periodical Variations of Underground Temperature," Trans. Roy. Soc. Edinb. March 1860.

V degrees, and kept permanently at this lower temperature, the scales used being as follows:—

(1) For depth below the surface,—scale along OX, 10 quarter inches, or a , represents 400,000 feet.

(2) For rate of increase of temperature per foot of depth,—scale of ordinates parallel to OY, 10 half inches, or b , represents $\frac{1}{354000}$ of V per foot. If, for example, $V=7000^\circ$, this scale will be such that 10 half inches, or b , represents $\frac{1}{50.6}$ of a degree per foot.

(3) For excess of temperature,—scale of ordinates parallel to OY, 10 half inches, or b , represents $\frac{V}{\frac{1}{2}\sqrt{\pi}}$, or 7900° , if $V=7000^\circ$.

Thus the rate of increase of temperature from the surface downwards would be sensibly $\frac{1}{51}$ of a degree per foot for the first 100,000 feet or so. Below that depth the rate of increase per foot would begin to diminish sensibly. At 400,000 feet it would have diminished to about $\frac{1}{141}$ of a degree per foot. At 800,000 feet it would have diminished to less than $\frac{1}{50}$ of its initial value,—that is to say, to less than $\frac{1}{2550}$ of a degree per foot; and so on, rapidly diminishing, as shown in the curve. Such is, on the whole, the most probable representation of the earth's present temperature at depths of from 100 feet, where the annual variations cease to be sensible, to 100 miles; below which the whole mass, or all except a nucleus cool from the beginning, is (whether liquid or solid) probably at, or very nearly at, the proper melting temperature for the pressure at each depth.

17. The theory indicated above throws light on the question so often discussed—Can terrestrial heat have influenced climate through long geological periods? and allows us to answer it very decidedly in the negative. There would be an increment of temperature at the rate of 2° F. per foot downwards near the surface 10,000 years after the beginning of the cooling in the case we have supposed. The radiation from earth and atmosphere into space (of which we have yet no satisfactory absolute measurement) would almost certainly be so rapid in the earth's actual circumstances as not to allow a rate of increase of 2° F. per foot underground to augment sensibly the temperature of the surface; and hence I infer that the general climate cannot be sensibly affected by conducted heat at any time more than 10,000 years after the commencement of superficial solidification. No doubt, however, in particular places there might be an elevation of temperature by thermal springs, or by eruptions of melted lava; and everywhere vegetation would for the first 3,000,000

or 4,000,000 years, if it existed so soon after the epoch of consolidation, be influenced by the sensibly higher temperature met with by roots extending a foot or more below the surface.

18. Whatever the amount of such effects is at any one time, it would go on diminishing according to the inverse proportion of the square roots of the times from the initial epoch. Thus, if at 10,000 years we have 2° per foot of increment below ground,

At	40,000 years	we should have	1°	per foot.
„	160,000	„	$\frac{1}{2}$	„
„	4,000,000	„	$\frac{1}{10}$	„
„	100,000,000	„	$\frac{1}{50}$	„

It is therefore probable that for the last 96,000,000 years the rate of increase of temperature under ground has gradually diminished from about $\frac{1}{10}$ th to about $\frac{1}{50}$ th of a degree Fahrenheit per foot, and that the thickness of the crust through which any stated degree of cooling has been experienced has gradually increased in that period from $\frac{1}{5}$ th of what it is now to what it is. Is not this, on the whole, in harmony with geological evidence, rightly interpreted? Do not the vast masses of basalt, the general appearances of mountain-ranges, the violent distortions and fractures of strata, *the great prevalence of metamorphic action* (which must have taken place at depths of not many miles, if so much), all agree in demonstrating that the rate of increase of temperature downwards must have been much more rapid, and in rendering it probable that volcanic energy, earthquake shocks, and every kind of so-called plutonic action have been, on the whole, more abundantly and violently operative in geological antiquity than in the present age?

19. But it may be objected to this application of mathematical theory (1) that the earth was once all melted, or at least melted all round its surface, and cannot possibly, or rather cannot with any probability, be supposed to have been ever a uniformly heated solid 7000° warmer than our present surface temperature, as assumed in the mathematical problem; and (2) no natural action could possibly produce at one instant, and maintain for ever after, a seven thousand degrees' lowering of the surface temperature. Taking the second objection first, I answer it by saying, what I think cannot be denied, that a large mass of melted rock exposed freely to our air and sky will, after it once becomes crusted over, present in a few hours, or a few days, or at the most a few weeks, a surface so cool that it can be walked over with impunity. Hence after 10,000 years, or, indeed, I may say after a single year, its condition will be sensibly the same as if the actual lowering of temperature experienced by the surface had been produced in an instant and main-

tained constant ever after. I answer the first objection by saying that if experimenters will find the latent heat of fusion, and the variations of conductivity and specific heat of the earth's crust up to its melting-point, it will be easy to modify the solution given above, so as to make it applicable to the case of a liquid globe gradually solidifying from without inwards, in consequence of heat conducted through the solid crust to a cold external medium. In the mean time we can see that this modification will not make any considerable change in the resulting temperature of any point in the crust, unless the latent heat parted with on solidification proves, contrary to what we may expect from analogy, to be considerable in comparison with the heat that an equal mass of the solid yields in cooling from the temperature of solidification to the superficial temperature. But, what is more to the purpose, it is to be remarked that the objection, plausible as it appears, is altogether fallacious, and that the problem solved above corresponds much more closely in all probability with the actual history of the earth, than does the modified problem suggested by the objection. The earth, although once all melted, or melted all round its surface, did in all probability really become a solid at its melting temperature all through, or all through the outer layer, which had been melted; and not until the solidification was thus complete, or nearly so, did the surface begin to cool. That this is the true view can scarcely be doubted when the following arguments are considered.

20. In the first place, we shall assume that at one time the earth consisted of a solid nucleus, covered all round with a very deep ocean of melted rocks, and left to cool by radiation into space. This is the condition that would supervene, on a cold body much smaller than the present earth meeting a great number of cool bodies still smaller than itself, and is therefore in accordance with what we may regard as a probable hypothesis regarding the earth's antecedents. It includes, as a particular case, the commoner supposition, that the earth was once melted throughout, a condition which might result from the collision of two nearly equal masses. But the evidence which has convinced most geologists that the earth had a fiery beginning, goes but a very small depth below the surface, and affords us absolutely no means of distinguishing between the actual phenomena, and those which would have resulted from either an entire globe of liquid rock, or a cool solid nucleus covered with liquid to any depth exceeding 50 or 100 miles. Hence, irrespectively of any hypothesis as to antecedents from which the earth's initial fiery condition may have followed by natural causes, and simply assuming, as rendered probable by geological evidence, that there was at one time melted rock all over the surface, we need

not assume the depth of this lava ocean to have been more than 50 or 100 miles; although we need not exclude the supposition of any greater depth, or of an entire globe of liquid.

21. In the process of refrigeration, the fluid must (as I have remarked regarding the sun, in a recent article in 'Macmillan's Magazine'*, and regarding the earth's atmosphere, in a communication to the Literary and Philosophical Society of Manchester†) be brought by convection to fulfil a definite law of distribution of temperature which I have called "convective equilibrium of temperature." That is to say, the temperatures at different parts in the interior must differ according to the different pressures by the difference of temperatures which any one portion of the liquid would present, if given at the temperature and pressure of any part, and then subjected to variation of pressure, but prevented from losing or gaining heat. The reason for this is the extreme slowness of true thermal conduction, and the consequently preponderating influence of great currents throughout a continuous fluid mass in determining the distribution of temperature through the whole.

22. The thermo-dynamic law connecting temperature and pressure in a fluid mass, not allowed to lose or gain heat, investigated theoretically, and experimentally verified in the cases of air and water by Dr. Joule and myself ‡, shows, therefore, that the temperature in the liquid will increase from the surface downwards, if, as is most probably the case, the liquid contracts in cooling. On the other hand, if the liquid, like water near its freezing-point, expanded in cooling, the temperature, according to the convective and thermo-dynamic laws just stated (§§ 21, 22), would actually be lower at great depths than near the surface, even although the liquid is cooling from the surface; but there would be a very thin superficial layer of lighter and cooler liquid, losing heat by true conduction, until solidification at the surface would commence.

23. Again, according to the thermo-dynamic law of freezing,

* March 1862.

† Proceedings, Jan. 1862, "On the Convective Equilibrium of Temperature in the Atmosphere."

‡ Joule, "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," *Phil. Mag.* S. 3. vol. xxv. p. 369. Thomson, "On a Method for determining Experimentally the Heat evolved by the Compression of Air; Dynamical Theory of Heat, part 4," *Trans. Roy. Soc. Edinb.*, Session 1850-51; and reprinted, *Phil. Mag.* S. 4. vol. iv. p. 424. Joule and Thomson, "On the Thermal Effects of Fluids in Motion," *Trans. Roy. Soc. Lond.*, June 1853 and June 1854. Joule and Thomson, "On the Alterations of Temperature accompanying Changes of Pressure in Fluids," *Phil. Mag.* S. iv. vol. xv. p. 538.

investigated by my brother, Professor James Thomson*, and verified by myself experimentally for water †, the temperature of solidification will, at great depths, because of the great pressure, be higher there than at the surface if the fluid contracts, or lower than at the surface if it expands, in becoming solid.

24. How the temperature of solidification, for any pressure, may be related to the corresponding temperature of fluid convective equilibrium, it is impossible to say, without knowledge, which we do not yet possess, regarding the expansion with heat, and the specific heat of the fluid, and the change of volume, and the latent heat developed in the transition from fluid to solid.

25. For instance, supposing, as is most probably true, both that the liquid contracts in cooling towards its freezing-point, and that it contracts in freezing, we cannot tell, without definite numerical data regarding those elements, whether the elevation of the temperature of solidification, or of the actual temperature of a portion of the fluid given just above its freezing-point, produced by a given application of pressure, is the greater. If the former is greater than the latter, solidification would commence at the bottom, or at the centre, if there is no solid nucleus to begin with, and would proceed outwards; and there could be no complete permanent incrustation all round the surface till the whole globe is solid, with, possibly, the exception of irregular, comparatively small spaces of liquid.

26. If, on the contrary, the elevation of temperature, produced by an application of pressure to a given portion of the fluid, is greater than the elevation of the freezing temperature produced by the same amount of pressure, the superficial layer of the fluid would be the first to reach its freezing-point, and the first actually to freeze.

27. But if, according to the second supposition of § 22, the liquid expanded in cooling near its freezing-point, the solid would probably likewise be of less specific gravity than the liquid at its freezing-point. Hence the surface would crust over permanently with a crust of solid, constantly increasing inwards by the freezing of the interior fluid in consequence of heat conducted out through the crust. The condition most commonly assumed by geologists would thus be produced.

28. But Bischof's experiments, upon the validity of which, so far as I am aware, no doubt has ever been thrown, show that melted granite, slate, and trachyte, all contract by something about 20 per cent. in freezing. We ought, indeed, to have

* "Theoretical Considerations regarding the Effect of Pressure in Lowering the Freezing-Point of Water," *Trans. Roy. Soc. Edinb.* Jan. 1849.

† *Proc. Royal Soc. Edinb.*, Session 1849-50.

more experiments on this most important point, both to verify Bischof's results on rocks, and to learn how the case is with iron and other unoxidized metals. In the mean time we must consider it as probable that the melted substance of the earth did really contract by a very considerable amount in becoming solid.

29. Hence if, according to any relations whatever among the complicated physical circumstances concerned, freezing did really commence at the surface, either all round or in any part, before the whole globe had become solid, the solidified superficial layer must have broken up and sunk to the bottom, or to the centre, before it could have attained a sufficient thickness to rest stably on the lighter liquid below. It is quite clear, indeed, that if at any time the earth were in the condition of a thin solid shell of, let us suppose, 50 feet or 100 feet thick of granite, enclosing a continuous melted mass of 20 per cent. less specific gravity in its upper parts, where the pressure is small, this condition cannot have lasted many minutes. The rigidity of a solid shell of superficial extent so vast in comparison with its thickness, must be as nothing, and the slightest disturbance would cause some part to bend down, crack, and allow the liquid to run out over the whole solid. The crust itself would in consequence become shattered into fragments, which must all sink to the bottom, or to meet in the centre and form a nucleus there if there is none to begin with.

30. It is, however, scarcely possible that any such continuous crust can ever have formed all over the melted surface at one time, and afterwards have fallen in. The mode of solidification conjectured in § 25, seems on the whole the most consistent with what we know of the physical properties of the matter concerned. So far as regards the result, it agrees, I believe, with the view adopted as the most probable by Mr. Hopkins*. But whether from the condition being rather that described in § 26, which seems also possible, for the whole or for some parts of the heterogeneous substance of the earth, or from the viscosity as of mortar, which necessarily supervenes in a melted fluid, composed of ingredients becoming, as the whole cools, separated by crystallizing at different temperatures before the solidification is perfect, and which we actually see in lava from modern volcanoes; it is probable that when the whole globe, or some very thick superficial layer of it, still liquid or viscid, has cooled down to near its temperature of perfect solidification, incrustation at the surface must commence.

31. It is probable that crust may thus form over wide extents

* See his Report on "Earthquakes and Volcanic Action," British Association Report for 1847.

of surface, and may be temporarily buoyed up by the vesicular character it may have retained from the ebullition of the liquid in some places; or, at all events, it may be held up by the viscosity of the liquid, until it has acquired some considerable thickness sufficient to allow gravity to manifest its claim, and sink the heavier solid below the lighter liquid. This process must go on until the sunk portions of crust build up from the bottom a sufficiently close-ribbed solid skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava pools or lakes.

32. In the honey-combed solid and liquid mass thus formed, there must be a continual tendency for the liquid, in consequence of its less specific gravity, to work its way up; whether by masses of solid falling from the roofs of vesicles or tunnels, and causing earthquake shocks, or by the roof breaking quite through when very thin, so as to cause two such hollows to unite, or the liquid of any of them to flow out freely over the outer surface of the earth; or by gradual subsidence of the solid, owing to the thermo-dynamic melting which portions of it, under intense stress, must experience, according to views recently published by my brother, Professor James Thomson*. The results which must follow from this tendency seem sufficiently great and various to account for all that we see at present, and all that we learn from geological investigation, of earthquakes, of upheavals and subsidences of solid, and of eruptions of melted rock.

33. These conclusions, drawn solely from a consideration of the necessary order of cooling and consolidation, according to Bischof's result as to the relative specific gravities of solid and of melted rock, are in perfect accordance with what I have recently demonstrated† regarding the present condition of the earth's interior,—that it is not, as commonly supposed, all liquid within a thin solid crust of from 30 to 100 miles thick, but that it is on the whole more rigid certainly than a continuous solid globe of glass of the same diameter, and probably than one of steel.

II. *On a New Photometer.* By H. W. DOVE ‡.

BY means of our present photometric arrangements, the intensity of two sources of light may, under certain circumstances, be measured; but it may be urged against them that they are quite inefficient when the sources of light to be com-

* Phil. Mag. S. 4. vol. xxiv. p. 395, "On Crystallization and Liquefaction as influenced by Stresses tending to Change of Form in Crystals."

† In a paper "On the Rigidity of the Earth," Proc. Roy. Soc. vol. xii. p. 103.

‡ Translated from the *Monatsbericht* of the Berlin Academy of Sciences, May 1861.

pared are of different colours, or if a determination is to be made of the brightness of light diffused in a given space, or if the quantity of light is to be measured which a very small, or only feebly transparent body transmits. In the latter case, the very convenient method given by Bunsen for bright flames cannot be applied: this method consists in illuminating by the lights a grease-spot on a sheet of paper both in front and behind so that it disappears. Babinet's method of neutralizing the colours of polarization of two masses of light polarized at right angles to each other also excludes the use of different-coloured lights, as is directly evident from the bright colorific phenomena which are produced in my dichroscope*. The transformation of a positive Daguerreotype into a negative, when the reflected light preponderates over the diffused light which reaches the eye from the same, which Pouillet has proposed, requires a room with black walls. It has consequently only a limited application; moreover, its delicacy is not considerable if the objects to be compared present small surfaces, inasmuch as all the masses of light which fall diffused on the plate from all sources concur in producing the phenomena on the Daguerreotype plate. The juxtaposition of equally dark shadows of a rod, as devised by Rumford, or of bright lines of light of a rotating luminous knob, as in Wheatstone's method, excludes at once differently-coloured sources of light, whose equal brilliancy the eye cannot judge. The enfeeblement of light caused by crossed Nicols, glass plates, or polarizing mirrors becomes an uncertain criterion in feeble sources of light, if the measurement consists in judging the actual disappearance, and not in the passage of one phenomenon into the opposite, which is essential in definite determinations. The method I use has these advantages over those enumerated:—that it is very delicate; that it can be applied to objects of any size in the same manner, whether they are brightly or feebly luminous, of the same or different colours, and whether transparent or opaque; that, moreover, it is fitted for determining the intensity of light of optical instruments, that it allows of several different methods of measurement which mutually control each other, and, lastly, that it is obtained by means of an instrument which is in the hands of every working man of science.

There are certain microscopic objects, for instance the skin of an ephemera, which appear dark upon a bright ground when illuminated from below, but bright upon a dark ground when the illuminating mirror is covered. This is seen in a far more beautiful manner in microscopic photographs, for instance in Major Dickson's tablet in Rostherne Church, when it is looked at in one of Schieck's microscopes with a power magnifying

* Phil. Mag. S. 4. vol. xx. p. 352.

fifty times. The illumination from below gives a deep black impression on a white ground; and if the mirror is covered, illumination from above gives a white impression upon a dark ground. From this it would appear probable that the printing would disappear if the light incident from above and from below had the same intensity, or if there were a definite relation between them, inasmuch as the angle under which they are incident may be different. If in the objective tube a polarizing Nicol be fitted, and if the ordinary ocular be replaced by that which contains an analysing Nicol, the inscription disappears on turning the analysing arrangement. The smallest further rotation transforms the previously dark inscription into a white one,—a proof of the great delicacy of the method. This is further seen in the fact, that if, at the position at which the inscription disappears, a feebly dim glass be interposed, the white inscription immediately appears on a dark ground if it be interposed in the light incident from below, while the bright inscription appears on a dark ground if the glass is interposed in the light incident from above.

If the light incident from below be twice successively dimmed, so that, from the disappearance of the inscription, it counterbalances the light which is incident from above with unaltered brightness, it is at once clear that the quantities of light must in both cases be the same, inasmuch as the rays fall on the microscopic object under exactly the same conditions. If now the methods which, with sources of light of different brightness, require the deadening of the stronger so that both shall be equal, contain also in themselves a determination of the degree of this deadening, a quantitative determination of their different intensity under the same conditions follows directly therefrom.

In modern microscopes there is the arrangement that the illuminating mirror can be placed aside by a double angular motion; further, that the instrument itself can be removed from the vertical into any other position, which, as it only deviates considerably from the horizontal in certain special cases, I shall designate the horizontal, in opposition to the vertical position, in which the use of the mirror is presupposed.

The modes of deadening are as follows:—

1. Diminution of the aperture of the objective tube.
2. Removal of the source of light from the same.
3. Increase of the acting surface of the source of light by inclining it towards the aperture, which represents the rectangular projection of that surface; in which case the cylindrical aperture can be so arranged, by adding a tube blackened on the inside, that only parallel rays fall on the photographic picture.
4. Rotation of an ocular provided with an analysing Nicol,

after the analysing Nicol has been placed in the aperture of the objective tube.

To diminish the aperture, a metallic lineal can be used in which circular apertures of gradually diminishing size are arranged in a straight line, of which the largest has the same diameter as the aperture. I shall call this lineal the *slider*. In the lineal which I used there were fourteen such apertures. Their diameter can be determined by the micrometric arrangement of the microscope. Instead of the slider an eccentric disc can be used with diminishing apertures, as is often found on the older microscopes; yet a rectilinear slider is preferable, because the excentric disc, if it is to contain many apertures, becomes of an inconvenient size. The distance of the source of light is determined on a scale. The zero-point of the scale is, in a horizontal position, the photographic picture, which is fastened on the objective stand by the ordinary objective holder.

In order to determine the size of the acting surface, any angle-measurers may be used. If the body to be tested is a plane surface diffusing light, it can be placed in the centre of a horizontal circle by whose limb it can be moved; or a plane mirror is placed on the surface, in which a distant scale is read off by means of a telescope. If the intensity of the light incident on a plane mirror under different angles is to be determined, the same method can be used to determine its angle. If, on the contrary, the reflecting plane is the free surface of a liquid, the incidence is obtained by inclining the axis of the microscope, which is measured by means of a mirror fixed to it. In a similar manner the rotation of the analysing Nicol is obtained by means of an affixed mirror.

Feeble sources of light can be intensified, if in a vertical position, by changing the plane illuminating-mirror for a concave mirror, and in a horizontal position by an illuminating lens which is so fixed that the concentration on the photographic image takes place in the same manner as previously by the concave mirror. Where parallelism of the incident rays is required, the source of light is in the focus of the lens.

For different sources of light the process is different. I shall discuss it singly for the different kinds.

Dioptric Colours of Absorption and diffused Light of Transparent Bodies.

Coloured Glasses.—When the microscope is in a vertical position, the object is illuminated from below by the mirror directed towards a portion of the heavens, and from above by the ordinary light of day. The thickness of the glasses which close the aperture of the objective is now changed until compensation is

attained. If the quantity of light incident from below is thereby more than compensated, the inscription does not appear white, but of a bright subjective colour; but the transition from dark on light ground, to light on dark ground, is readily perceived. With glass coloured only superficially (*Ueberfanggläser*), an increase of the thickness can only be obtained by superposing them; with coloured glasses, by means of flat-cut wedges which are pushed over one another; with coloured mica, by splitting it, and determining the thickness by means of the spherometer. If the compensation has been obtained for different-coloured glasses in succession, this serves as a scale of colours of the same brightness.

To answer the question in what relation the brightness decreases with increasing thickness, compensation is first effected for the greater thickness, and the magnitude of the aperture diminished by means of the slider until compensation is effected for the less thickness. With parallel incident light, the brightness is inversely as the aperture. If the polarizing Nicol is placed in the aperture, the same result can be obtained by turning the analyser; for this only affects the polarized light incident from below, and not the diffused and therefore unpolarized light incident from above.

In the same way the relation of the transmitted light with the same thickness of different-coloured substances is obtained by means of the slider or of the Nicol.

I was much surprised in these experiments at finding how little the eye can judge the intensity of the light which a translucent body transmits in comparison with a transparent one. I constructed a set of glasses out of a cut ground glass, and increased their number until the light which they transmitted was equal to that which traversed a red superficially-coloured glass with reflecting surfaces. Both held at the same distance from the eye, or held close before it, appear very different, that is, the red glass much brighter. Unconsciously we connect the idea of distinctness with the estimation of brightness, so that the latter is subordinate to the former.

The testing of dichroitic crystals is effected by obtaining compensation in different directions. In some which I examined the difference was considerable. The testing of coloured glasses for solar observations, which darken greatly, is effected, after the mirror has been turned aside, by directing the instrument towards the sun immediately after the glasses have been interposed. The determination of a still greater darkening, by combining different-coloured glasses, is effected in the same manner.

The absorption of very transparent substances, as coloured liquids, which must be used in long tubes provided with move-

able glass plates, is also effected when the instrument is in a horizontal position. The microscope is employed in the same position when the liquid is one which transmits very little light, as, for example, indigo solution. For this purpose I have used the capillary apparatus, in which the liquid ascends between two slightly inclined glass plates. The distance from the edge gives the increasing thickness of the layer of liquid for a given distance of the plates. In the case of coloured gases which are enclosed in vessels, the instrument also stands horizontal. The turbidity by smoke may be investigated in the open air.

With cloths, paper, &c., the increasing thickness is obtained by folding them together. The quantity of light transmitted in this case is not that directly observed, but partly that transmitted by the interstitial spaces. The same is the case with thin coatings of gold or silver.

Measurement of the Light diffused by Opaque Bodies.

If, in ordinary daylight, a sheet of white paper is held horizontally under the objective tube of a microscope placed horizontally, the illumination from above can be so regulated that the dark inscription is seen upon a white ground. By increasing the inclination, the white ground appears continually brighter in reference to the dark inscription. If the white sheet is exchanged for a dull black one, or for a uniformly blackened surface, under all inclinations the white inscription appears on a dark ground. With a coloured surface it is different. If in this case the bright inscription appears upon a dark ground, at a certain inclination it disappears, and, on increasing the inclination, changes into a dark inscription upon a bright ground. This gives a very simple means of judging which of two adjacent colours is the brightest. The surface is inclined until the inscription disappears, and the other colour is then brought into the same position. It is brighter or darker according as the inscription appears black or white. In an accurate determination of the angle at which the transition takes place, all lateral light must be excluded. To effect this, I have placed the horizontally arranged microscope in such a manner that a tube a foot long, blackened on the inside (part of a gun-barrel), was in the continuation of the optical axis of the microscope, by placing this tube directly against the inferior aperture of the objective tube. The coloured discs, placed in a vertical position and working on a vertical axis, were placed at the other end of the tube.

The moment at which a colourless transparent body, such as water, becomes opaque in consequence of total reflexion, is distinctly seen by looking obliquely into a glass at the under

side of the free surface—and in a still more surprising manner in the dazzling light which directly emerges from the perfectly dark cylindrical jet of water which is strongly illuminated from within, at the moment at which it separates in drops or is interrupted by a shock, while a light held in the dark space immediately behind passes through unchanged. Since with the frequency of interruption the probability for the limiting angle of total reflexion increases, we see directly how that which is seen in a bubble in water, multiplying itself, produces white foam—just as a crack in ice forms the transition to white snow, a crack in glass explains the white powder of a broken glass drop. But this powder is only white if the drop was of colourless glass; it is greenish if it were of green glass, just as the froth of champagne is clearer than that of beer, of coffee, or of chocolate, all, however, exceeded in whiteness by soap-bubbles. The depolarization of polarized light incident on a snow surface proves that the totally reflecting surfaces are inclined in all directions; and hence we can form an idea of the process of the irregular reflexion of light from a white, grey, or coloured surface—in the latter cases if, to the conditions producing the first phenomena, we add that of absorption in the passage through coloured substances.

On this view the rough surface of an unpolished body will not reflect light irregularly because it represents mirrors inclined in all directions; for on this supposition the colour of the body would not be perceptible; on the contrary, it would present surfaces to the light which facilitate the penetration at an almost vertical incidence. Hence it is that increasing polish gradually conceals the colour of a body. The fact that a very dark colouring substance appears brighter in a state of powder than one in a lump which had already a rough surface, does not disaccord with this; for here, by the greatly increased inequalities, the reflexion at a very acute inclination is increased, whereas the multifold rectangular incidences are little inclined to deepen the colour in consequence of the great opacity.

It follows from the preceding considerations, that diffused light can be considered as if each point was self-luminous; in this case if o is the aperture of the objective tube, and x the angle which the diffusing surface makes with the axis of the microscope, the surface which sends light upon the photographic picture will be $\frac{o}{\sin x}$, and the brightness will be proportional to this. If for two different substances with the same general illumination compensation takes place at the angles x_1 and x_2 , their respective brightness will be in the ratio $\frac{1}{\sin x_1}$ and

$\frac{1}{\sin x''}$. Assuming the strict validity of a diffusion taking place uniformly in all directions, the determination of the brightness of a body which causes this is reduced to the simple measurement of an angle. The strict validity, or its limitation, may, however, be ascertained empirically on comparing the values obtained by diminishing the aperture by means of the slider, the inclination remaining the same, with the values obtained by altering the inclination while the aperture remains the same. When an investigation is not made in this manner, the use of the slider is the most reliable.

If the brightness resulting from the combination of the light emitted by two differently coloured surfaces is to be determined, it is best obtained by means of Fechner's discs: in these, in concentric rings, the magnitude of the sector belonging to one colour gradually increases from 0° to 360° , while that of the other decreases simultaneously from 360° to 0° . The other rings being deadened off, the horizontal microscope is successively directed towards the individual concentric rings, and the brightness compensated.

In rotating a Newton's coloured disc, of course the brightness of a white one is not obtained, but only that corresponding to the absorption of all individual colours. It is only necessary to direct the photometer upon the rotating disk, and then ascertain the angle which gives compensation, and subsequently to repeat the same experiment with the white back of the disk, to see that in the first case much light was absorbed. To determine the quantity, a dark sector is gradually enlarged upon a white disc until the brightness of both discs is the same.

This gives rise to a physiological question, the answer to which is not without interest. If a disc is divided into five white and five dark sectors, the quantity of light which it sends to the eye, while rotating with a definite velocity, is the same as if the disc were divided into twenty-five dark sectors alternating with twenty-five white ones; but the intervals in the first case last five times as long as in the second. Has this an influence on the determination of the brightness? It has always appeared to me as if the brightness increased until the velocity of rotation exceeded a certain limit. Possibly the limit of the interval at which no increase takes place is different with different individuals.

Streaked surfaces reflect more light in the direction of the stripes than in one at right angles to it, because in the latter case there is a partial overshadowing. This is distinctly observed by producing a grating by passing chalk several times over a file. In these respects the most remarkable differences are obtained on certain plates of mother-of-pearl.

With a very uniform dead polish the influence of the material prevails. In this respect a brass disc gave somewhat greater brightness than a copper disc.

In this case the vertical direction of the instrument is preferable, because in rotating while in a horizontal position, the illumination of the other side of the photographic picture changes. If the surfaces diffusing light surround a given space, for instance the walls of a room, it is merely necessary, the microscope standing upright, to direct the concave mirror to different parts of the room, to show surprising differences by sudden transposition of dark into light.

Examination of Luminous Bodies.

In the preceding investigations, the chief point was, how much of the brightness of a given illumination is lost when it is subject to absorption. The want of a unit of light is here less felt than in the case of self-luminous bodies. As nature has not given us this unit, an approximation has been sought in normal candles, the light from which, if the length of the flame is kept the same, is tolerably constant. Other methods are by means of Argand lamps, or gas-flames from given apertures under constant pressure, or, finally, by means of a platinum wire which closes a circuit of given intensity. Uncertainty affects all photometric methods to the same extent, if it is required to reduce a given intensity of light to an absolute unit.

At first it would appear simplest to place the horizontal microscope in such a position that its visual axis shall cut the line joining the two flames whose intensities are to be compared, in that point at which the photographic image appears. But it is much more convenient to direct the microscope successively upon the two flames, and to alter their distance so that the constant light which from a definite distance illuminates the object makes the writing disappear. If both lights are in the prolongation of the visual axis of the microscope, the action of both lights is obtained directly after one another, if behind the first a screen is placed which conceals the second, and then the first and the screen are simultaneously removed: this can be effected so rapidly that the flames, which are considered to be constant, may be assumed to be really so. In flames not so bright, a twentyfold magnifying power may be used. In the case of a gas-flame at a distance of some hundred feet, the passage of black print on dark ground to light upon dark ground is very distinctly seen if a spirit-lamp containing alcohol saturated with salt is held in front. The feebleness of the light of some flames as compared with others is surprising. If the instrument is directed upon a stearine candle at a distance of

some feet, such a spirit-flame may be placed between the objective of the microscope and the photographic image, and yet the dark print upon white ground is seen on looking through the flame. At a greater distance it disappears, inasmuch as the print is completely covered by the flame in front.

The brightness of the moon in different phases may be determined in the same manner. The microscope is directed upon it, and the picture is illuminated from the front by a candle.

The ignition of a platinum wire closing a galvanic circuit, the intensity of which varies, may be determined in the same manner.

Some years ago I had occasion to see experiments upon the artillery-practice ground here, in which the electric light of a powerful battery was compared, by means of a large concave mirror, with that produced by rockets sent up for the purpose of ascertaining the progress of besiegers. The comparison was effected by directly looking at the place where the men were drawn up. It would have been obtained with greater certainty if the illumination of a white surface had been ascertained by means of the photometer.

The spark passing from the condenser of one electrical machine to another gives a very distinct picture, which may be compensated.

Even by the discharge of a Leyden jar the writing is visible.

The stratified light of a very beautiful Geissler's tube was investigated in the same way. To the pear-shaped middle, two narrow tubes were fixed which terminated in bulbs with wires fused in. The blue light which surrounded the wire with its narrow envelope of light was somewhat brighter than the red stratified light of the nearest tubes, and almost of the same brightness as that on the second leg, but much brighter than that of the other pole in the bulb nearest this pole.

The whitish stratified light illuminated the print so as to make it distinctly perceptible, both when the tube was connected with the Ruhmkorff's apparatus, and when it was held on the conductor of an ordinary electrical machine.

One of the legs of a U-shaped Geissler's tube was surrounded with sulphate of quinine fluorescing under its influence. This leg shone more strongly than that which was not so surrounded, a proof that the apparatus is sufficiently delicate for fluorescence.

Phosphorescent light, on the contrary, did not come up to my expectations. Only in one case out of seven did I succeed, with a tube which shows very brightly after insolation, in recognizing the writing. This, it is true, was in an inadequately darkened room.

Intensity of the Light of Optical Instruments.

The examination of telescopes is very simple. Where, when the telescope is directed upon an infinitely distant object, the rays emerge parallel from the eyepiece, the intensity of the light of the telescope is determined by the brightness of a section of this emergent cylinder of light. The microscope is so placed that, while the aperture of the objective tube covers the aperture of the eyepiece, the axis of the microscope is the rectilinear prolongation of the axis of the telescope directed towards the heavens. For different instruments which are to be compared with one another, this may be rapidly effected either with a clear or with a uniformly clouded sky. The determination is made by approaching a constant light to the front side of a photographic picture.

It is not advantageous to use direct sunlight for this illumination of the front side; for with such strong illumination the writing appears like gold print, which renders the determination more difficult.

The determination of microscopes takes place in the same manner.

That of the reflexion of mirrors is effected by the method spoken of when considering the diffusion of rough surfaces. It has surprised me that a carefully polished silver mirror, at any rate under acute incidences, reflects more light upon the glass side than upon the metallic; in other words, that the addition of the reflexion from the outer surface of the glass more than compensates the loss which the metallic surface coating the glass experiences by the glass. The influence of this external reflexion is determined if the mirror, while at the polarizing angle of glass, is viewed through an eyepiece provided with a polarizing Nicol, so that the reflected light is polarized. The decrease of the intensity in consequence of multifold reflexion is easiest obtained by mirrors which can be brought near by means of a screw, where the reflecting side of the one overlaps that of the other laterally. The total reflexion of a rectangular prism is most easily obtained by bringing it under the aperture of the object-tube of the microscope standing almost vertical. The microscope, on the contrary, is almost horizontal in investigating a reflecting prism, or the combination of two, which I have named *reversion prism*. As this serves to change linearly-polarized into elliptically-polarized light, without, as in Fresnel's rhombohedron, coming out of the axis of the instrument, the intensity of its light can be compared with that of such a one. The method appears to me important for hollow mirrors and condensing-lenses, from its application to lighthouses with polyzonal lenses. In my resi-

dence I could only extend to a distance of 160 feet the experiments to ascertain if, by parallelism of the rays, the action of distance were eliminated or not; but the great intensity of light convinced me that the test was applicable to very considerable distances. Experiments at a constant distance of a stationary light might give information as to the limits of the influence of incipient obscurations in the atmosphere. Here the instrument acts as a diaphanometer. I also think that in solar eclipses it furnishes a much more accurate mode of determining the illumination than previous methods.

The intensity of the light of different parts of the spectrum may be obtained in two ways,—by allowing the individual rays to fall directly on the microscope arranged horizontally, or by directing the microscope upon a white wall on which the spectrum is received. A comparison of both shows the influences of diffusion caused by rough surfaces, and dependent on the length of wave. In this case I think it convenient to use, for illuminating the front side of the photograph by reflexion, the solar light itself, incident through another aperture in order to eliminate thereby the varying intensity at different times according to the transparency of the air.

I have investigated polarized colours as follows:—Between two Nicols, selenite plates of $\frac{1}{4}$ to $\frac{1\frac{1}{4}}{4}$ thickness were interposed, and the photographic picture was laid on the aperture of the upper one. In the polarizing apparatus which I have described, this takes place simply, inasmuch as, after introducing the selenite plates, it is examined like a telescope. The bull's-eye of the apparatus concentrates a luminous flame on the aperture of the polarizing Nicol.

Determination of the Light in a given space.

If the horizontal microscope be placed in any part of the room, the quantity of light which falls from above upon the object decreases with increasing distance from the window, while that sent upwards from the illuminating mirror directed towards the sky remains the same. It is hence clear that the equilibrium established in one place between the upper and lower illumination ceases in another, and in this way the equally bright places in the room are obtained.

For photographic and, as I have heard, for ophthalmological purposes, it is often desirable to have a definite illumination in a room. I will assume that on a day of given brightness a photographic view gives the desired result in a given time. The microscope is placed vertically near the apparatus, and a flame removed so far from the illuminating mirror that compensation is produced. To obtain the same light on repeating the process,

the distance of the lamp remaining the same, that position of the room is sought at which compensation occurs. In fact, the change in the brightness of the day's light in weather which is not quite clear, or when the sky is uniformly dull, is very considerable: this is best seen by directing the illuminating mirror to different parts of the sky, or by comparing the slight intensity of the light reflected from the blue sky with that of a whitish sky. Hence I think this photometer adapted for travellers, whose scientific outfit generally contains a microscope, or, as only a feeble power is required, it can be easily added to it. Measurements of the intensity of light while the sun has a high position in the southern hemisphere, compared with those made on the northern hemisphere for the same height of the sun, are entirely wanting. As, by the heating action of direct insolation, it must be considerable, it will also be perceived in the photometric results.

It will perhaps be convenient to make special photographs for this photometric process. Dickson's monument has probably raised letters on a marble plate, by which the brightness of the letters, where the shadow cooperates, is not the same all over the plate, from which the passage in the brighter and darker parts is not simultaneous. This difference is still more perceptible in the raised parts of the edge. The photograph of an English bank-note shows the transition more uniformly. The very fine impression attacked my eyes when viewed too long. I should propose to make a copy of printing of uniform darkness and size, or a simple drawing, for example that of a black cross on a bright ground, or that of a dark ring upon the same ground. Copies of copper-plates are less well fitted, because this negative picture appears objectionably quaint. It will not be difficult to find the most convenient photograph for such a purpose.

The manufacture of suitable sliders with apertures decreasing in size is also desirable.

I would here mention a practical application of the method to the estimation of colouring matters, the goodness of which, as in the case of indigo for example, can usually only be ascertained by direct inspection. The impression which such a substance makes upon the eye depends exclusively on the quantity of light which it sends to the eye, and on its colour. It is difficult to make a prismatic analysis with opaque bodies, but it is not so with an absorption analysis. Ultramarine appears quite black when viewed through a glass coloured red superficially; by glasses of other colours it appears peculiarly coloured. The quantity of light which it sends through such a glass may be

determined photometrically, although an accurate description is not easily given. If a special colouring matter is to be determined, it is only necessary to ascertain, by one of the methods described, the darkening which it experiences by glasses of definite colour. For a practical purpose, the special kind of glass need not be determined. In the hand of the practical man they are the known reagents for the colouring matter which he wishes to test.

III. *On the Nature of Nitrogen, and the Theory of Nitrification.*
By T. STERRY HUNT, F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Montreal, November 23, 1862.

ENCLOSED I send a translation of a little paper of mine from the *Comptes Rendus*, published in September last. It relates particularly to two papers, one by myself, and one by Schönbein, which have appeared in your journal, and I should be much pleased to see it appear in your pages.

I remain, Gentlemen,

Your most obedient Servant,

T. STERRY HUNT.

In 1848 I suggested that free nitrogen is the nitryl of nitrous acid, NHO^4 , $\text{NH}^3 - \text{H}^4 \text{O}^4 = \text{NN}$, corresponding to the nitric nitryl, NNO^2 , and to the phosphoric nitryl, PNO^2 (*American Journal of Science* [2] v. 408 ; vi. 172 ; viii. 375). It might then be supposed that, like these two bodies, nitrogen should under favourable conditions fix $\text{H}^4 \text{O}^4$, and regenerate nitrous acid and ammonia. In April 1861 I published a note in 'The Canadian Journal' of Toronto, in which it was said that the spontaneous formation of these two bodies by the combination of atmospheric nitrogen with water would serve to explain the production of ammonia, so often remarked in the presence of air and reducing agents, and also the formation of nitrates in the experiments of Cloez, without the intervention of ammonia, and at the expense of air and water in presence of alkaline matters (*Comptes Rendus*, vol. lxi. p. 135).

The simultaneous production of ozone and an acid of nitrogen by the electric spark, and during the slow oxidation of phosphorus, may be explained by the power of active oxygen to oxidize ammonia, thus setting free the acid of a small portion of regenerated nitrite of ammonia, and even, in accordance with

the observations of Houzeau, carrying its oxidizing action so far as to acidify the nitrogen of the atom of ammonia. Certain of the reactions attributed to ozone would thus, as many chemists have already maintained, be due to a minute portion of nitrous acid, which is formed when active oxygen is brought in contact with moist atmospheric nitrogen. On the other hand, the hydrogen set free by reducing agents may, by destroying the acid of the regenerated nitrite of ammonia, set free the ammonia of the salt, and even form a second atom by the reduction of the acid (The Canadian Journal, March 1861). These views will also be found in a note written by me, and published in the American Journal of Science for July 1861 (page 109), and copied into the Philosophical Magazine for September 1861, and the 'Chemical News.' I found that a current of air which had passed through a solution of permanganate of potash acidulated with sulphuric acid, acquired the odour and the reaction of ozone. This disappeared when the air was passed through a solution of potash, which at the end of a certain time appeared to contain a nitrite. This reaction, which seems to indicate the formation of nitrous acid, not by an electric or a catalytic action accompanying the production of ozone, but by the action of nascent oxygen upon atmospheric nitrogen in the presence of water, supports the above views, and, as I have remarked in the note in question, furnishes the key to a new theory of nitrification.

The formation of nitrite of ammonia by the combination of the nitryl NN with H^4O^4 , must necessarily be limited to very minute quantities by the instability of this ammoniacal salt, which, as is well known, decomposes readily into nitrogen and water. In order therefore to produce any considerable quantity of a nitrite by this reaction, there is required the presence of active oxygen or of a fixed base to separate the ammonia.

The recent experiments of Schönbein have furnished new evidences of the direct formation of a nitrite at the expense of the nitrogen of the atmosphere. According to him, when sheets of paper moistened with a feeble solution of an alkali or an alkaline carbonate are exposed to the air, especially in the presence of watery vapour, and at a temperature of 50° or 60° C., the alkaline base soon fixes a sufficient quantity of nitrous acid to give the characteristic reactions. Appreciable traces of nitrite are, according to Schönbein, obtained in this way even without the intervention of an alkali. He moreover found that distilled water, mixed with a little potash or sulphuric acid, and evaporated slowly at a temperature of 50° C. in the open air, fixes in the one case a small portion of ammonia, and in the other a little nitrous acid. Traces of a nitrite are also formed in pure water

under similar conditions. Schönbein explains all these results by the combination of nitrogen with the elements of water, producing at the same time ammonia and nitrous acid. As he has well remarked, this reaction serves to explain the absorption of nitrogen by vegetation, and, through the oxidation of nitrites, the formation of nitrates in nature. By these elegant experiments, he has confirmed in a remarkable manner my theory of nitrification, and of the double nature of free nitrogen. It is however evident that, since the publication of my note of March 1861 referred to above, we cannot say with Schönbein that the generation of nitrite of ammonia from nitrogen and water is "a most wonderful and wholly unexpected thing." (Letter from Schönbein to Faraday, *Philosophical Magazine*, June 1862, p. 467). I cannot, however, admit with these gentlemen that the results of Schönbein are due to evaporation, except in so far as the cooperation of water, and a slightly elevated temperature, are necessary conditions of the reaction.

IV. *On the Measurement of Altitudes by means of the Temperature at which Water Boils.* By J. BURGESS, Esq.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

SINCE Archdeacon Wollaston first employed the temperature of ebullition as a means of measuring altitudes, the subject has been several times discussed. The late Mr. James Prinsep calculated a table of heights corresponding to boiling-points for every degree of temperature from 176° to 214° F., which was employed by Colonel Sykes*, and has been several times reprinted†; and, more recently, Professor J. D. Forbes, in 1843 and again in 1854, contributed two papers on the subject to the 'Transactions of the Royal Society of Edinburgh.' By projecting graphically the altitudes derived from barometrical measurements made by him among the Alps, but uncorrected for temperature, in terms of the observed boiling-points, Professor Forbes found that they lay almost exactly in a straight line. Hence he inferred that "*the temperature of the boiling-point varies in a simple arithmetical ratio with the height*"‡.

2. But the general expression for difference of level in terms

* *Phil. Mag.* (1835) vol. vii. p. 311.

† Davies and Peck's 'Math. Dict.,' art. "Levelling;" Smith and Thuillier's 'Manual of Surveying for India,' p. 435; *Journal of the Royal Geographical Society*, vol. viii.

‡ *Transactions of the Royal Society of Edinburgh*, vol. xv.

$$h' - h = L \times \log \frac{B}{B'}$$

where B and B' are the heights of the barometer at the lower and upper stations respectively, $h' - h$ the difference of elevation uncorrected for temperature, and L a constant. Now, if Professor Forbes's hypothesis were true, we should have

$$T - T' = n \log \frac{B}{B'}; \dots \dots \dots (1)$$

T and T' being the boiling-temperatures at the lower and upper stations, or under the pressures B and B' respectively. Hence we obtain as the expression for the approximate difference of elevation

$$h' - h = \frac{L}{n} (T - T'); \dots \dots \dots (2)$$

and the hypothesis is correct or otherwise, according as the quantity n is constant or variable.

3. In the following Table are collected a number of observations made by M. Izarn among the Pyrenees*, Dr. Forbes in the Alps†, Dr. Joseph Hooker on the Himalaya and Khasia Mountains‡, MM. Martins and Bravais on Mont Blanc§, M. Marie on Mont Pila||, &c. In column (4) are tabulated the values of n derived from each observation, in (5) the boiling-points corresponding to the observed pressures in column (2) computed with $n = 112$, and in (6) the differences between the observed values in (3) and the computed ones in (5).

After making allowance for errors of observation, it is manifest from this Table that the value of n slowly decreases with the pressure. But so slow is the rate of decrease that Professor Forbes's hypothesis affords good approximate results if, for heights under 10,000 feet, we employ $n = 113.33$; and, L being = 60369 feet, by substitution in equation (2), we have

$$h' - h = 532.7(T - T'). \dots \dots \dots (3)$$

* *Comptes Rendus de l'Académie*, vol. xix. p. 169.

† Edinb. Phil. Trans. vol. xv.; and vol. xxi. part 2. p. 237.

‡ Hooker's Himalayan Journals, vol. ii.; and Edinb. Phil. Trans. vol. xxi. part 2. pp. 239, 240.

§ *Comptes Rendus*, vol. xix. p. 166. || *Ibid.* vol. xviii. p. 252.

(1) Locality.	(2) Barom. at 32° F.	(3) Observed boiling- point, F.	(4) Value of n.	(5) Computed boiling- point, n=112.	(6) Differ- ences.	(7) Observers.
Geneva	millims. 730·40	210·00	115·35	210·05	+0·05	Martins and Bravais.
Martigny	723·6	209·5	117·29	209·61	+0·11	Dr. Forbes.
Mont Pila	723·52	209·5	116·50	209·59	+0·09	M. Marie.
Mont Pila	716·45	209·05	114·53	209·11	+0·06	M. Marie.
Pyrenees.....	700·02	207·84	116·46	208·00	+0·16	M. Izarn.
Pyrenees.....	685·72	206·87	114·85	207·00	+0·13	M. Izarn.
Pyrenees.....	676·92	206·27	114·03	206·37	+0·10	M. Izarn.
Chamounix.....	673·99	206·08	113·21	206·14	+0·06	Martins and Bravais.
Mont Pila	666·94	205·48	114·57	205·52	+0·04	M. Marie.
Pyrenees.....	664·46	205·32	114·46	205·47	+0·15	M. Izarn.
Pyrenees.....	660·76	205·09	113·73	205·19	+0·10	M. Izarn.
Mont Pila	655·79	204·71	113·53	204·80	+0·09	M. Marie.
Mont Pila	645·99	203·88	114·77	204·07	+0·19	M. Marie.
Pyrenees.....	643·26	203·72	114·32	203·89	+0·17	M. Izarn.
Naversch	638·6	203·58	111·4	203·54	-0·04	Dr. Forbes.
Pyrenees.....	638·49	203·38	113·96	203·53	+0·15	M. Izarn.
Pyrenees.....	637·33	203·35	113·13	203·44	+0·09	M. Izarn.
Chungtam	in. 24·597	202·5	112·46	202·54	+0·04	Dr. Hooker.
Myrung	24·453	201·9	113·75	202·06	+0·16	Dr. Hooker.
Montanvert	millims. 612·3	201·33	113·69	201·49	+0·16	Dr. Forbes.
Montanvert	609·3	201·18	112·73	201·25	+0·07	Dr. Forbes.
Prarayon.....	606·9	200·96	113·0	201·06	+0·10	Dr. Forbes.
Darjiling.....	in. 23·358	199·6	114·09	199·83	+0·23	Dr. Hooker.
Tacul	millims. 588·1	199·48	112·43	199·53	+0·05	Dr. Forbes.
St. Bernard	575·9	198·46	112·4	198·51	+0·05	Dr. Forbes.
Breven	569·0	197·93	111·93	197·92	-0·01	Dr. Forbes.
Faulhorn.....	556·8	197·20	109·71	196·87	-0·33	Bravais and Peltier.
Faulhorn.....	554·3	196·76	111·23	196·65	-0·11	Bravais and Peltier.
Zemu Samdong	in. 21·605	195·9	112·93	196·03	+0·13	Dr. Hooker.
Col Collon	millims. 527·5	194·53	110·16	194·24	-0·29	Dr. Forbes.
Great Mulets	529·69	194·31	112·76	194·43	+0·12	Martins and Bravais.
Aiguille du Moine.	528·05	194·28	112·01	194·29	+0·01	Dr. Forbes.
Mainom	in. 20·480	193·4	112·19	193·43	+0·03	Dr. Hooker.
Yeumtong	19·49	191·1	111·58	191·02	-0·08	Dr. Hooker.
Great Plateau.....	millims. 478·39	189·62	111·25	189·46	-0·16	Martins and Bravais.
Tungu.....	in. 18·869	189·5	111·73	189·45	-0·05	Dr. Hooker.
Pichincha	17·208	185·27	111·26	185·09	-0·18	M. Wisse.
Mont Blanc	millims. 423·74	183·91	110·64	183·57	-0·34	Martins and Bravais.
Yeumtso	in. 16·385	183·2	109·64	182·58	-0·62	Dr. Hooker.
Sebolah Pass	15·928	181·9	109·48	181·21	-0·69	Dr. Hooker.

4. Now from equation (1) we have

$$n = \frac{T - T'}{\log B - \log B'}$$

and from Regnault's Table of Tensions, as corrected by Moritz, we obtain

$$T = 100^\circ \text{ C.} \dots B = 760 \text{ millims.} \dots dB = 27.212397, \\ T' = 80 \dots B' = 354.616,$$

by means of which we find for the standard boiling-point

$$n_{100} = \frac{dT}{d \log B} = \frac{BdT}{MdB} = 64.307626,$$

and between 80° and 100° C. ,

$$n_{80-100} = \frac{100 - 80}{\log 760 - \log 354.616} = 60.412836;$$

and since between these points n is found to vary almost exactly as the temperature, we may write for boiling-temperatures between 80° and 100° C. ,

$$n = 64.3076 - 0.19474(100 - T). \dots (4)$$

Hence

$$\log \frac{770 \text{ millims.}}{B} = \frac{5.13493(100 - T)}{230.215 + T} \dots (5)$$

Combining (2) and (5), and introducing the value of L for a standard atmosphere at 0° C. , the approximate height above the point where water boils at 100° C. is expressed in metres by

$$h_m = 94568^m \times \frac{100 - T}{230.215 + T}; \dots (6)$$

and approximately by

$$h_m = 285^m.54(100 - T) + 0.955(100 - T)^2. \dots (7)$$

5. Allowing for the difference of pressure between 760 millims. and 30 inches, we may represent the pressures on the English barometer corresponding to boiling-temperatures between 176° and 212° F. with great exactness by the formula

$$\log B = \log 30 \text{ in.} - 0.00864,1566(212^\circ - T) \\ - 0.00001,43365(212^\circ - T)^2 - 0.00000,00316,1(212^\circ - T)^3. (8)$$

This formula, which is of the form first used by Biot, will give the same results as the more complicated one of Regnault, when T lies between 172° and 216° F. From this we may at once derive the height in feet of the point at which water boils at $T^\circ \text{ F.}$, viz.

$$h = 521.684 \text{ ft.} (212^\circ - T) + 0.8655(212^\circ - T)^2 + 0.0019(212^\circ - T)^3;$$

or, as good approximations in two terms,

$$h = 520.48 \text{ ft. } (212^\circ - T) + 0.967 (212^\circ - T)^2,$$

or

$$h = 520.18 \text{ ft. } (212^\circ - T) + (212^\circ - T)^2.$$

But from equation (8) we also obtain

$$n_{212} = \frac{BdT}{MdB} = 115.71976;$$

and in the same manner as before,

$$n = 115.71976 - 0.194757 (212^\circ - T), \quad \dots (9)$$

and

$$\log 30 \text{ in.} - \log B = \frac{5.1346 (212^\circ - T)}{382.1744 + T}; \quad \dots (10)$$

and

$$\therefore h = 309971 \text{ ft.} \times \frac{212^\circ - T}{382.174 + T} \quad \dots (11)$$

$$\text{or } \log h = 5.491321 + \log(212^\circ - T) - \log(382.174 + T).$$

Or, avoiding the fraction in the divisor, we may employ the formulæ

$$h = 309880 \text{ ft.} \times \frac{212^\circ - T}{382 + T},$$

and

$$\log h = 5.491194 + \log(212^\circ + T) - \log(382 + T);$$

or

$$h = 308837 \text{ ft.} \times \frac{212^\circ + T}{380 + T} \quad \dots (12)$$

and

$$\log h = 5.489729 + \log(212^\circ - T) - \log(380 + T),$$

either of which will give almost identically the same results as equation (11), the last of the two (owing to a very small second difference in the value of n which has been neglected) being theoretically the most accurate of the three.

6. If the boiling-point is observed at two stations whose difference of level is required, writing $d = 212^\circ - T$, and $d' = 212^\circ - T'$, we have

$$h' - h = \frac{309971 (T - T')}{170.174 + T + T' + .00169 dd'}$$

and

$$\log (h' - h) = 5.491321 + \log (T - T') - \log \left(170.174 + T + T' + \frac{dd'}{600} \right); \quad (13)$$

but the term $\cdot 00169dd'$, or $\frac{dd'}{600}$ nearly, may be omitted, except when *both* stations are at great elevations. We may also use the constants 308837 ft. and 168° from equation (12) instead of 309971 ft. and $170^\circ \cdot 174$.

7. It only remains to introduce the corrections for the temperature of the air, &c. Putting t and t' for the temperature of the air at the lower and upper stations, and $\tau = \frac{1}{2}(t + t')$, also $a =$ the coefficient of the dilatation of the air for 1° F. as determined by Regnault, we have, by substitution in Bessel's formula for barometrical measurements, the corrected difference of level

$$H' - H = (h' - h) \times \frac{398 \cdot 37 [1 + a(\tau - 32^\circ)]}{397 \cdot 37 - a(\tau - 32^\circ)} \times \frac{1}{g} \quad (14)$$

g being the ratio of the force of gravity in the latitude λ of the stations to that in latitude 45° . Hence from equations (12) and (13)

$$H' - H = \frac{289358(T - T')}{168 + T + T' + \frac{dd'}{600}} \times (1 + 0 \cdot 00205\tau) \times \frac{1}{1 - 0 \cdot 0026 \cos 2\lambda}$$

and

$$\log(H' - H) = 5 \cdot 461436 + \log(T - T') + \log(1 + 0 \cdot 00205\tau) - \log\left(168 + T + T' + \frac{dd'}{600}\right) - \log(1 - 0 \cdot 0026 \cos 2\lambda),$$

in which the term $\frac{dd'}{600}$ need rarely be employed, and the correction for latitude never, except for great altitudes in low latitudes. Now for the sake of brevity let us write

$$h = 308837 \text{ ft.} \times \frac{212 - T}{380 + T}, \text{ and } A = \frac{398 \cdot 37 [1 + a(\tau - 32^\circ)]}{397 \cdot 37 - a(\tau - 32^\circ)} \times L;$$

then we have

$$H' - H = (h' - h) \times \frac{398 \cdot 37 [1 + a(\tau - 32^\circ)]}{397 \cdot 37 - a(\tau - 32^\circ)} \text{ nearly,} \quad (15)$$

and

$$\log(H - H') = \log(\log B - \log B') + \log A \text{ nearly,} \quad (16)$$

of which the terms $\log A$ and the multiplier $\frac{398 \cdot 37 [1 + a(\tau - 32^\circ)]}{397 \cdot 37 - a(\tau - 32^\circ)}$ may be tabulated in terms of τ , and h and $\log B$ in terms of T , as in the Tables appended.

8. Lastly, when the observations are taken at the upper station only, and consequently t' is known, and it is necessary to estimate τ , the mean temperature between the sea-level and the station, if we suppose the diminution of temperature, in terms of

the altitude in feet, to be generally conformable to the expression

$$\frac{h}{300 + \frac{h}{400}}$$

we may represent the difference of temperature by

$$\frac{653(T - T')}{375 + T - T'}$$

or, as nearly as the nature of the case admits,

$$\tau = t' + \frac{17}{20}(212^\circ - T')$$

Example.—Professor Forbes found the boiling-point on the Col d'Erin, between Evolena and Zermatt, to be $191^\circ.93$, and the air 34° F.; whilst the barometer at Geneva was 28.73 in., and the temperature 72° , to determine the height. Here,

by Table I. . . .	$h' = 10834$	feet.
by Table III. . . .	$h = 1134$	„
Difference	<u>9700</u>	„

$\tau = \frac{1}{2}(34 + 72) = 53$, multiplier in Table II. 1.04555 ;
 \therefore height above Geneva 10142 feet,
 and above the sea $10142 + 1343 = 11485$ „

By Logarithms.

Log 28.73 in. . . .	1.45834
Table I. log B' . . .	<u>1.29765</u>
Difference	0.16069 log . 9.20599
$\tau = 53^\circ$ F. . . .	Table II., log A. 4.80016
$H' - H = 10142$ ft. as before, log . . .	4.00615

And if we take the upper observation only into account,

$$\tau = 34^\circ + \frac{17}{20}(212^\circ - 191^\circ.93) = 34^\circ + 17 = 51^\circ,$$

and $h' = 10834$; hence by Table II.,

$$H' = 10834 \times 1.04145 = 11283 \text{ feet}$$

above the point where water boils at 212° , differing from the determination above, from both observations, by about 200 feet, or $\frac{1}{5.6}$ th of the height; but the observation at Geneva gives its height above this point as only 1235 feet instead of 1343 feet, indicating that the pressure of 30 inches at the time of the observation occurred about 108 feet above the sea-level.

TABLE I.—Logarithms of the Barometrical Pressures in inches of Mercury, with the Altitudes in feet, corresponding to Boiling-points between 176° and 215° F.

T.	Log B.	Differ-ences.	Heights in feet.	Differ-ences.	T.	Log B.	Differ-ences.	Heights in feet.	Differ-ences.
176	1·145960	+ 9780	19992	- 590	196	1·335056	+ 9109	8576	- 550
177	155740	9745	19402	589	197	344165	9077	8026	548
178	165485	9709	18813	586	198	353242	9046	7478	546
179	175194	9674	18227	584	199	362288	9015	6932	544
180	184868	9640	17643	582	200	371303	8984	6388	542
181	194508	9604	17081	580	201	380287	8953	5846	541
182	204112	9571	16481	578	202	389240	8923	5305	538
183	213683	9536	15904	576	203	398163	8892	4767	537
184	223219	9502	15328	574	204	407055	8862	4230	535
185	232721	9469	14754	571	205	415917	8832	3695	533
186	1·242190	9435	14183	570	206	1·424749	8802	3162	532
187	251625	9401	13613	568	207	433551	8773	2630	529
188	261026	9368	13045	565	208	442324	8743	2101	528
189	270394	9335	12480	564	209	451067	8714	1573	526
190	279729	9302	11916	561	210	459781	8684	1047	524
191	289031	9270	11355	560	211	468465	8656	523	523
192	298301	9237	10795	557	212	477121	8628	0	521
193	307538	9205	10238	556	213	485749	8598	- 521	519
194	316742	9173	9682	554	214	494347	8571	-1040	517
195	325915	9141	9128	552	215	502918	-1557	

TABLE II.

$\frac{t+t'}{2}$ Fahr.	Multiplier.	Log A.	Differ-ence for 1°.	$\frac{t+t'}{2}$ Fahr.	Multiplier.	Log A.	Differ-ence for 1°.
	Difference for 1° = ·00205.				Difference for 1° = 0·0205.		
10	0·95744	4·76193	93	52	1·04350	4·79931	85
12	·96154	·76378	92	54	·04760	·80101	85
14	·96564	·76563	92	56	·05170	·80271	85
16	·96973	·76747	92	58	·05580	·80440	84
18	·97383	·76930	91	60	1·05990	4·80608	84
20	0·97793	4·77112	91	62	·06400	·80776	84
22	·98203	·77294	90	64	·06810	·80943	83
24	·98662	·77475	90	66	·07220	·81109	83
26	·99022	·77655	90	68	·07630	·81275	83
28	·99432	·77834	89				
				70	1·08040	4·81440	82
30	0·99842	4·78013	89	72	·08450	·81604	82
32	1·00252	·78191	89	74	·08860	·81768	82
34	·00662	·78368	88	76	·09270	·81932	81
36	·01071	·78544	88	78	·09680	·82094	81
38	·01481	·78720	88	80	1·10090	4·82256	81
40	1·01891	4·78895	87	82	·10500	·82418	80
42	·02301	·79070	87	84	·10910	·82579	80
44	·02711	·79243	87	86	·11320	·82739	80
46	·03121	·79416	86	88	·11730	·82898	80
48	·03531	·79588	86	90	1·12140	4·83058	79
50	1·03940	4·79760	85	92	·12550	·83216	79
52	·04350	·79931	85	94			

TABLE III.—Altitudes in feet, corresponding to different heights of the Mercurial Barometer in inches and decimals, and at 32° F.

Barometer, in inches.	.0.	.1.	.2.	.3.	.4.
	feet.	feet.	feet.	feet.	feet.
14	19981·8	19795·3	19610·0	19425·9	19243·2
15	18172·9	17998·7	17825·7	17653·8	17482·9
16	16480·9	16317·5	16155·2	15993·8	15833·5
17	14891·4	14737·6	14584·8	14432·8	14281·7
18	13392·8	13247·6	13103·1	12959·5	12816·6
19	11975·3	11837·5	11700·5	11564·3	11428·9
20	10631·2	10501·8	10369·6	10240·1	10111·3
21	9351·3	9226·7	9102·8	8979·4	8856·6
22	8131·7	8012·8	7894·4	7776·5	7659·2
23	6966·2	6852·5	6739·2	6626·6	6514·3
24	5850·4	5741·4	5632·8	5524·7	5417·0
25	4780·1	4675·6	4571·3	4467·1	4363·9
26	3751·8	3651·2	3550·9	3451·0	3351·5
27	2762·3	2664·4	2568·8	2472·6	2376·8
28	1808·9	1715·4	1622·3	1529·4	1437·0
29	888·8	798·6	708·6	619·0	529·7
30	0·0	— 87·3	— 174·2	— 270·1	— 353·4
31	— 859·7	— 944·2	— 1028·3	— 1112·2	— 1195·8
Barometer, in inches.	.5.	.6.	.7.	.8.	.9.
	feet.	feet.	feet.	feet.	feet.
14	19061·9	18881·8	18702·7	18524·9	18348·3
15	17313·3	17144·6	16977·1	16810·6	16645·2
16	15674·1	15515·7	15358·2	15201·7	15046·1
17	14131·4	13982·0	13833·5	13685·8	13538·6
18	12674·5	12533·1	12392·6	12252·7	12113·6
19	11294·2	11160·2	11026·8	10894·1	10761·9
20	9983·1	9855·5	9728·6	9602·2	9476·5
21	8734·4	8612·7	8491·6	8371·1	8251·1
22	7542·4	7426·1	7310·4	7195·2	7080·4
23	6402·4	6291·0	6180·2	6069·8	5959·9
24	5309·8	5203·0	5096·6	4990·7	4885·2
25	4261·1	4158·4	4056·1	3954·3	3852·8
26	3252·4	3153·7	3055·3	2957·3	2859·6
27	2281·2	2186·1	2091·3	1996·8	1902·7
28	1344·8	1253·0	1161·4	1070·2	979·4
29	440·6	351·9	263·5	175·4	87·5
30	— 436·5	— 519·2	— 604·9	— 690·1	— 775·0
31	— 1279·2	— 1362·3	— 1445·1	— 1527·7	— 1610·0

V. *On the Motions of Camphor towards the Light.* By JOHN WILLIAM DRAPER, M.D., *Professor of Chemistry and Physiology in the University of New York.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE read with interest the very ingenious communication of Mr. Tomlinson, "On the Motion of Camphor towards the Light," inserted in the present (November) Number of the *Philosophical Magazine*.

In this communication Mr. Tomlinson considers the circumstances under which camphor crystallizes on the sides of glass vessels exposed to the sun, and the action of screens of tinfoil and of other materials in preventing or removing those crystallizations.

The conclusion at which Mr. Tomlinson arrives, respecting the cause of these results, is as follows:—"I think enough has been stated to prove that the motion of camphor, &c. towards the light is really the effect of heat. . . . The bottles exposed in or near a window will always have one side colder than the other, and this colder surface will determine the deposit." "Whatever protects the bottles from radiation, either wholly or in part, prevents the formation of deposits." This simple and satisfactory explanation he considers to be altogether new, remarking that he is "supporting a new theory against the united testimony of many philosophers," and adding, "my presumption might be pardoned if I ventured to propound an entirely new theory as to the motions of camphor, &c. towards the light."

The readers of the *Philosophical Magazine* are aware that several years ago I published some experiments on these camphor motions. Those experiments Mr. Tomlinson has carefully examined. They constitute, in fact, the avowed basis of his memoir. As to the conclusion I arrived at, Mr. Tomlinson remarks, "the result of Dr. Draper's elaborate inquiry was to multiply phenomena, and leave the theory as it was."

If Mr. Tomlinson will turn to page 135 of the Appendix of the work he has quoted, or, better still, to the *Philosophical Magazine* for February 1840, page 84, I think he will conclude that this statement is scarcely correct, and that I had done something more than leave the theory where it was. In a letter to the Editor of the *Philosophical Magazine*, I enumerated briefly the facts I had observed, and concluded by furnishing their explanation in the following words:—"Now can we explain these singular results on any other known principle than this—that the side of the jar nearest to the sun radiates freely the heat that it receives back again, while radiation is interfered with at the

other side; that, in point of fact, the anterior side is the colder, and the other the hotter?"

Such is the theory that I gave. It is the same as Mr. Tomlinson's. The only difference between us is this,—that the conclusion I came to more than twenty years ago he has arrived at and considers to be new today.

Of the correctness of that explanation I have never since had any doubt. To show how well it was understood at that time, I may add that the Editor of the *Philosophical Magazine* himself, in a foot-note to the communication in the Number of the Journal I have just quoted, made a suggestion for the purpose of removing one of the difficulties which was in its way.

Yours truly,

University, New York,
November 28, 1862.

JOHN W. DRAPER.

VI. *Mineralogical Notes.* By Professor MASKELYNE and Dr. VIKTOR VON LANG, of the *British Museum**.

[With Three Plates.]

1. *On Connellite.* By Nevil Story Maskelyne.

THE rare mineral Connellite has never had a satisfactory locality assigned to it. To an old specimen in the British Museum a label was attached which gave Chessy as its locality; but the aspect of the specimen, and the minerals it consisted of, were not those by which the mineralogist recognizes the products of that famous French working. All the few known specimens, on the contrary, bear their united testimony to a Cornish source of this beautiful mineral, from the manner in which a massive and sometimes crystalline cuprite interveining quartz, carries chrysocolla and often also cupreous arseniates as a coating, or in small deposits in its hollows and fissures. My attention had been attracted to the mineral by the observation that the minute blue velvet-like filaments which compose it seemed to possess resplendent planes. Placing some under the microscope, they proved to be distinct and beautiful crystals.

During last autumn, Mr. J. M. Williams showed me his splendid collection in Cornwall. I searched in it for specimens of Connellite, and succeeded in finding two or three drawers, through which were scattered a few specimens of this most rare and beautiful mineral. They were mingled with specimens of Liroconite and Copper-mica; and the drawers which had contained them previously to their removal to Carhays Castle were stated

* Communicated by the Authors.

by Mr. Williams to have been unopened for fifty years. With the specimens were a few original labels, which exhibited, however, only two kinds of inscription, viz. "Arsenate of copper, Huel Unity," and "Arsenate of copper, Huel Damsel." It was Professor Connell, after whom this mineral is deservedly named, who first proved its old designation as an arseniate of copper, to be erroneous. Messrs. Lettsom and Greg, in their 'Manual of British Mineralogy,' mention Huel Providence and Carharrack as reputed localities of this mineral. The latter locality, from its proximity to the old Huel Unity (both mines being now merged in what was till lately called the St. Day United Mines), has not improbably been a source of it; but the claim of Huel Providence to the production of Connellite is problematical.

The crystals of this mineral are extremely minute, the largest not possessing dimensions greater than $\frac{1}{200}$ th of an inch in thickness and about $\frac{1}{10}$ th of an inch in length. I have found one with both terminations perfect, but of considerably smaller dimensions than the above. The crystals belong to the hexagonal system, and exhibit a hexagonal pyramid formed of the two rhombohedra 100 and $\bar{1}22$, sometimes terminating the regular hexagonal prism ($10\bar{1}$); but more usually associated also with scalenohedra, the one of which is the inverse of the other, and the second hexagonal prism ($2\bar{1}\bar{1}$) truncating the edges of the first.

In crystals so minute, of which one presents as many as forty-eight planes on an area represented by the dimensions above given, it would be almost impossible to obtain measurements of any value by the ordinary reflecting goniometer.

But by means of a small plano-convex lens in front of a small telescope with a magnifying power of about nine times attached to the goniometer, and which converts that telescope into a sort of microscope of low power, it is not difficult to obtain measurements of considerable exactitude. The adjustment is aided by screw motions; and it is requisite to diminish the size of the aperture for the light as much as is consistent with the adequate illumination of the planes. When the faces are minute, and present a plane surface and lustre, the method is exact enough. But where the planes are curved, as is the case with the scalenohedra of Connellite, very accurate measurement is impossible.

The angles obtained from the more perfect planes of this mineral agree with an angular element of $(100)(111) = 53^\circ 10'$.

The planes on the crystal are

r 100, z $\bar{1}$ 22, a 10 $\bar{1}$, b 2 $\bar{1}$ $\bar{1}$, o 9 $\bar{4}$ $\bar{2}$, ω 46 $\bar{7}$.

	Found.	Calculated.
$rz =$	106 20'	106 20'
$rb =$..	36 50
$rr' =$	87 15	87 56
$ra' =$..	46 7
$rz'' =$	47 15	47 10
$rb'' =$..	66 25
$oo' =$	16 27	16 10
$oo'' =$..	101 16
$oa'' =$..	23 58
$ob =$	13	13 6
$or =$..	27 23
$o'z'' =$..	53 34
$o'\omega' =$..	42 50

The crystals (Pl. II. fig. 1) are perhaps those from Huel Unity. On a specimen from Huel Damsel, given me by Mr. Williams, I found the form in fig. 2; and specimens from that locality (so far as it was possible to assign particular labels to the specimens) seemed more slender and fibrous, in one case even asbestiform.

Examined under the microscope by polarized light, the plane of polarization was found to be parallel to the axis of the prism, and the crystal proved to be devoid of dichroism.

2. *On a Crystal of Columbite from Monte Video.* By
N. S. Maskelyne.

Some time since, W. Garrow Lettsom, Esq., Her Majesty's Minister and Consul-General at the Republic of Uruguay, sent to the British Museum a crystal of Columbite. It was found by him imbedded in granite in the neighbourhood of Monte Video. Mr. Lettsom's quick and accurate eye and intimate knowledge of minerals enabled him at once to recognize the true character of this crystal, and he extracted it from the rock containing it. Though split in the operation, the fragments, reunited, build up the entire crystal. He has searched persistently for others, but has not found one.

It is a small lustrous crystal with a very dark brown streak, and with a specific gravity of 5.660.

It presents the planes (see Pl. II. fig. 3)—

a 100	g 110	u 111
b 010	m 130	n 211
c 001	z 150	

I take the parameters of this mineral as assumed by Schrauf (*Sitzungsberichte der K. Akad. der Wissenschaften.* Wien, 1861,

p. 445). Professor Miller's indices are convertible into those of Schrauf by dividing the x and z indices in his treatise by 3.

Its specific gravity is high, coming near to, but being rather higher than that of the variety of the Ilmen Columbite, described by Hermann as possessing the highest specific gravity. He places this limit at 5.73. In other respects, too, the Monte Video Columbite exhibits a remarkable analogy with the Columbite of Ilmen.

Schrauf has figured (fig. 6 of his memoir) a crystal from the Ilmen Mountains, and in fig. 4 one from Haddam, both presenting much similarity to this crystal. They belong to the crystalline type which he has called "Habitus I.;" and it is this "habit" which the Monte Video crystal affects. There is, however, also a crystal of the Ilmen Mountain Columbite numbered 8 among the specimens acquired with the Allan-Greg collection by the British Museum.

It is a very perfect little crystal, and presents a near resemblance to that found by Mr. Lettsom. But its specific gravity is 5.969, a much higher one than even the highest assigned by Hermann to the Columbite from the Siberian locality, and in fact is that of some specimens from Bodenmais. But what gives an important interest to this specimen, is the circumstance that it carries a plane identical with a new plane on the Monte Video crystal, viz. (1 5 0).

The forms it exhibits are identical with those carried by the Monte Video crystal, with the exception that the plane n is absent (see fig. 4).

A locality producing Columbite is almost sure to prove one of much mineralogical interest when investigated. The analogy of the Monte Video specimen to those from the Ilmen Mountains may fairly justify the hope that so skilled a mineralogist as Mr. Lettsom may yet bring to light other interesting minerals from the Monte Video locality.

The existence of the plane (1 5 0) is established by the following angles, each of which is the mean of three measurements on various parts of the crystal:—

	Allan Greg, No. 8.	Monte Video.	Calculated.
(0 1 0)	(1 5 0) = 26° 23'	26° 20'	26° 9'
(1 3 0)	(1 5 0) = 13° 8'	13° 18'	13° 8'

There can be no doubt about the indices of this plane, as the corresponding angles for the plane (1 6 0) are

$$(0 1 0) (1 6 0) = 22^\circ 15' \text{ and } (1 3 0) (1 6 0) = 17^\circ 2'.$$

3. *On the Crystalline Form of Lanthanite.* By Viktor von Lang.

It has been hitherto supposed that lanthanite crystallizes in the pyramidal system; but M. Descloizeaux found recently, in the course of his important researches on the optical properties of minerals, that lanthanite possesses two optic axes, the mean line of which is perpendicular to the perfect cleavage plane. The crystals of lanthanite must therefore be considered to belong to the prismatic system. M. Descloizeaux, when in England this year, had the kindness to acquaint us with his discovery; and I tried in consequence to determine also the crystallographic elements of lanthanite, the mineralogical collection of the British Museum possessing small but tolerably good crystals of this mineral from Bethlehem in Pennsylvania.

The crystals were found to be combinations of a prism $m(110)$, a pyramid $o(111)$, and the two planes $b(010)$ and $c(001)$. As the plane c , which is at the same time a perfect cleavage plane, predominates very much, the crystals have the form of thin plates, the sides of which are bevelled by the small faces m and o , as is represented in figure 5 of Plate II.

From the measurements I made, the parameters of lanthanite were found to be

$$a : b : c = 1 : 0.9528 : 0.9468.$$

From these values the following angles are calculated:—

	Calculated.	Observed.
$110.010 = \circ$	\circ	$43^{\circ} 37'$
$110.\bar{1}10 = 87\ 14$	87 14	87 15
$110.111 = 37\ 24$	37 24	37 26
$111.11\bar{1} = \dots$	\dots	74 48
$111.010 = 54\ 53$	54 53	
$111.1\bar{1}1 = 70\ 14$	70 14	
$111.\bar{1}11 = 66\ 28$	66 28	

4. *On new forms of Mesotype.* By Viktor von Lang.

The minerals mesotype, mesolite, and scolezite all crystallize in prisms with angles varying little from 90 degrees, and hence present much similarity in their external appearance. Provided the crystals be not too small, the separation of the three species may easily be effected by the aid of the polarizing apparatus. Scolezite is recognized by the small angle which the plane of polarization makes with the direction of the prism. This angle

is zero for mesotype: placed in a cone of polarized light, we also find that in mesotype the first mean line must be parallel to the prism. Mesolite, on the contrary, shows in polarized light certain phenomena of interference, which were first observed by Descloizeaux, and which are certainly due to the complicated twin structure of these crystals. On submitting a prismatic crystal from a specimen which is labelled Brevicite in the mineralogical collection of the British Museum, to the test of polarized light, I found that the plane of polarization was indeed parallel to the prism, but that the plane of the optic axes went across that direction; and it was possible even to see one of the optic axes next the limit of the field of view.

The prism of this crystal, therefore, cannot be identical with the prism hitherto known on mesotype, but must be parallel to the axis *a*. I measured several similar crystals, and found that their planes are in simple relation to the planes which are already known on mesotype. What convinced me still more that I had to deal with crystals of this species was, that I found on the back of the same specimen other crystals, which, although exhibiting the well-known character of combination, showed also several of the new planes previously found on the former kind of crystals.

The prism parallel to the axis *a* would be expressed by the symbol (031) with the parameters used in the mineralogical treatise of Brooke and Miller. But as this form determines the character of combination, and as I even found a crystal twinned on this plane, I think it more convenient to give to it the simpler symbol (011), multiplying the parameter *c* with 3, which does not alter the symbol of the face (110).

For mesotype, therefore, the symbols of the known faces become

$$a(100) \quad m(110) \quad o(113) \quad y(313),$$

and those of the new faces

$$b(010) \quad c(001) \quad n(210) \quad h(101) \quad u(011) \quad z(111) \quad f(311).$$

Fig. 5, Plate III. shows the principal zones formed by these faces. Using the following angles given by Brooke and Miller,

$$\begin{aligned} ma &= 45^{\circ} 30', \\ mo &= 63^{\circ} 20', \end{aligned}$$

we find for the crystallographic elements the values

$$a : b : c = 0.9470 : 0.9306 : 1.$$

With these elements are calculated the angles of the following Table:—

	$a(100).$	$b(010).$	$c(001).$	$m(110).$	$h(101).$	$u(011).$	$z(111).$
$m(110)$	45° 30'	44° 30'	90° 0'	0° 0'	59° 24'	58° 31'	33° 34'
$n(210)$	26 58	63 2	90 0	18 32	49 40	70 37	37 49
$h(101)$	43 26	90 0	46 34	59 24	0 0	62 5	36 28
$u(011)$	90 0	42 56	47 4	58 31	62 5	0 0	35 44
$z(111)$	54 16	53 32	56 26	33 34	36 28	35 44	0 0
$o(113)$	71 40	71 20	26 40	63 20	32 33	32 32	29 46
$f(311)$	24 52	72 5	73 21	31 10	23 47	65 8	29 24
$y(313)$	48 7	76 10	45 11	50 41	13 50	48 36	22 38

The crystals are all very small (about one-tenth of an inch across) and colourless; they occur in the cavities of a white mass, probably of the same chemical composition. The crystals being elongated parallel to the axis c , are combinations of the faces $a b m n z o$, Pl. III. fig. 1. I found on a crystal of this kind the angles

$$mz = 33^\circ 30',$$

$$mo = 63^\circ 30'.$$

The face f was observed cutting off the edge formed by the two faces (111) and $(1\bar{1}0)$, and lying in one zone with them. I measured

$$(111)f = 59^\circ 17' \text{ (} 59^\circ 40' \text{ calc.)}$$

$$(111)(1\bar{1}0) = 91^\circ 14' \text{ (} 90^\circ 50' \text{ ,,)}$$

Considering that the face was not very perfect, I think these angles afford a sufficient ground for assuming the symbol of f to be (311) .

Another crystal on which the faces of the prism m were tolerably good yielded me the following values for the angles of the prism:—

$$(110)(1\bar{1}0) = 90^\circ 37'$$

$$(1\bar{1}0)(\bar{1}\bar{1}0) = 89^\circ 26'$$

$$(\bar{1}\bar{1}0)(\bar{1}10) = 91^\circ 1'$$

$$(\bar{1}10)(110) = 88^\circ 56'$$

Hence we see that, although the faces (110) and $(\bar{1}\bar{1}0)$ are pretty nearly parallel, as we find a mean value of

$$(110)(\bar{1}\bar{1}0) = 180^\circ 0',$$

the faces $(1\bar{1}0)$ and $(\bar{1}10)$ deviate sufficiently from parallelism to give

$$(1\bar{1}0)(\bar{1}10) = 180^\circ 30'.$$

This is another example, in addition to those found by Kock-

sharow, in which the planes of even well-developed crystals have undergone an alteration in their relative position.

One crystal of this kind was found to be a twin, fig. 2. The two individuals crossed one another nearly at right angles, and the faces a and v both were in one plane. The twin face is therefore parallel to $(0\ 1\ 1)$, as for this twin face the two individuals cross at angles of $94^\circ\ 8'$ and $85^\circ\ 52'$. The crystal was so small that it was impossible to measure angles formed by faces of the two individuals.

The crystals on which the prism $(0\ 1\ 1)$ predominates are combinations of the faces $abcnhuz$ (figs. 3, 4); the face c being always more developed than b , and the face z occurring only very seldom. None of these faces are very brilliant, and the worst of it is that the face u is always curved, so that it was found impossible to get an angle of the prism for measurement except by a rough approximation.

I found

$$\begin{aligned} na &= 26\ 16' \\ ha &= 44 \quad \text{appr.} \\ zu &= 35 \quad \text{,,} \end{aligned}$$

We see that the crystals of this kind are composed of faces which, with the exception of the face a , are new. As the angles could not be measured with great accuracy, it might be supposed that the face h is not a new face, but in fact identical with m . But this is contradicted by the results obtained from the optical properties exhibited by the crystal; for in the polarizing apparatus one finds that the axis of greatest optical elasticity must be parallel to the edge ah , and therefore this edge must be parallel to the axis b ; for the observations of Descloizeaux give the following symbol as representing the optical orientation,

$$\begin{array}{c} c\ b\ a; \\ + \end{array}$$

that is to say, the axis of smallest optical elasticity (which is at the same time the first mean line) is parallel to the greatest crystallographic parameter, the axis of greatest elasticity parallel to the mean, and the axis of mean elasticity parallel to the smallest parameter.

5. *Aërolitics*. By N. S. Maskelyne.

The branch of science that treats of Meteorites has acquired sufficient importance to justify our giving it a special name, and I therefore propose for it the denomination with which this article is headed. Many reasons conspire to render this study of "aërolitics" one of increasing interest, and to make it highly desirable that collections of meteorites should exist to illustrate it, as com-

plete as possible, not only in the numbers of the different falls they represent, but also as regards the modes in which the specimens are prepared for exhibition. These remarkable bodies will always command a general interest, from the fact that in them we see matter foreign in its origin and history to our own world, and handle, in them, the only tangible substances that belong to the space beyond our atmosphere. But the special interest attaching to a collection of them arises from the fact that, while they exhibit features of marked similarity, they withal, both as regards their constituent minerals, and the manner in which those minerals are mixed with each other, possess almost every one of them a very distinct individuality. Moreover, every day that the collection of specimens representing the older meteoric falls is deferred, adds to the difficulty of forming a complete series of them. It was on these accounts that the small but valuable collection that three years ago existed in the British Museum, has since that period been very largely increased. Towards the furthering of this object most valuable assistance has been rendered by Governors of Colonies and Indian Presidencies, who have exerted their authority with a liberality that has been in one case indeed rivalled by the patriotism of a valuable and learned body, the Asiatic Society of Calcutta. The result of this and of the considerable acquisitions made by purchase, has been that the aërolitic collection, which is an appanage to the Mineral Department in the British Museum, has now risen in point of material into the foremost place among such collections in the world*.

To accumulate so great a material is, however, but one step towards the end which should be held in view in the formation of a scientific collection. The next step consists in making that material available for the uses of science, partly by a proper preparation and exhibition of the specimens, partly by a complete description of them. I propose in this and subsequent papers to contribute something towards the last of these objects.

Yet when one approaches the subject with a view to undertake

* Every great collection has its own characteristic merits. If I may speak of that in the British Museum as the richest in material, taking the mass of the specimens as well as their numbers into consideration, it is with cordial pleasure that I express the highest admiration and respect for what I will not call a rival collection at Vienna. That collection is a classical one. Its specimens have been gradually collected, well described, and admirably exhibited. That aërolitics exists at all as a scientific subject is probably due to the existence of, and the care bestowed on, that collection. In the cause of science it is to be hoped that persons in authority in Vienna may not feel any jealousy of the rising collection in London, but may be ready to exchange, to the mutual advantage of both collections, duplicate specimens of aërolites not common to the two,

investigations in it, one cannot help feeling some disappointment, as well at the incompleteness of the chemical results that have been hitherto obtained, as at the unsatisfactory position of our knowledge concerning the origin and the sources of meteorites. Aërolitical science has to deal with the circumstances that attend the fall of a meteorite, no less than with its mechanical condition and its chemical composition; and from the data thus acquired it has to arrive at conclusions regarding the origin, the motion, and the cosmical relations of the foreign matter that thus wanders as it were into the atmosphere of our earth.

The general literature of the subject is becoming very considerable. Besides the tables and researches published by Mr. Greg in our own country, and besides many papers of Baron Reichenbach in Poggendorff's *Annalen*, Hofrath Haidinger has, by his active pen and energetic mind, contributed, in Austria, perhaps more valuable notices on the fall of meteorites than all other living authors; and Dr. Laurence Smith, as well as Prof. Silliman, by their accurate collection of facts and by their own investigations as chemists, have done much for the subject in America, where also the vigilant activity of Prof. Shepard has been conspicuous in collecting and distributing the specimens themselves.

The more special and exact literature, that, namely, which details the work done on meteorites in the laboratory, carries the names of the best inorganic analysts of this century, including those of Rose, Wöhler, and Rammelsberg. But if the progress thus far made in either the general or the special parts of the subject is not very large, it is at any rate enough to convince us that we see with tolerable clearness the questions to which we have to seek answers, and what are the cardinal points of interest raised by the presence of a meteorite on our globe, and by the circumstances attending its advent to it.

The chemical methods adopted for the analysis of a meteorite are probably unsatisfactory to every chemist who has employed them. The separation of the olivinoid and soluble felspathic from the insoluble felspathic, augitic, and other constituents, by the action of an acid, is necessarily incomplete; and the assignment of even empirical formulæ for the augitoid and felspathic ingredients is no less unsatisfactory. Yet in many meteorites it seems very difficult to conceive any better direct mode of operation. The intimate manner in which the different minerals are sometimes mingled, in what I may call a microscopic breccia, building the structure of the minute spherules in some of those belonging to the large group to which G. Rose has given the happy name of chondritic meteorites, and the excessive subdivision of the nickel-iron which in infinitesimal spangles is disseminated alike through homogeneous spherules and through those which present this

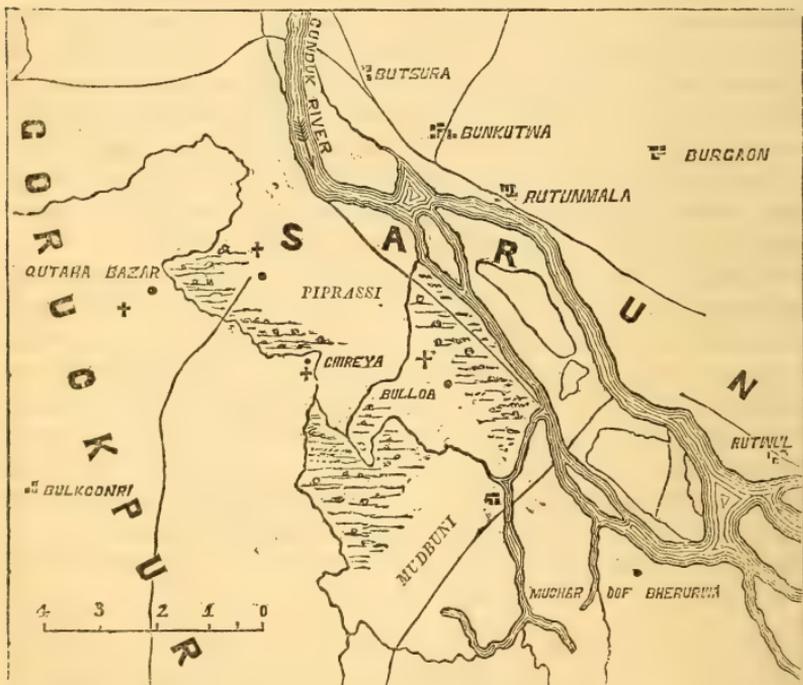
brecciated character,—these facts, which the microscope alone reveals, seem to bar the chemist from any complete mechanical separation of the ingredients of many meteorites, whether by the agency of a magnet, or by that of the selection, by the eye and hand, of distinct homogeneous particles*. Still there are cases in which analysis is possible; in some meteorites, as in that of Parnallee, the minerals are tolerably isolated from each other; and the fact that the chemist in dealing with such meteorites as those of Chantonay, Stannern, Luotolax, Bokkevelde, and Bishopville, is enabled to place each of them as the characteristic member of a group, may furnish ground for the hope that approximate methods may be found for at least determining the nature of the minerals contained in any given meteorite. One such method appears to be furnished by the microscope. A thin transparent section of an aërolite exhibits, under a low power, in a tolerably characteristic way the minerals of which the aërolite is composed. By comparing these minerals as thus seen and as observed *en masse* in the specimen with the minerals that predominate in certain well-investigated and, so to say, standard types of meteorite, one soon learns to discriminate between them, and to predicate of any given individual aërolite, with what others it presents mineralogical analogies, though the assignment to each of these minerals of its precise place as a mineral species is in some cases very difficult. Occasionally, however, as in the coarser-grained varieties, one is enabled to discriminate and to separate by mechanical selection for chemical analysis certain mineral ingredients in a state of considerable purity.

I have sought by these means to determine the lithological character, if I may so call it, of some of the undescribed meteorites in the British Museum. As a nomenclature is much wanted in our language to represent what is so completely expressed by the terms Meteorstein and Meteoreisen in the German, I propose calling the former (the meteoric stone) by the original term Aërolite, the meteoric iron by the term Aërosiderite, and the intermediate varieties (including the Pallasites of Rose), in which the iron is continuous and associated with silicate, by the term Aërosiderolites or Siderolites. The term meteorite would remain a generic expression for the whole.

* Probably it would be found practicable to determine the iron indirectly by the estimation of the hydrogen developed by the treatment of the aërolite with acids, under conditions convenient for collecting that gas. The sulphuretted hydrogen might be estimated at the same time; and even if it were all calculated as emanating from meteoric pyrites, the ultimate error in the analysis would be less than by a method in which the entire separation of the metallic iron is generally impossible, and the estimation of ferrous oxide therefore as often too high.

6. *The Fall of Butsura, May 12, 1861.*

The group of aërolites that fell on May 12, 1861, on the banks of the Gunduk, forty-two miles north-east of Goruckpur, presents features of a general interest that claim for it a prominent place among those to be described in these Notices. Five pieces of that group were sent to the Asiatic Society of Bengal, at Calcutta; and they have been thence forwarded to London, where they were exhibited during the period of the International Exhibition, at the British Museum. They have since been cut, in directions agreed upon by Mr. Oldham on the part of the Asiatic Society, and in accordance with a liberal and patriotic resolution of the Society to share with the National Museum in London their valuable acquisitions in Indian aërolites. These five stones fell at four distinct places south-west of the main stream of the Gunduk, near the village of Mudbuni and on the opposite side of the river to Butsura, which, as being the nearest place indicated on the Royal Atlas of Johnston, is perhaps the best to give its name to the fall (see Map). The four spots



where the aërolite fell are marked with a cross on the map, and form the angles of an irregular four-sided figure, one side of which runs nearly north-west and south-east, taking a direction parallel to the general course of this part of the Gunduk. The northern angle is very near to, and rather to

the north of, a little place called Piprassi; the south-eastern angle is a little to the north-west of one called Bulloah. These points are some three miles apart. Two very small fragments, weighing about 5 and 7 ounces respectively, fell at the latter locality. A thin slab-like piece fell at the former. It weighed about 11 pounds. Of the other angles, one is formed by a spot called the Qutahar Bazar (described in one account as in the Thannah of Nimboah); this is the north-westerly angle. The southern angle is at a spot called Chireya. The stones that fell at these two points respectively weighed 13 lbs. and $8\frac{3}{4}$ lbs. These points, like the former, are some three miles apart; and whereas Chireya and Piprassi are only two miles, the northern point, Bulloah, and Qutahar Bazar are some three miles distant from each other.

For the narrative of the circumstances accompanying the fall of these aërolites, I am indebted partly to Mr. Atkinson, the Secretary of the Asiatic Society, partly to my friend Dr. Oldham, the Director of the Indian Geological Survey.

The fall of the Qutahar Bazar and Chireya specimens was heralded by a report from out of a cloudless sky with a sound like that of ordnance, succeeded by several successive peals of seeming thunder. An appearance as of smoke was seen above the ground where they fell. One stone penetrated the soil for a cubit (=18 inches); the other did so to half that depth.

The two small fragments from Bulloah were accompanied by phenomena well substantiated by a near eye-witness. A native was taking his cattle to the water, when he was startled by three very loud reports, and saw in the air on high "a light" (a luminous body), which fell to the ground within 200 yards of him. Here too the sky was serene, and the weather fiercely hot, but there was a very small cloud, out of which this witness stated the report and the luminous body to have come. "First," he adds, "there was the loud report, and about the same time I saw the light like a flame; then the stone fell, and in falling made a great noise, and after it fell the sand was taken up high into the air." He went to the spot whence the sand had been raised from the ground, and found there five pieces of stone. They were very hot, and so was the sand all round, which was thrown up to the height of a foot. Unfortunately only two of these five fragments were preserved. Dr. Oldham further mentions that the incandescent fragments in falling are stated to have scintillated like iron when at a white heat.

The Piprassi stone was seen to fall by a witness quite independent of the other, but unfortunately from a much greater distance. In the midst of the calm hot day, while sitting in a field on the east side of the village of Piprassi, with many of the

villagers, he states that they were startled by three loud reports succeeded by a rumbling sound which gradually died away. Their attention was immediately arrested by a cloud of smoke, which rose from the ground at about 1000 yards from them. They saw nothing like a falling body, but they heard a whistling sound as of a bullet, but much louder. They went to the spot and found the stone, round which the gravel had been thrown up for some 2 feet. Fortunately the stone was not carried away, for nobody touched it for two days. It was Mahadeo!

Two hours after the fall, the serenity of the weather was interrupted by a storm accompanied by a little rain.

The reports of the explosion were heard at a distance of sixty miles from the locality.

Dr. Oldham, on sending these most interesting aërolites to England, accompanied them by remarkable observations of his own. The two little Bulloah fragments fit exactly together, and both fit on to the Piprassi stone. The Chireya stone in like manner fits with sufficient precision to that which fell at Qutahar. He surmised also that a careful adjustment would succeed in uniting all five fragments into a whole; and he indicated as a guide to this adjustment, a remarkable vein of iron which ran through the Piprassi and the Qutahar stones. I have since tried every possible means of effecting this; and though it is not practicable to find continuous surfaces of contact on the Piprassi and Qutahar stones, I have been enabled to determine the precise position they must have occupied relatively to each other, and have modelled and constructed an intermediate piece which, allowing contact of the stones at one part, builds the whole of the fragments into one large shell-like piece, obviously itself a fragment of some far larger mass. But this presents also another point of great interest. The Bulloah and the Piprassi stones, at the contact surfaces by which they fit together, exhibit no crust, though in other respects coated with it. The Chireya and Qutahar fragments, on the other hand, present a crust hardly, if at all, distinguishable from that covering the rest of their mass, on the very parts that form the faces of junction, and at which they fit with unquestionable precision. These surfaces indeed are smooth, and the edges very much rounded off, while those of the Bulloah and Piprassi stones fit together with the exactitude of adjustment with which the portions of a broken piece of oolite might be reunited.

Before attempting to draw conclusions from these facts, I will describe the general characters of the several fragments, in order that all the data offered by this aërolitic fall may be given in consecutive order.

The two that have been preserved out of the five stones that

fell at Bulloah are small fragments, fitting on, as before mentioned, to one of the long edges of the Piprassi stone. Probably the whole five formed a long bar-like piece fitting on to that edge, and these two would, in that case, constitute the half of it.

The Bulloah stones (numbered 1 and 2 in the figures on Plate IV.) are rounded along their summits and sides, and are there coated with a crust of a sooty black, and of dense texture. On the surface of contact they and the Piprassi are not crusted. The material of which the interior of the Bulloah stones is composed proves, when examined by a lens, to contain a profusion of protruding points of metallic iron. It presents a yellowish-brown ground-mass. It is mottled with irregular dark stains, which surround the metallic iron. This iron, associated with a considerable amount of meteoric pyrites, is present in this *aërolite* to a very high percentage. It is very evenly distributed in small, isolated, irregularly formed and sometimes crystalline-looking particles, not aggregated into a sponge, as in the siderolites, but, as in the beautiful *aërolite* of Akbarpúr, the grains of metal seem linked by a ferruginous or iron-stained mineral, which may possibly indicate the vestiges of a sponge-like structure of the iron at some earlier period in its history, when perhaps the silicates were less basic than at present, and less of the iron oxidized.

Besides these ingredients, there are several very irregularly-distributed spherules of a mineral of the greenish-brown colour and translucency, as well as the lustre, of dirty bees-wax. It is somewhat transparent in thin sections, and presents the characters of olivine.

A minute amount of iron pyrites occurs besides the meteoric pyrites; and a little of a very dark-coloured mineral is also present, generally with a lustrous fracture, and perhaps occasionally somewhat crystalline.

In a section under the microscope with a power of one-inch focus, this *aërolite* does not prove to be a very remarkable one. The mass of it seems to consist of olivine. This is associated with a grey mineral, and also with one that is of an opaque white. This grey mineral in some cases seems to constitute entire nodules of the *aërolite*, and sometimes seems mingled in the sort of brecciated mass, containing olivine crystals, that forms other nodules in it. It presents the appearance, in the former case, either of a dark mottled surface spangled with dark points (consisting sometimes of iron, and in some cases curiously distributed, as if spurted through the mass from a centre), or of a mineral presenting very regular and minute parallel cleavage-planes with dark grey bars running along them, often rayed out like a fan, and with cross cleavages usually oblique, but at angles

which vary with the inclination of the section to the axis of the crystal.

There is also another mineral, transparent and presenting cleavages nearly perpendicular to each other, which appears to be distinct from the foregoing.

What these minerals thus associated in small proportion with the olivine may be—whether they are solely augitic, or whether also the long felspathic-looking bars are really fragments of some felspar—is at present difficult to say with certainty. But in a subsequent article in these Notes I purpose giving all the data I possess for assigning to these and other meteoric minerals their true mineralogical character.

The Butsura fall, therefore, seems, like other aërolites rich in iron, to approach in character to a siderolite in that the silicates it contains consist, as I believe, for the most part of olivine. This olivine is generally very transparent, and comparatively colourless; but near the iron particles, and forming a continuous fringe to them, its granules become of a ferruginous colour, and are at times, especially in parts of the Qutahar and Chireya stones, red, like fragments of garnet or zircon.

The meteoric pyrites is present in a ratio of about one-half the apparent quantity to that of the iron. It is generally in little independent particles of the same average size as those of the iron; and it sometimes is continuous with the iron in the same particle, like the copper and silver of Lake Superior.

The Bulloah stone exhibits less of the ferruginous olivine than the others around the iron, and may perhaps contain more of the barred and grey mineral or minerals. The result is a paler hue on it. Its crust, on the other hand, is thicker and coal-black, that on the other stones having a browner cast.

But the specific gravity of the aërolite seems pretty constant in its different parts, namely about 3.60.

The next stone in order to the fragments that fell at Bulloah is the thin slab-like piece that fell at Piprassi, marked 3 in the figures. One of the faces of this piece is convex, while the other side presents a somewhat hollowed form. It is nearly rectangular in its general outline. The inner, as well as the outer, side presents some large but shallow hollows or "pittings." This piece, as before observed, does not fit on directly to the great mass that fell at Qutahar Bazar; but that it formed a closely contiguous part to it on the original aërolite there can be no doubt. In fact the general contour of the stone, the correspondence of the outline and character of the shallow hollows on both, and, finally, the existence in them of the remarkable vein of nickel-iron before alluded to, and which runs persistently in one plane through each of them from the

top to the bottom,—these all serve as guides to the restoration of the original form of the *ærolite*, so far as these two parts of it are concerned, and are the grounds of justification for the restoration of this part of the meteorite which I have attempted, by moulding the small intermediate piece, to unite the two stones, and which is marked on the figure with the number 4. The Qutahar stone, which becomes thus adapted to the Piprassi piece, is a fine mass of an irregular wedge shape (it is numbered 5). The inner side is fitted with large shallow depressions, and presents a rather concave surface. The outer side is flat and smooth. The base on which it stands, and which is the result of the wedge-like form, is also smooth, rounded at the edges, and presenting hollows and irregularities on one half of its surface, while to the side of this base, on the inner or just below the concave part of the stone, the irregular piece that fell at Chireya adapts itself. This last fragment (numbered 6) is somewhat pitted and deeply grooved on its upper side, and rounded everywhere else. Indeed, notwithstanding the precision with which it fits to the Qutahar stone, the faces and edges at the parts of contact are rounded off so as almost to obliterate the original form of the stones at this part. The contour presented by the reconstructed mass, so far as the reuniting of these scattered fragments enables one to build it up, is that of a shell or the thick outer rind of one side of a considerable *ærolite*.

The lithological character of the Piprassi, Qutahar, and Chireya stones is very similar to that of the Bulloah pieces. But there are differences between them worthy of being noticed. Thus the crust on them is not very different from the dense black crust that coats those of Bulloah; it is, however, less characteristic and less thick. They are all dull, as the crusts on highly olivinous meteorites generally are, as contrasted with the shining enamels on the felspathic-augitic kinds. It exhibits crystalline metallic-looking points, as well of iron as of meteoric pyrites and, at very rare intervals, of iron pyrites, that are disseminated among small globular projections of a pitch-black colour. It is these black projections, on the other hand, that constitute the whole mass of the Bulloah crust. But in the three larger masses the crust assumes a dirty blackish-brown hue.

The facts above recorded appear to me to throw some light upon several interesting questions.

We may hazard a pretty safe conjecture as to the direction of the Butsura fall, by observing that the lighter stones fell to the S.E. of the heavier ones; the Bulloah three miles S.E. of Piprassi; the Chireya a similar distance E.S.E. of Qutahar. If we suppose that the disruption of all the stones was simul-

taneous, we might further assume that they fell with a diverging flight; for the Qutahar Bazar and Piprassi points are considerably further asunder than those of the Bulloah and Chireya falls. In fact, a line passing from the E.S.E. to W.N.W. would represent the direction of the flight of the *aërolite*; and if we are to judge by the different divergences of the stones, that flight would not have been at a great inclination to the horizon.

Had it been quite horizontal, the point of divergence would have been, on this view, about seven miles E.S.E. of the central point of the fall, and two miles N.W. of Mudbuni. As, however, it would seem to have fallen from a considerable elevation, it may have been much further off, though the point of disruption would have been somewhere nearly vertical over the position thus indicated.

But this fall is remarkable for the evidence it affords of the incrustation of an *aërolite* subsequently to its disruption, as well as of the probability of successive disruptions, of which one, at least, was not followed by incrustation. In the great Parnallee *aërolite*, and still more in that which fell at Bustee, we have cases, of which indeed every collection must exhibit some more or less evident examples, showing crusts on different parts of an *aërolite* that seem not to have been contemporaneous—where, in fact, the crust on one part has not the thickness and homogeneity that characterizes that on another part. The following, in the British Museum collection, are cases in point: Stannern, Bokkeveldt, Benares, l'Aigle, and Mezö-Madaras. These facts are among those we have to explain. On the present occasion they were accompanied, according to every witness, by reports in the air, and by a subsequent roll of thunder. In two cases the distinct reports were three in number. There was a cloud in the sky, out of which the *aërolite* seemed to descend; while at Bulloah the stone or stones were seen to fall as a luminous body, which at some part of its path appeared to scintillate in the air. The shell-like form, too, of the united fragments, in suggesting the idea of an internal core or mass from which the external pieces have been severed, recalls to mind the suggestion of Mr. Benjamin Marsh, that the bursting of the meteorite is the result of the expansion produced by heat. If we couple with Mr. Marsh's suggestion the remarkable explanation by Hofrath Haidinger of the intense coldness declared to have been exhibited by the Dhurmsala stones after their having fallen quite hot, I believe that suggestion will prove a very fertile one. The coldness of cosmical space must be shared by bodies wandering therein without atmosphere.

Such a body entering with planetary velocity the terrestrial atmosphere, is arrested in its course with an abruptness of which

it is as difficult to get a clear conception as is that velocity itself. The intense heat instantaneously developed on the surface of the mass will assuredly be sufficient to melt that surface down into an enamel, before it could have time to penetrate to even a sensible depth into the body. If, as is probable, this fused and white-hot enamel flies off from the mass as it proceeds with the scream of a huge projectile through the air, its place will be continually taken by a fresh and continuously flowing stream of the same incandescent material. That material, too, is combustible. The metallic iron in many an aërolite ranges above 20 per cent., and is associated with sulphur as pyrites, and sometimes in other forms. Here at least is cause for much, if not a sufficient cause for the whole, of the spectacle exhibited by the blaze of a meteor. That the air itself is also heated to whiteness, and as such becomes visible, as Haidinger suggests, is highly probable, and would add still more to the brilliancy of the light.

But while the enormous velocity of the body is thus instantaneously arrested and converted into heat, the effect of that heat will not be exhibited in the molten spray of enamel alone. The heated surface will gradually, but by no means slowly, impart its heat to the interior; and notwithstanding the non-conducting character of the stony ingredients of an aërolite, the outer portions (a sort of shell around it) will rapidly rise in temperature. The coldness of the interior would only gradually be overcome, and, long before it would be so, the expansion of the external parts would tend to tear them away from a contracted and far more than ice-cold core within. The limits and the form of that core, the conditions under which disruption would ensue (indeed, whether it would ensue at all, as it would not if the mass were absolutely homogeneous), would depend on the structure of the mass, its directions or planes of weaker aggregation, or perhaps the unequal distribution in it of matter of various degrees of conductivity. But when the disruption comes, it must come with explosion.

The process may be repeated, or it may take place at different intervals on the different sides of the meteorite. The earlier explosions may take place at points in its path where there is still velocity enough to produce a fresh enameling,—sometimes in a copious flow, at others only enough to barely glaze the exposed surface of the stone again; the later ones may occur when the velocity is more nearly spent, and the friction is no longer competent to generate the glaze.

The cloud in the air, out of which the meteorite has been seen to come in so many authenticated instances, would be satis-

factorily explained by the dust of the enamel after its separation from the *ærolite* in its course, and the combustion of its iron, sulphur, &c. ; perhaps, also, small fragments are splintered and fly off by the same principle as the larger ones, and, partially burning, become dust too.

Following in the track of the body, this dust would soon, however, linger behind it and hang in the air like a vapour-cloud, as is often seen to be the case in the wake alike of a meteor and of a meteorite.

Finally, if the reports represent the successive concussions of the air produced by the disruption of the *ærolite* (and reaching the ear generally in the inverse order of their occurrence in time), we must attribute the "thunder" that is so often described as succeeding the reports, to the echo of the reports themselves.

That a noise, the true extent of which is likely to be exaggerated, should be heard over so large a range of country as sixty linear miles, is perhaps not so surprising when we consider the distance to which a small cannon can be heard, even over a surface of country teeming with obstacles and air-currents calculated to impede the passage of the sound ; whereas from a height of two or three miles in a still, clear air, the spread of even a comparatively small sound over an area with a radius of thirty or forty miles seems nothing astonishing. To me, at least, who have heard the roar of a train between Shrivensham and Swindon, as I stood, on a still night, in the station at Cirencester, a distance of certainly nearly twenty miles, such a wide promulgation of a sound in the air is no more difficult to understand than it is to credit the assertion of our *æronauts*, who a few months back heard a musical instrument played on the earth when their balloon was some three miles above the ground. That this propagation of the sound of a cannon or a train is not due to the conduction of the earth, is proved by the fact that it is only in certain states of the atmosphere, independent of wind, that it occurs.

The cause to which I have assigned the disruption of a meteorite, and the reports which accompany it, may also furnish an explanation of the great size of some *ærosiderites* and *siderolites*, as compared with that of the largest stones. The more rapid conducting power of the metal, as well as its greater power of resisting a *divellent* force, would—perhaps after a first disruption—tend to prevent the repeated breaking up of the mass ; and this may be the case in many instances, notwithstanding the fact that in others meteorites of this kind have fallen in associated and perhaps dis severed masses or even in showers.

VII. *On a Tactical Theorem relating to the Triads of Fifteen Things.* By A. CAYLEY, Esq.*

THE school-girl problem may be stated as follows:—"With 15 things to form 35 triads, involving all the 105 duads, and such that they can be divided into 7 systems, each of 5 triads containing all the 15 things." A more simple problem is, "With 15 things, to form 35 triads involving all the 105 duads."

In the solution which I formerly gave of the school-girl problem (*Phil. Mag.* vol. xxxvii, 1850), and which may be presented in the form

	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>	<i>g</i>
<i>abc</i>				35	17	82	64
<i>ade</i>		62	84			15	37
<i>afg</i>		13	57	86	42		
<i>bdf</i>	47		16		38		25
<i>bge</i>	58		23	14		67	
<i>cdg</i>	12	78			56	34	
<i>cef</i>	36	45		27			18

(viz. the things being *a, b, c, d, e, f, g, 1, 2, 3, 4, 5, 6, 7, 8*, the first pentad of triads is *abc, d 35, e 17, f 82, g 64*, and so for all the seven pentads of triads), there is obviously a division of the 15 things into (7 + 8) things, viz. the 35 triads are composed 7 of them each of 3 out of the 7 things, and the remaining 28 each of 1 out of the 7 things, and 2 out of the 8 things: or attending only to the 8 things, there are 28 triads each of them containing a duad of the 8 things, but there is no triad consisting of 3 of the 8 things. More briefly, we may say that in the system there is an 8 without 3, that is, there are 8 things, such that no triad of them occurs in the system.

I believe, but am not sure, that in all the solutions which have been given of the school-girl problem there is an 8 without 3.

Now, considering the more simple problem, there are of course solutions which have an 8 without 3 (since every solution of the school-girl problem is a solution of the more simple problem):

* Communicated by the Author.

but it is very easy to show that there is no solution which has a 9 without 3. I wish to show that there is in every solution at least a 6 without 3. This being so, there will be (if they all exist) 3 classes of solutions, viz. those which have at most (1) a 6 without 3, (2) a 7 without 3, (3) an 8 without 3. I believe that the first and second classes exist, as well as the third, which is known to do so.

The proposition to be proved is, that given any system of 35 triads involving all the duads of 15 things; there are always 6 things which are a 6 without 3, that is, they are such that no triad of the 6 things is a triad of the system. This will be the case if it is shown that the number of *distinct* hexads which can be formed each of them containing a triad of the system is less than

$$\left(\frac{15 \cdot 14 \cdot 13 \cdot 12 \cdot 11 \cdot 10}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5 \cdot 6} = 5 \cdot 7 \cdot 11 \cdot 13 = \right) 5005, \text{ the entire number}$$

of the hexads of 15 things. Now joining to any triad of the system a triad formed out of the remaining 12 things (there are

$$\frac{12 \cdot 11 \cdot 10}{1 \cdot 2 \cdot 3} = 4 \cdot 5 \cdot 11 = 220 \text{ such triads), we obtain in all}$$

$(220 \times 35 =) 7700$ hexads, each of them containing a triad of the system. But these 7700 hexads are not all of them distinct. For, first, considering any triad of the system, there are in the system 16 other triads, each of them having no thing in common with the first-mentioned triad. (In fact if *e. g.* 123 is a triad of the system, then the system, since it contains all the duads, must have besides 6 triads containing 1, 6 triads containing 2, and 6 triads containing 3, and therefore $35 - 1 - 6 - 6 - 6 = 16$

triads not containing 1, 2, or 3.) Hence we have $\left(\frac{35 \cdot 16}{2} = \right) 280$

hexads, each of them composed of two triads of the system; and since each of these hexads can be derived from either of its two component triads, these 280 hexads present themselves twice over among the 7700 hexads.

Secondly, there are in the system seven triads containing each of them the same one thing, *e. g.*

123, 145, 167, 189, 1.10.11, 1.12.13, 1.14.15, containing each of them the thing 1. That is, we have

$$\left(\frac{7 \cdot 6}{2} = \right) 21 \text{ pairs such as } 123, 145 \text{ containing the thing 1,}$$

and therefore $(15 \times 21 =) 315$ pairs such as $\alpha\beta\gamma, \alpha\delta\epsilon$. And for any such pair, combining with $\alpha\beta\gamma\delta\epsilon$ any one of the remaining 10 things, we have 10 hexads $\alpha\beta\gamma\delta\epsilon\zeta$, each of them derivable from either of the triads $\alpha\beta\gamma, \alpha\delta\epsilon$; that is, we have $(315 \times 10 =) 3150$ hexads which present themselves twice over among the 7700

before-mentioned branch is this conic; that is, the conic APA by its intersection with an arbitrary plane traces out a conic; or, what is the same thing, the sheet, the locus of the conic APA', is met by an arbitrary plane in a conic, that is, the sheet is a surface of the second order; and the given surface thus includes a surface of the second order, and is therefore made up of two surfaces of the orders m and 2 respectively. The demonstration seems to me to add at least something to the evidence of the theorem asserted, but I should be glad if a more simple one could be found. Analytically, the theorem is—"If

$$(x, y, z, \alpha x + \beta y + \gamma z)^{m+n},$$

where (α, β, γ) are arbitrary, break up into factors $(x, y, z)^m$, $(x, y, z)^n$, rational in regard to (x, y, z) , then $(x, y, z, w)^{m+n}$ breaks up into factors $(x, y, z, w)^m$, $(x, y, z, w)^n$, rational in regard to (x, y, z, w) ." It would at first sight appear that (α, β, γ) being arbitrary, these quantities can only enter into the factors of $(x, y, z, \alpha x + \beta y + \gamma z)^{m+n}$ through the quantity $\alpha x + \beta y + \gamma z$; that is, that the factors in question, considered as functions of $(x, y, z, \alpha, \beta, \gamma)$, are of the form

$$(x, y, z, \alpha x + \beta y + \gamma z)^m, \quad (x, y, z, \alpha x + \beta y + \gamma z)^n;$$

and then replacing the arbitrary quantity $\alpha x + \beta y + \gamma z$ by w , the factors of $(x, y, z, w)^{m+n}$ will be $(x, y, z, w)^m$, $(x, y, z, w)^n$. But this reasoning proves too much; for in a similar way it would follow that if $(x, y, \alpha x + \beta y)^{m+n}$, where α, β are arbitrary, breaks up into the factors $(x, y)^m$, $(x, y)^n$, rational in regard to (x, y) (and quâ homogeneous function of two variables *it always does so break up*), then $(x, y, z)^{m+n}$ would in like manner break up into the factors $(x, y, z)^m$, $(x, y, z)^n$, rational in regard to (x, y, z) . And a simple example will show that it is not true that the factors of $(x, y, \alpha x + \beta y)^{m+n}$ only contain (α, β) through $\alpha x + \beta y$; in fact, if the function be $= x^2 + y^2 + (\alpha x + \beta y)^2$, then the factor is

$$\frac{1}{\sqrt{\alpha^2 + 1}} \{(\alpha^2 + 1)x + (\alpha\beta + i\sqrt{\alpha^2 + \beta^2 + 1})y\},$$

which cannot be exhibited as a function of $\alpha, \beta, \alpha x + \beta y$. I am not acquainted with any analytical demonstration; the geometrical one cannot easily be exhibited in an analytical form.

IX. *Experiments on the Calorific Conductibility of Solids.* By F. NEUMANN, *Professor of Physics in the University of Königsberg**.

I WILL first describe in a few words the method which I have used. First, I do not observe the stationary, but the variable condition of temperatures; and it is the rapid convergence of the trigonometric series by which this variable state is represented, which enables it to serve for the determination of conducting powers; the first terms of the series are sufficient, when the time t is tolerably large: secondly, the bars used have not to undergo any deformation which escapes calculation: thirdly, this condition is realized by introducing into the bar to be studied very slight thermo-electric piles, formed of an iron wire and a german silver wire, which are soldered in the bar; as long as the temperatures of the solderings are between 0° and 100° , the intensity of the current is proportional to the differences of temperatures; and this law differs so little from the truth, that it may be assumed to be exact: fourthly, *the absolute values of the internal and external conductibilities may be determined simultaneously* by my method.

The bars used were 3 or 4 lines in the sides. The two thermo-electric rods were soldered at a short distance from the end of each bar, and their wires were connected with a differential galvanometer of a special construction, by means of which the sum and the difference of the two currents which traverse the piles may be exactly measured.

One of the ends of the bar thus prepared, was heated by a flame until the temperatures attained the state of equilibrium; the flame was then removed, and at the expiration of a certain time the sums and the differences of the intensities of the two currents were observed; they were thus obtained as a function of the time, the isochronous oscillations of a magnetized needle serving as chronometer. The duration of the oscillation of my apparatus was eight seconds, and I could therefore determine the intensities of the currents every eight seconds. It will be already seen that the differences of the intensities depend essentially on the internal conductivity, while the sums correspond to the external conductivity; and analysis leads in fact to this result, that the ratios of the sums successively observed are expressed by the same constant, and the successive differences by another constant. The combination of these two constants gives then the absolute values of the two conducting-powers.

In some experiments I also replaced the bars by rings, the rods being then fixed in two points diametrically opposite.

* Translated from the *Annales de Chimie et de Physique*, October 1862.

The first observations which I made in this way, three years ago, only refer to the course of the experiments; but then it was necessary to use at least two bars of different lengths, or rather two rings of different diameters, to determine at one time the coefficients of external and of internal conductibility. I found in this way the following numbers for the internal conductibility of some metals, of which I determined at the same time the specific gravity, and the relative electric conductibility.

	Internal calorific conductibility.	Specific gravity.	Electric conductibility.
Copper	1306	8·73	73·3
Brass	356	8·48	17·9
Tin	362	7·19	21·1
German silver	129	8·54	6·4
Iron	193	7·74	10·2

I took for the electric conductibility M. Wiedemann's number 73·3; his law of the proportionality of the two coefficients of conductibility is tolerably well verified by my results, as may be judged by the ratios of my numbers, which are respectively 17·6, 19·8, 17·1, 19·9, 18·9; mean 18·7: the small differences from this mean are the less astonishing, as my observations do not refer to the same temperature. It must also be remembered that M. Becquerel found 136 for the calorific conductibility of copper, M. Lenz 73, M. Riess 66, M. Bust 94, that of silver being 100.

My units are the minute, the Paris line, and the quantity of heat capable of raising the temperature of a cubic line of water by 1°*. When I shall have subjected all my results to a uniform discussion, it is possible that the numbers may undergo some slight modification.

To investigate badly-conducting substances I had recourse to another method. I made cubes 5 or 6 inches on the side, or spheres of the same diameter. These bodies, having been uniformly heated, were allowed to cool in the air; and when this cooling had lasted a certain time (which was from half an hour to an hour), the temperatures at the centre and at the surface were observed, by means of the thermo-electric rods already mentioned. Their influence on the course of the temperatures may always be estimated by calculation. In this way I obtained the following values for the ratio $\frac{k}{cD}$, k being the calorific conductibility, c the specific heat, and D the density.

* To reduce the above values to Peclet's units or to those of M. Ångström, it is necessary to multiply them respectively by 0·0848 or by 0·0509.

Substance $\frac{k}{cD}$.

Oil	1·37
Melted sulphur	1·68
Ice	13·5
Snow	4·2
Frozen soil	10·8
Sandstone	16·0

For oil we may call $cD=0\cdot26$, which will give $k=0\cdot35$ for this substance; and its electric conductivity will be $0\cdot02$ if it be shown that Wiedemann's law is general. Dr. Matthiessen has found $0\cdot04$ for metallic carbon.

For ice we have

$$cD=0\cdot5,$$

whence

$$k=6\cdot7.$$

The relative values of snow and frozen earth may serve for a future theory of the freezing and thawing of the earth's surface, which may lead, perhaps, to an explanation of the glaciers of Siberia. Rocks present marked difficulties in respect of their calorific conductivity; and it is indispensable to know them to account for the motion of heat in the interior of the globe.

Thus, I found $\frac{k}{cD} = 12\cdot9$ in a coarse-grained granite, and $=7\cdot0$ in a serpentine from Töplitz.

The only serious difficulty which prevents perfectly accurate results from being obtained by this kind of observation, is the very distinct variation of the conductibilities with the temperature. Unfortunately the theory of heat is still, so to say, in its infancy as concerns the laws according to which these variations take place.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxiv. p. 560.]

March 27, "THEORETICAL Considerations on the Conditions 1862. under which the Drift Deposits containing the Remains of Extinct Mammalia and Flint-implements were accumulated; and on their Geological Age." By Joseph Prestwich, Esq., F.R.S., F.G.S.

April 3.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"On the Law of Expansion of Superheated Steam." By William Fairbairn, Esq., LL.D., F.R.S., and Thomas Tate, Esq.

In a former paper selected for the Bakerian Lecture, entitled *Phil. Mag.* S. 4. Vol. 25, No. 165, Jan. 1863. F

“Experimental Researches to determine the Density of Steam at different Temperatures, and to ascertain the Law of Expansion of Superheated Steam” (Phil. Trans. 1860, p. 185), it was shown that although Dumas, Gay-Lussac, and other distinguished physicists had determined the density of steam at 212°, it was, however, left for these researches to ascertain the law of density, volume, &c. at all temperatures, and also the law of expansion of superheated steam. These experiments have therefore been continued, and have elicited remarkable results as regards the rate of expansion at various temperatures.

The earliest experiments on this subject were made by Mr. Frost in America, but without sufficient accuracy to be of scientific value. Mr. Siemens has also experimented on steam isolated from water; his results give a much higher rate of expansion for steam than for ordinary gases; but, owing to some obvious defects of Mr. Siemens’s method of conducting the experiments, we consider his results are not reliable.

For gases, the rate of expansion is expressed by the formula for constant volume,

$$\frac{P}{P_1} = \frac{E+t}{E+t_1}, \dots\dots\dots (1)$$

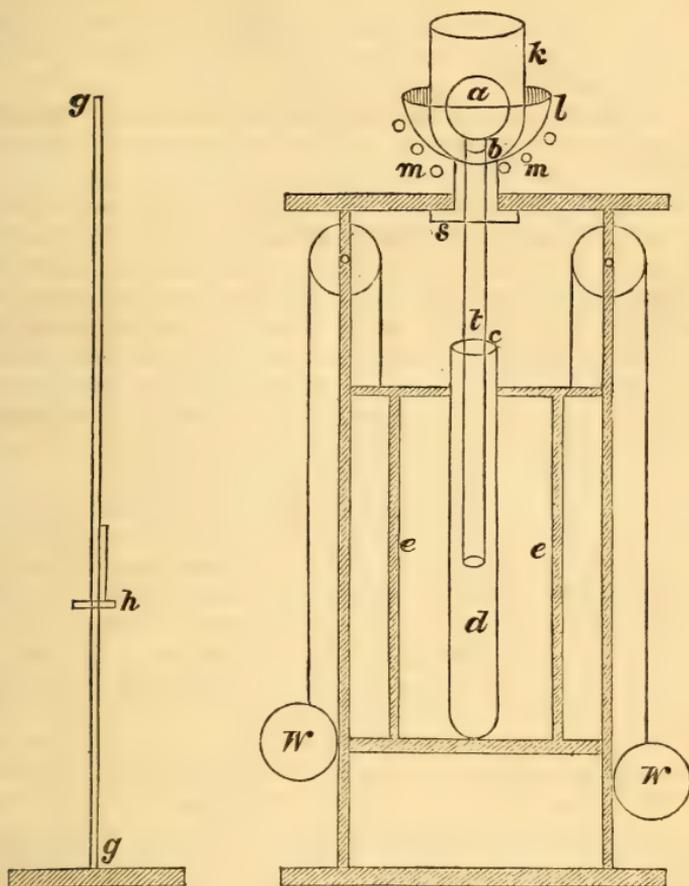
where E is a constant determined by experiment, and decided by Regnault as 459 in the case of air. In the paper alluded to, it was shown that, with a certain proviso, the rate of expansion of superheated steam nearly coincided with that of air. Within a short distance of the maximum temperature of saturation the rate of expansion of steam was found to be exceedingly variable; near the saturation-point it is higher than that of air, and decreases as the temperature is increased, until it becomes sensibly identical with that of air. The results upon which this law was based were too limited in their range for much numerical accuracy in the constants deduced.

Hence it has been our object in the present paper to supply the deficiency in the previous one, by affording experimental data of the expansion of steam at higher temperatures and with a greater range of superheating than was possible with the apparatus employed in ascertaining the density of steam. The results obtained in these later experiments, however, confirm the general law deduced from the previous ones.

The figure represents the apparatus used when the pressure did not exceed that of the atmosphere, consisting of a glass globe (a) 3 inches in diameter, and stem 35 inches long; the capacity was known to a point (b), where a piece of platinum wire was twisted to mark accurately the point at which the mercury column in the stem was to be brought to maintain a constant volume in the globe.

A $1\frac{1}{4}$ -inch tube (d), filled with mercury, rested upon the frame ($e e$). The weight of the tube and frame was counterbalanced by weights ($w w$). By such an adjustment the tube (d) could be regulated with facility, preserving the upper level of the mercury column

at one uniform height. A cathetometer (*g g*), with vernier (*h*), to read the lower and variable level of the mercury column, was intro



duced. To heat the globe, the oil-bath (*k*) was used, fitted to the tube (*t*) by a stuffing-box (*s*); the oil-bath is itself immersed in a mercury bath (*l*), surrounded by a coil of jets of gas (*m m*).

The globe, filled with dry and warm mercury, the air-bubbles being extracted by means of an air-pump, was inverted to form a Torricellian vacuum. A small glass globule of water was then inserted, the platinum wire fixed in its place, and an india-rubber cap fitted to the extremity of the stem. Being transferred to its place, and the india-rubber cap replaced by an open glass cistern, so that the glass (*d*) could be elevated to its position, the jets were lighted, and the temperature elevated to 300°.

From this point the levels of the column were read off at intervals of 50° until the temperature of saturation was reached. The levels were taken in a series of descending temperatures, to avoid the influence of steam boiling out of the mercury as the temperature rose,

and to eliminate the effect of the cohesion of the glass on the water, as explained in our previous paper on the density of steam.

Twelve cubic inches of mercury were measured into the globe, and a file-mark made on the stem, below which, at a distance of 14.45 inches, another file-mark was made, affording a fixed point for ascertaining the correspondence of the upper file-mark with the readings on the cathetometer.

Let a be the reading on the fixed rod of the level of the column, b the reading of the lower file-mark on the globe-stem; then $b - a$ = the height of the column of mercury on the globe-stem.

To correct for temperature, $7\frac{1}{2}$ inches of mercury, enclosed by the oil-bath and its stuffing-box, were corrected for the temperature of the oil, and the remainder of the column for the temperature of the atmosphere at the time. By deducting the column so corrected from the reading of the barometer at the time, the total pressure in the globe is obtained. The readings of the thermometer are corrected for the portion out of the oil-bath. The pressure of mercurial vapour is calculated from data supplied with great courtesy by M. Regnault, and embodying the results of unpublished experiments. The pressure of this vapour is assumed to be the same as that in a vacuum, as the vapour in the globe remains still for a sufficient time (it is believed) for saturation to take place. In this view we have been strengthened by M. Regnault's opinion. By deducting the pressure of mercury vapour from the total pressure in the globe, the pressure of the steam is obtained.

On referring to the experiments contained in the paper, it will be seen that the law of expansion of gaseous bodies is expressed by the formula

$$\frac{E+t}{E+t_1} = \frac{PV}{PV_1}; \quad \therefore E = \frac{PVt_1 - P_1V_1t}{P_1V_1 - PV},$$

where E is a constant. Taking Regnault's constant 459 as the rate of expansion of air for constant volumes, a remarkable coincidence will be observed in the experiments contained in the paper when reduced to the same standard of value. The values of E thus deduced have been placed in the last column of the calculated experiments. They show a decreasing rate of expansion from the saturation-point upwards, until at no great increase of temperature the rate of expansion coincides with that of a perfect gas.

Taking from the Tables the two results, which in each instance represent the case of expansion at the greatest distance from the saturation-point, we have the following values of E :—

$E =$ (1)	474.48	(3)	466.85	(5)	460.28
	450.11		451.94		
(2)	455.57	(4)	464.83		
	443.86		460.49		

Mean value of E deduced from these numbers = 458.74.

Hence the conclusion which we suggested in our previous paper

has been satisfactorily demonstrated by a more extended series of experiments, and the rate of expansion of superheated steam is shown to be almost identical with that of air and other permanent gases, if calculated at temperatures not too close to the maximum temperature of saturation.

“On a New Method of Approximation applicable to Elliptic and Ultra-elliptic Functions.” By Charles W. Merrifield, Esq., F.R.S.

April 10.—Major-General Sabine, President, in the Chair.

“On the Total Solar Eclipse of July 18, 1860, observed at Rivabellosa, near Miranda de Ebro, in Spain.”—The Bakerian Lecture. By Warren De la Rue, Esq., F.R.S.

The Lecturer gave an account of the more interesting phenomena of the eclipse, and of the methods employed in observing and recording them; the details of his observations being given in an elaborate Paper bearing the above title. The Lecture was illustrated by a great number of diagrams and models. The photographic images of the eclipse were projected on a screen by means of the electric lamp, and some of the more striking phenomena were imitated by apparatus contrived for that purpose.

The following is an abstract of the Paper:—

The author, for some time previous to the organization of the Astronomer Royal's expedition to Spain, had contemplated making an attempt to photograph the phenomena of the total eclipse of July 18th, 1860, but as soon as he was informed of the Astronomer Royal's views he agreed to join his party, now known as the Himalaya Expedition, from the name of Her Majesty's ship which conveyed the astronomers composing it to Spain. He attributes much of the success of his operations to the admirable arrangements of Professor Airy in England, and to those concerted with Mr. Vignoles in Spain; for he was able in consequence greatly to increase the extent of his preparations, and to convey a complete temporary observatory fitted up with all the numerous requirements which are essential in astronomical photography. Besides himself, his party consisted of Mr. Beckley of the Kew Observatory, Mr. Reynolds (now Mr. De la Rue's private assistant), Mr. Downes, and Mr. E. Beck, and subsequently the late Mr. S. Clark. The author expresses himself greatly indebted to these gentlemen for their most efficient assistance.

The party took up their station at a village called Rivabellosa, situated near the town of Miranda de Ebro; the site selected was a thrashing-floor, on which the observatory was erected.

The instruments employed consisted of the Kew heliograph, for the photographic records; an achromatic telescope, by Dallmeyer, mounted on a sort of alt-azimuth stand contrived by the Astronomer Royal, which permitted of an equatorial movement by the ingeniously arranged joint action of two racked radius bars. To this telescope the author fitted a diagonal eyepiece of his own contrivance, which allowed of the use of reflexion from plain glass in the first

instance, and then from a portion silvered on the top surface the instant the period of totality commenced. By its means he avoided the perplexity and loss of time occasioned in unscrewing and screwing portions of the apparatus at the most critical period. To these were added a small transit theodolite, three chronometers, two barometers, and several thermometers.

The weather proved so unpropitious that it was with much difficulty the objects of the party could be carried out; and it was only by using every available opportunity that even the Kew instrument could be placed in position.

The geographical position of the site of the observatory was ascertained to be—north latitude $42^{\circ} 42'$, west longitude 11 min. $42\cdot7$ sec., elevation above the mean high-water mark 1572·4 feet.

The author made two sketches of the luminous prominences during the period of totality, on paper previously ruled to represent the position-lines drawn on a piece of parallel glass placed in the focus of the eyepiece, which magnified about 60 times. These position-lines consisted of a square calculated to exactly include the lunar disk, and two external squares, one exactly one minute of arc distant from the central square and from the other. The angles of the squares were joined by diagonal fainter lines. The whole system was moveable through an arc of 90° , and its position could be read off on a graduated external circle divided from 10 to 10 degrees. The drawings were by chance made of nearly the exact diameter of the lunar disk in the photographs (4 inches), and proved very valuable in interpreting the phenomena revealed by the latter, as the one could be compared by superposition with the other, and the several prominences be thus identified.

One of the prominences, situated about 30° from the north point towards the east, became visible several minutes before totality, even during the employment of the unsilvered portion of the diagonal reflector. As the sun disappeared the author watched for the so-called Baily's beads, but no such phenomenon occurred, which occasioned no surprise to him, as he had always believed that it arose in all probability from the atmospheric disturbance of an image formed by a telescope wanting in definition.

The author goes on to describe the various appearances presented by the several protuberances, which were not all of a rose-colour, and those which presented this hue were much paler in colour than his previous reading had led him to expect. He is able to speak with considerable certainty on this point, having before the eclipse painted several colours on his drawing-paper, and was thus enabled to compare these directly with the prominences by means of the light emitted by the corona, it being sufficiently great and polychromatic for that purpose. The light of a lamp which was at hand proved not only useless, but was detrimental in making the comparisons. There was a considerable amount of detail, both of form and colour, in the prominences, which the author has shown in two coloured drawings which accompany the paper; these are founded on

the original sketches, which are also given in fac-simile, but to some extent corrected by means of the photographs.

That the prominences belong to the sun and not to the moon was rendered evident to the observer by the progressive covering of the luminous prominences on the east in the direction of the moon's motion, and the gradual uncovering of fresh prominences on the west; while prominences situated in a position nearly at right angles to the moon's path shifted their angular position on the moon's edge several degrees during the observations. The prominence which became visible before totality, which the author designates by A, was found to have shifted $3^{\circ} 25'$ on the moon's limb in an interval of about $2\frac{1}{2}$ minutes; it was therefore evident that the region of the moon which at the commencement of the period was in apparent contact with the prominence was at some distance from it at the end; and as the prominence underwent no change during that time, the theory falls to the ground which ascribes the phenomenon of the luminous protuberances to some peculiar action of the moon's edge on light coming originally from the sun.

The author describes the general effect of the eclipse to the unassisted eye. He was particularly struck with the peculiar illumination of the surrounding landscape as the sun became reduced to a small crescent; the shadows of all objects were so sharp and the light so brilliant that it reminded him of the illumination produced by the electric light; at the same time peculiar hues were assumed by the sky and landscape, which suggested the idea that the light of the sun near the periphery is not only less intense than that of the centre, but that it may be different in quality.

No attempt was made to obtain accurate observations of the corona, but nevertheless a few seconds were devoted to this phenomenon. Even several minutes before totality the whole contour of the moon could be distinctly seen; when totality had commenced, the moon's disk appeared of a deep brown in the centre of the corona, which was extremely bright near the moon's limb and appeared of a silvery white, softening off with a very irregular outline and sending forth some long streams. It extended generally to about from 0.7 to 0.8 of the moon's diameter beyond her periphery.

The darkness during the totality was not nearly so great as might have been expected from accounts of previous total eclipses. The illumination was markedly distinct from that which occurs in nature on any other occasion, and certainly was greater than on the brightest moonlit night, although at the time the light appeared to the author as less bright than what he remembered of bright moonlight. By subsequent trials he was led to conclude that the light during a total eclipse most resembles that degree of illumination which exists in a clear sky soon after sunset, when, after having made out a first-magnitude star, other stars of less brilliancy can be discerned one after another by an attentive gazer. Jupiter and Venus were the only objects the author had time to identify, but some neighbouring observers saw also Castor.

The most important part of the paper treats of the photographic

observations. The several preparations are minutely described, and drawings, showing the general arrangements of the observatory, are given. In the focus of the secondary magnifier of the Kew heliograph, two position-wires, crossing at right angles, are fixed at approximately an angle of 45° to a parallel of declination. The object-glass has an aperture of 3.4 inches and a focal length of 50 inches: the primary focal image of the sun at his mean distance is 0.47 inch; but before it is allowed to fall on the sensitive plate, it is enlarged to about 3.8 inches by means of an ordinary Huyghenian eyepiece. The object-glass is so constructed as to ensure the coincidence of the chemical and visual foci; this coincidence is, however, disturbed in a slight degree by the Huyghenian magnifier, which renders a slight adjustment necessary. For ordinary sun-pictures, and those of the several phases of the eclipse except the totality, the aperture was reduced to 2 inches,—a peculiar instantaneous apparatus being employed to regulate the exposure of the sensitive plate.

The driving-clock of the heliograph was, for convenience, kept going during the taking of the partial phases of the eclipse; but it was not really necessary to keep it in motion, because the time of exposure certainly did not exceed the $\frac{1}{50}$ th of a second.

The position-wires, by stopping off the sun's light, are depicted in the negatives as white lines crossing the solar disk. It was essential, in order to turn these several pictures to account, to note exactly the time of their being taken, which was done by Mr. Beckley; the clicking noise made by the instantaneous apparatus, when it struck against a stop after releasement, indicating the epoch, which was noted to the nearest half-second. The exact position of the cross wires was also ascertained by observations of the sun made on each side of the meridian; this was necessary, because, in consequence of the weather, the pole of the heliograph could be only approximately adjusted in position.

Upwards of fifty plates were placed in the heliograph between $11^{\text{h}} 28^{\text{m}}$ A.M. and $4^{\text{h}} 16^{\text{m}}$ P.M. on July 18th; some before the commencement of the eclipse, and some after. During totality two photographs were obtained. One picture was produced on a plate which was exposed from the exact commencement of totality during the minute succeeding this epoch; the second picture was exposed from about a minute previous to the reappearance of the sun until not more than a second before he became visible. In these pictures the several prominences are depicted with great clearness; and when one negative is superposed on the other, corresponding parts exactly coincide. During the taking of the second photograph, an excusable curiosity on the part of two of the assistants disturbed the telescope twice, so that the prominences have depicted themselves three times; but there was no difficulty in stopping out the images not belonging to either of the three phases thus recorded. The author has moreover turned this accident to account, and estimated the relative brightness of the prominences in comparison with the sun's photosphere; and he considers that they are at least 600 times less brilliant than it. This conclusion has been drawn from the minimum

time required by the prominences to depict themselves, which can be made out from the photograph in question.

By means of a new micrometer contrived for that purpose by the author, the several photographs have been measured and discussed. The position-angles of the line joining the sun's centre and the moon's centre, and the distances of these centres for the several epochs of the photographs, have been calculated and compared with the corresponding values calculated by Mr. Farley for the geographical position of the observatory. Other calculations have also been made from the photographs and compared with certain elements of the eclipse calculated by Mr. Carrington. The results show that the photographic method of observing solar phenomena is capable of great exactness.

The nearest approach of the centres of the sun and moon, as ascertained from the photographic measurements, was $11''\cdot8$, calculation giving as a mean $12''\cdot8$. The relative diameter of the moon, that of the sun being taken as unity, as derived from measurements of the photographs, comes out $1\cdot0511$, which is precisely the theoretical number; on the other hand, they tend to show that the diameters at present assumed for the sun and moon, taken conjointly, are about $4''\cdot0$ in excess of the truth.

The paper is accompanied by an extensive series of calculations, which it is not here necessary to describe. Those, however, relating to the measurements of the positions of the luminous prominences on the two totality-pictures have especial interest. These measurements were made in two ways: 1st, the original negatives were measured by the author's new micrometer; 2nd, enlarged positive copies were taken on glass, and the contours of the prominences traced and etched upon the glass, which was afterwards centered on a dividing engine and divided, the divisions being subsequently etched. Copper duplicates were then made of the glass plates, which served to print off diagrams which accompany the paper.

Without describing minutely the measurements, it will suffice here to state that the results go to prove that the luminous prominences must belong to the sun and not to the moon. For example, the change in the angular position of the prominence at a right angle to the moon's path, and designated A in the paper, has been calculated to have been $5^{\circ} 21'$ for the assumed geographical position of the station; by measurement of the two photographs it is $5^{\circ} 32'$. The motion of the moon in covering and uncovering a prominence nearly in the line of her path was calculated to have been $92''\cdot1$; by measurement it was found to have been $93''\cdot0$. The accordance of these numbers is so extremely close, that it would be difficult to obtain more convincing proofs that the luminous prominences belong to the sun.

XI. *Intelligence and Miscellaneous Articles.*USE OF THE WEIGHTS AND MEASURES OF THE METRIC SYSTEM
IN SCIENTIFIC PURSUITS.

THE Report of the Committee of the House of Commons on Weights and Measures contains decisive evidence on the employment of the Metric System in chemistry, natural philosophy, and general science. The examination of Mr. Graham, the Master of the Mint, is as follows :—

664. Has your attention been directed to the progress of the decimal and metric system in this country?—It has lately, with reference to the use of the system in scientific papers.

665. Is not the system attended with great advantages?—Yes; and I may say it has superseded in a great measure the ordinary system within the last ten years.

666. Do our chemists continue to make use of the gramme?—Yes, I may say almost exclusively in scientific papers.

667. Have they done that voluntarily?—Yes.

668. They have, therefore, done so from a conviction of the advantages of the system?—Yes.

669. Do you find it also an advantage of the metric system, that its divisions form a sort of common language, better understood by scientific men abroad than the old system?—Yes, that is a very great recommendation.

670. Has it ever come to your knowledge that the French, in consequence of the variation between the English and French systems, neglect the scientific papers which are published in England, in which our own system of national weights and measures is used?—That is certainly the case, and they are frequently not translated, I believe, on that account.

671. But still the metric system is finding its way in every scientific calculation?—Yes.

672. Is it the general opinion of scientific men that this is a very desirable change to be carried into effect?—As far as my information goes, it is a very general opinion.

673. Would you go so far as to say that no scientific man thinks that ultimately any other system is possible?—I believe that scientific men who have considered the subject, and who are in the habit of using the metric system, think so.

674. Do you observe that the system makes its advance into popular scientific papers which are published?—It is beginning to find a place now in scientific elementary works.

675. In the scientific papers which you sometimes write yourself, do you use it from a conviction of its convenience?—Yes.

676. Is it used in papers read before the Royal Society?—It is.

677. Is it a fact that some years ago its use was objected to by

the Royal Society?—I cannot say that it was formally objected to, but it was little used ten or twelve years ago.

678. Now, on the other hand, is it recommended by the Royal Society to be used in papers read before them?—Not formally; but I believe the English system would not form a recommendation to a chemical paper.

681. Have you travelled in France?—Yes.

682. Have you ever had occasion to observe whether English people have great difficulty in acquiring the French metric system in France?—I think not; in fact I was rather struck with the facility with which English ladies made use of it in keeping their accounts.

683-4. Do you confirm the opinion of many other witnesses before this Committee, that the adoption of the metric system would be a great international advantage, as well as an advantage for scientific men?—Yes.

685. From your position, and the amount of your scientific pursuits, have you had opportunities of observing its progress, and knowing its value?—Certainly.

686. Generally, if you were introducing a new system, what unit of weight would you recommend?—The gramme.

687. You would not recommend any of the old systems of weights?—No.

688. If you made any change at all, it would be desirable, would it not, to make such a change as would be a uniform change all over the world?—Yes.

The following questions and answers occur in the evidence of Professor Miller of Cambridge:—

1489. Do you find in your learned pursuits that our present system of weights and measures interferes with scientific investigation in any way?—Not in the least; they are so complicated that it is quite impossible to use them. The balance-makers provide balances made for accurate purposes with decimal weights of some kind. Mr. Robinson used to provide balances with weights of a grain and its decimal subdivisions and multiples. M. Cœrtling, one of our best balance-makers at present, also supplies his balances with weights on the metric system, with their decimal subdivisions and multiples.

1490. How long has it been the case that the decimal metric system has been introduced in scientific operations?—As long as I can remember: I should think that since 1830 no chemist ever made use of any weights which were not decimally divided.

1493. So far as scientific investigations are concerned, our present system is useless?—Entirely. I believe I do not know the value of any of the ordinary subdivisions, the scruple, and the drachm for instance.

1495. Do you think the metric system is extending in this coun-

try?—All chemists use it by preference. If you refer to any of our scientific journals, I think you will observe that weights are almost invariably given in grammes, and measures in millimetres.

Since the publication of the above-named Parliamentary Report, a deputation has been received by the Right Hon. Milner Gibson, President of the Board of Trade, to enforce the propriety of the general adoption of the metric system. At this interview, Professor Owen of the British Museum showed its importance in the study of natural history. In this science, as he observed, the majority of the facts include the elements of weight and measure. The evils and inconveniences of the present state of things were illustrated by such instances as the following:—

An English anatomist and physiologist gives the weight of the brain, lungs, &c. in relation to the weight of the body, of some rare animal. The foreign physiologist desires to reduce the English weights to those of his own country. If the kind of weight used by the Englishman be not specified, viz. avoirdupois or troy, the description is useless to the foreigner in regard to the important constants of the proportion of parts or organs to the whole body.

Whilst British observers vary even in the use of weights legalized in their own country, our present systems of measures of capacity make it still more difficult to impart to foreign *savans* the results of our observations.

As to linear admeasurements, a foreigner has generally some misgiving whether the British naturalists, in describing lengths by “lines,” mean the twelfth or the tenth part of the English inch. The most careful describers feel themselves, therefore, compelled, for the sake of their continental fellow-labourers, to make use of two systems of notation; and I have for some time used the French decimal system, appending the dimensions, in parts of the metre, to the denominations of the English system. A foot-rule, with the divisions of the metre on one side, is now indispensable for associated labours of observation in regard to constants of linear admeasurement.

Although, when the system of weight or measure is noted by the observer, its reduction, or the finding of the equivalent in another system, is a small demand upon time, yet the repetition of that act takes a serious amount from the working-hours of the individual; and when multiplied by the number of observers obstructed by conflicting systems of weights and measures, the impediment to the progress of the sciences of observation become so great as to render the subject quite worthy of the consideration of legislative authority.

**EXPERIMENTAL DETERMINATION OF THE VELOCITY OF LIGHT:
DESCRIPTION OF THE APPARATUS. BY M. LÉON FOUCAULT.**

Spite of restricted space and of the want of figures, I shall try to describe the principal parts of the apparatus which has yielded a

value for the velocity of light so different from that which has hitherto been accepted in science.

It consists of a micrometric sight cut in a plate of silvered glass : a rotating mirror supported on the axis of a small air turbine ; a bellows with constant pressure ; an achromatic objective ; a series of an odd number of concave spherical mirrors of silvered glass ; a mirror inclined at partial reflexion ; a micrometric microscope ; and a circular screen in the form of a toothed wheel set in motion by a chronometric wheelwork. I shall first describe the apparatus at rest.

A beam of solar light, reflected horizontally by a heliostat, falls on the micrometric sight, which consists of a series of vertical lines at a distance of $\frac{1}{10}$ millim. from each other. This sight, which in the experiment is the real standard of measure, has been divided with great care by M. Froment. The rays which have traversed this plane, impinge at a distance of a metre on a plane rotating mirror, where they undergo a first reflexion, which sends them towards a first concave mirror at a distance of 4 metres. Between these two mirrors, and as near the plane mirror as possible, the object-glass is placed, having on the one side the virtual image of the sight, and on the other the concave mirror with its two conjugate foci. These conditions being fulfilled, the beam of light, after having traversed the object-glass, forms an image of the sight on the surface of this first concave mirror.

Hence the beam is reflected a second time in a direction so oblique that it avoids the apparatus of the rotating mirror, the image of which it forms in space at a certain distance. At the place at which this image is produced a second concave mirror is placed, arranged so that the pencil, once more reflected, passes near the first spherical mirror, forming a second image of the sight ; this is taken up by a third concave surface, and so on, until the last image of the sight is formed at the surface of a last concave mirror of an odd number. I have been able thus to employ as many as five mirrors, which develop a line 20 metres in length. The last of these mirrors is at a distance of 4 metres from the last but one, which faces it, a distance equal to its radius of curvature ; and it reflects the image on its own path, a condition which is exactly fulfilled by superposing on the surface of the opposite mirror the emergent image with the return image ; that being effected, it is certain that the pencil reascends the series, repasses by the plane mirror of the rotatory apparatus, and that, finally, all the rays re-emerge by the sight, point by point, as they entered. This fact of the return of the rays may be confirmed, and an accessible image procured by turning, by partial reflexion, on the surface of a mirror inclined at 45° , a portion of the pencil which is examined with a feeble microscope. This latter, which in all respects resembles the micrometric microscopes in use for astronomical observation, forms with the sight and the inclined mirror a very stable solid whole.

In the apparatus thus described, the real image sent to the microscope and formed by return rays partially reflected, occupies a

definite position in reference to the mirror and to the sight itself. This position is precisely that of the virtual image of the sight, seen by reflexion in the plane of the mirror. This, at any rate, is what occurs when the apparatus is at rest.

But when the plane mirror rotates, this image changes its place, seeing that during the time in which the light takes to traverse twice the broken line of the concave mirrors, the rotating mirror continues to rotate, and that the rays, on their return, do not meet it under the same incidence as at the moment of arrival. It follows that the image is displaced in the direction of the rotation, and this displacement increases with the velocity of the rotation; it also manifestly increases with the length of the path, and with the distance which separates the sight from the rotating mirror. The manner in which these various quantities affect the experiment as well as the velocity of light itself, are expressed by a very simple formula, which has been already established, and which I shall only have to recall here.

Calling V the velocity of the light, n the number of turns of the mirror in a second, l the length of the broken line comprised between the rotating mirror and the last concave mirror, r the distance of the sight from the rotating mirror, and d the observed displacement, we get, by considering the structure of the apparatus,

$$V = \frac{8\pi nlr}{d},$$

an expression which gives the velocity of light by means of quantities which must be separately measured.

The distances l and r are measured directly by a rule, or by a paper band, which is then referred to the unit of length. The displacement may be observed micrometrically; it remains to be shown how the number of turns (n) is estimated which the mirror makes in a second.

Let us first describe how a constant velocity is imparted to the mirror.

This mirror of silvered glass, 14 millims. in diameter, is mounted directly on the axis of a small air turbine of known construction, admirably executed by M. Froment. The air is furnished by a high-pressure bellows made by M. Cavaille-Coll, who has justly acquired a high reputation in the manufacture of large organs. As it is important that the pressure be very constant, the air on emerging from the bellows traverses a regulator, recently devised by M. Cavaille, and in which the pressure of a column of water of 30 centimetres does not vary $\frac{1}{2}$ a millimetre. Hence the fluid emerging from the orifices of the turbine represents a remarkably constant motive power. On the other hand, the mirror, in accelerating, meets with a resistance in the surrounding air, which for a given velocity is also perfectly constant. Hence the moveable body placed between these two constant forces cannot fail to assume and retain a uniform velocity. Any stop acting on the flow of air renders it possible to regulate this velocity within very narrow limits.

It remained, finally, to count the number of turns, or rather to impart to the moveable body a determinate velocity. This problem was completely solved in the following manner:—

Between the microscope and the mirror for partial reflexion is placed a circular disc, whose finely-toothed edge encroaches upon the image observed, and partially cuts it off. The disc rotates uniformly on itself so that, if the image shone in a continuous manner, the teeth which it carries on its circumference would escape from sight by the rapidity of its motion. But the image is not permanent, it arises from a series of discontinuous appearances, which are equal in number to that of the revolutions of the mirror; and in the particular case in which the teeth of the screen also succeed in the same number, an easily explicable optical illusion is produced, which makes the teething appear as if the disc had no existence. Let us suppose, then, that this disc, carrying n teeth on its circumference, makes one turn in a second, and at the same time the turbine is started; if, in regulating the flow of the motive air, the apparent fixity of the teeth can be maintained, it is certain that the mirror makes exactly n turns in a second.

M. Froment, who had made the turbine, has been good enough to devise and construct a chronometric wheelwork to make the disc move; it is a very remarkable piece of clockwork, which solves in an elegant manner the problem of uniform motion in the particular case in which there is no work to perform. The success is so complete, that I am daily in the habit of starting the mirror at 400 turns in a second, and of seeing the two apparatus work to almost $\frac{1}{10000}$ for entire minutes.

Yet, although I had obtained all certainty with regard to the measurement of the time, I was surprised at finding in my results discordances which were not in any ratio with the precision of the means of measurement. After long research, I ultimately found that the source of error lay in the micrometer, which is by no means so accurate as is usually thought. To obviate this difficulty, I introduced into the system of observation a modification which ultimately comes to a simple change in the variable. Instead of measuring the deviation micrometrically, I adopt for it a value defined beforehand, either $\frac{7}{10}$ of a millimetre, or 7 entire parts of the image, and I try, experimentally, what distance there must be between the sight and the rotating mirror to produce this deviation: as the measures refer then to a length of about a metre, the last fractions still retain a directly visible magnitude and leave no room for error.

By this means the apparatus has been freed from the principal source of uncertainty. Since then, the results agree within the limits of the errors of observation, and the means are so fixed that I have been able to give confidently the new number which it appears to me expresses very nearly the velocity of light in space; that is to say, 298,000 kilometres (190249.16 miles) in a second of mean time.
—*Comptes Rendus*, Nov. 24, 1862.

PHENOMENA OF TRANSPORT THROUGH POROUS BODIES: APPLICATION TO DIRECT ANALYSIS: DIALYSIS. BY M. ERN. GUINET.

After a very brief analysis of Mr. Graham's researches, the author proceeds as follows:—

Having experienced certain difficulties in the use of vegetable parchment, I endeavoured to replace the dialyser by a porous vessel of pipeclay, such as are used for batteries, instead of which it would be better to use shallow vessels. I have repeated most of Mr. Graham's principal experiments, and have made some which appeared impossible with vegetable parchment.

The following are some of them:—

Solution of gum and sugar, in which is immersed a porous vessel containing pure water. At the expiration of twenty-four hours a great part of the sugar has traversed the porous vessel, and is dissolved in the water, which does not contain a trace of gum.

Solution of caramel and of bichromate of potass. The salt traverses the porous vessel alone; the separation is speedily effected. If a drop of the mixed solutions is allowed to fall on a porous vessel, a brown spot of caramel is obtained, surrounded by a yellow one of bichromate, which shows the more rapid diffusion of the latter salt.

Cotton dissolved in ammoniacal solution of copper. The water in the porous vessel becomes blue from dissolving ammoniacal oxide of copper; the cotton remains on the outside. The evident object of this experiment is to get the cotton in a soluble modification; but as ammoniacal oxide of copper diffuses slowly, I must wait a month to obtain a result. It is clear that this experiment could not be made with vegetable parchment, which is acted on by the ammoniacal copper solvent.

Experiments were made in which water was replaced by other liquids, such as bisulphide of carbon and oil of turpentine.

The diffusibility of different crystalloids in bisulphide of carbon is by no means the same in all cases. Thus, if iodine, sulphur, and naphthaline are dissolved in bisulphide, the two latter pass into a porous vessel full of pure bisulphide much sooner than the former.

If it might be permitted to venture an explanation of the unexpected phenomena discovered by Mr. Graham, it might be said that parchment-paper or porous vessels act as a kind of sieve, through which the more attenuated molecules pass more readily; for the colloids have generally a high equivalent and a considerable atomic volume. The opposite is the case with crystalloids; and the less diffusible of the crystalloids are those which correspond to the greatest atomic volume (taking for this the quotient of the atomic weight by the density, which cannot be exact). Such is iodine, which is less diffusible than sulphur.—*Comptes Rendus*, Nov. 10, 1862.

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XII. *On the Formation of Alpine Valleys and Alpine Lakes.*
By JOHN BALL, M.R.I.A., F.L.S. &c.*

EVERY one who feels an interest in the past history of the Alps must be glad to find renewed attention given to the natural agencies that have given that region its existing conformation. Many persons will therefore have read with satisfaction the papers recently published by Professor Ramsay, the President of the Geological Society, in the Quarterly Journal of that Society for August 1862, and by Professor Tyndall in the Philosophical Magazine for the following month. To these should, perhaps, be added some important observations contained in an address delivered at the recent Meeting of the British Association, by Mr. Beete Jukes, the President of the Geological Section. In these publications by eminent English men of science, the views of preceding alpine geologists, such as Charles Martins, Gastaldi, and Omboni, which have been ably summed up and extended in some recent memoirs by M. Mortillet, have received a still wider extension, and we are called upon to enlarge very much our previous conceptions as to the agency of those great glaciers which, at a period geologically very recent, descended from the flanks of the higher Alps to the level of the plains.

If controversies in science were decided by the authority of eminent names, or if the discussion of the problems raised by these papers required a complete acquaintance with the whole field of physics and geology, I should certainly not enter the lists against such formidable opponents. The problems in question, however, occupy a limited field in the region which is

* Communicated by the Author.

common alike to physics and to geology ; their solution depends in a great part upon facts which must be studied on the ground ; and I am thus led to hope that a somewhat long and extensive acquaintance with the Alps, during which the questions at issue have been very frequently the subject of my thoughts, may authorize me without presumption to take a share in the discussion. Writing at a distance from England, and with but slight opportunities for knowing what is passing elsewhere, I shall be forgiven if I repeat objections previously urged by others, or advance arguments that have been already satisfactorily answered.

Professor Ramsay attributes to the action of glacier-ice the hollowing out of lake-basins in the Alps and elsewhere ; Professor Tyndall sees in the same powerful agent the main, if not the exclusive, means for the formation of alpine valleys. An anonymous writer, in terms which show that his knowledge of the subject is on a par with his good taste, has confounded together the scope of the two papers. I hope to show not only that the problems attempted to be solved by their writers are different, but, furthermore, that the main objections to each solution rest upon considerations entirely distinct.

Taking, in the first instance, the larger of the two questions raised for discussion, I shall inquire whether there is reason to admit Professor Tyndall's conclusion, drawn chiefly from his observations in the neighbourhood of Monte Rosa, that the valleys of the Alps have, as a general rule, been scooped out by great glaciers from the flanks of previously continuous mountain masses.

The first thought of any one considering this question is to endeavour to take a comprehensive view of the present configuration of the surface. If the reader will look at any general map of the Swiss and Savoy Alps, he will in the first place observe that between the higher central ranges and the plains of France there is interposed a zone of secondary rocks, elevated into ridges from 4000 to 6000 feet above the sea-level. In carrying his eye along this zone from the neighbourhood of Grenoble to that of Aarau, he will see that the direction of the ridges, which is at first nearly north and south, is gradually bent towards the east, so that the principal chain of the Jura points from N.E. by E. to S.W. by W. He will further observe that the chain nowhere consists of a single ridge, but of three, four, or five parallel ridges with furrow-like valleys lying between them, here and there cut through by some stream that appears to flow through a fracture that has traversed the entire range. Including along with the Jura the ranges of Western Dauphiné, the Vosges Mountains, and the somewhat higher ridges west of the valley of the Arty, it will be apparent that the arguments of Professor

Tyndall cannot possibly apply to a system of valleys which are, without exception, parallel to the highest ridges, and where, if the present inequalities were filled up, and the whole mass covered with ice, the new glaciers, supposing them competent to form valleys, would certainly shape them at right angles to their present direction.

If he neglect these outlying ranges for a moment, and direct his attention to the central region of the Alps, the observer will scarcely fail to note, as one of the most characteristic features of the Swiss Alps, the line of valley which extends for nearly 140 miles from Martigny to Coire. With one slight distortion, the Rhone flows directly from E.N.E. to W.S.W. between the Furka Pass and Martigny. On the opposite side of that low pass, the line of valley descending to Andermatt preserves exactly the same direction. The famous gorge of the Devil's Bridge allows the Reuss to carry off towards the north the drainage of the valley, but the main line of depression keeps true to its original direction through the glen that mounts to the Oberalp Pass; and east of that ridge the same direction is so accurately preserved, that a line drawn from Chiamet (the highest hamlet) to Coire is nowhere half a mile from the present bed of the Vorder Rhine. Another of the main valleys of the Alps, that of the Inn, is nearly exactly parallel to the standard line which we have traced across Switzerland. From Kufstein, where the railway enters the Tyrol, the valley of the Inn, with a slight distortion between Innsbruck and Ried, maintains a constant direction up to its head at the lake of Sils; but a traveller following steadily the same course finds the pass of the Maloja west of the lake very little raised above the level of the valleys on either side, and leading through the Val Bregaglia to Chiavenna, from whence, if he will keep on W.S.W. across the ridge that separates him from Roveredo, he will enter another line of valley that is continuous with the upper end of the Lago Maggiore, and may even be traced through the Val Vegezzo and the Val Anzasca to the foot of Monte Rosa. It is sufficient to carry the eye across the map, to perceive how very generally the same direction prevails among the valleys of the central region of the Alps, and that, as in the instances above quoted, these lines of depression traverse a ridge, or contain streams that flow in opposite directions—showing that by no conceivable change in the general conformation of the land could a single stream or glacier have done the work. The line of valley, in great part occupied by lakes, that stretches from Interlaken to Kussnach on the Lake of Lucerne, the system of valleys between the Lake of Thun and the Rhone, the Val Pellina, the Lex Blanche or Allée Blanche, and the Valley of Chamouni, are so many instances in point,

and, when taken together, bring to my mind the conviction that some considerable portion at least of the existing valleys in the Alps owe their origin to forces which have operated on a great scale, and which can scarcely be any other than those that have raised the mountain ridges to which the same valleys are related. If it be urged that several of the valleys to which I have referred lie along the line of outcrop of softer and more easily disintegrated rocks, such as certain slates in the valley of the Rhone, and that the action of either water or ice would for that reason be more effective in scooping out the valley where we now see it, I may reply that the very fact alleged shows the working of denudation along the same line at some early period in the history of the elevation of the Alps, and the strong probability of the existence at the same period of a corresponding valley.

If we now pass from the contemplation of a wide tract of mountain country to the examination of particular groups or *massifs*, we very frequently find indications of the prevalence of a common direction in the secondary ridges and valleys, transverse, but seldom exactly perpendicular, to that of the main valleys. Thus in the Bernese and Lepontine Alps we find the ridges enclosing the two main branches of the Aar Glacier, those on either side of the Geren Thal, the Val Leventino between Airolo and Faido, and several minor valleys, all showing a degree of parallelism which points to the operation of some common cause. The series of seven minor valleys lying about due east and west between the Val Maggia and that of the Tosa is perhaps a better illustration, as there is nothing in the general configuration of the district to make it conceivable that, if the hollows were filled up, water or ice would reopen trenches where the present valleys exist. The four or five great ridges extending northwards from the range of Monte Rosa towards the valley of the Rhone furnish, as I believe, another illustration of the same tendency to the formation of groups of parallel valleys.

The facts hitherto adduced seem to me to point very strongly to the conclusion that mechanical forces, acting on a large scale and in definite directions, have had at the least a considerable share in the formation of alpine valleys. For reasons to which I shall further advert, it appears most probable that complicated forces acting on the mountain masses have given rise to many valleys whose direction gives no clue, or none that has yet been traced, to their origin; but I am quite ready to admit that the present condition of the surface cannot readily be explained without very largely admitting the action of water, whether in the liquid or solid form. To study the various agencies that, sometimes working together, sometimes in alternation, have given to

the alpine world its present form and aspect, to attribute to each its own share, and to find fair evidence in support of his conclusions, is the formidably difficult task of the alpine geologist; and even if he be fortunate enough to gain the help of the ablest workers in the field of physical science, it will be long before that task can approach to a conclusion. In considering whether or no it is probable that the agent which has excavated valleys whose vertical depth below the ridges that enclose them often exceeds an English mile, and in two of the valleys referred to by Professor Tyndall is at least a mile and a half, the first step is to examine the mode of action of existing glaciers.

The whole mechanical effect of a glacier upon its bed is directed towards the removal of inequalities, whether in the bottom or sides. It is quite understating the case, to say that if valleys were excavated by ice through strata at all approaching to uniform hardness, they would all tend to the same type of equal slope in the bed, and absence of projecting masses in the containing walls, of which the valley of the Rhone between Martigny and Sion gives the best known example. Any one who has watched the manner in which the bottom of a glacier slides over the concavities without touching them, and applies all its immense grinding power to the convex portions of its bed, will admit that a great degree of inequality in the resistance of different portions of the rock would not prevent a valley scooped out exclusively by glacier-action from approaching nearly to the same uniform model. Fortunately for the lover of natural scenery, the fact is widely different from what it would be if the theory were generally well founded. The most common type of an alpine valley is that which is formed by a succession of level basins rising in steps one above the other as they approach the head of the valley, and connected by gorges whose opposite sides often approach near together, and which are always much narrower than the basins that they link together. There is often distinct evidence, and usually good reason to suspect, that these basins were originally lakes; and in the walls of the gorges intermediate it is common to see proof that at some former period the depth of these lakes must have been greatly increased by barriers of rock, that once held them in and at a later period were cut through by streams of ice or water. I do not pretend that the type of valley above described is universal; but it is too common, both in the Alps and other mountain countries, not to present a formidable difficulty in the way of Professor Tyndall's bold hypothesis. Even though I should admit, as I cannot do, that such lakes as still exist, or have existed, could be formed by a glacier, the existence of a succession of such approximately level steps, separated by steep slopes and narrow

gorges, is to me a conclusive proof that some agency other than that of ice must have directed the original formation of valleys of this type. In one respect the inequalities of the sides of alpine valleys offer a stronger argument against their glacial origin than those of their bed. The surface of the ice, even near the side of the glacier, moves much faster than the part in contact with its bed; and it suffices to cut away the foundation of a lofty cliff at the point where the glacier abuts against its base, to determine its fall. Looking at the operation as a whole, it is to me quite inconceivable that a glacier should be competent to scoop out valleys a mile or more in depth, and yet be unable to remove the main inequalities from its own channel.

A comparison of the effects of existing glaciers with those of the present torrents must lead many observers besides Professor Tyndall to the conclusion that the former are far more powerful excavators than the latter, but do not to my mind justify the inference that no assignable limit can be placed to the work which they have actually accomplished. Vast as the period may be during which glaciers occupied many of the valleys of the Alps, it was not long enough to enable them to accomplish more than a certain definite amount of excavation; and I think it likely that the amount may hereafter be determined with some degree of accuracy. Indications are not wanting both of what glaciers have achieved, and of what they have failed to achieve. Ever since the range of Mont Blanc assumed its present form, before the so-called glacial epoch, and during that long period, and ever since down to the present hour, the snow that accumulates in the basin of the Mer de Glace has been discharged into the valley of Chamouni by the ice-fall of the Glacier des Bois. During the whole of that immense period, the uttermost mechanical effect of the glacier upon its bed has been applied to grind down the ledge over which it has to flow, and to bring the channel to a uniform slope. It is impossible to doubt that a considerable effect has been produced; the ledge has been lowered perhaps by 1000 feet, possibly more; the angle which the slope of the ice-fall makes with the glacier above has been rendered much more obtuse; but still the ice-fall remains, and the bed of the glacier has not been ground down to the uniform slope.

I refer to a well-known glacier, familiar to many readers, though I can conceive a partial, but, I think, an insufficient answer to my argument, founded on local conditions, and not on general grounds. I could easily cite many similar instances which would be less open to cavil. The same argument, *mutatis mutandis*, might be applied to lateral obstacles, such as the rocks known as l'Angle on the west side of the same glacier.

It is impossible to leave this part of the subject without some

brief reference to the ingenious suggestion of Professor Tyndall, who perceives in the operations of the glaciers upon the rocky framework of the Alps, not merely the agent that has hollowed out the existing valleys, but one competent, by its destructive power, to bring about the change from the present climate of the Alps to that which prevailed when the glaciers descended to the plains. If I could have the pleasure of standing beside him on the Superga, or any other central position from whence he could survey the outline of the main chain of the Cottian and Pennine Alps, serrated with peaks and intervening depressions, I would beg of Professor Tyndall to consider in detail whether, and by what means, glaciers, however extensive, or endowed with whatever mechanical power, could have determined the form of the topmost crest of that great range, or can hereafter reduce it below its present level. It will certainly not escape him that the grinding power of ice is exerted to an appreciable extent only by considerable ice-streams flowing through defined channels, when they have reached the level at which the temperature of the whole mass is brought near to the melting-point, and that the snow and ice which cover the highest parts of the Alps, so far from tending to hasten their downfall, act as a protection from other agents of destruction; so that in truth no rocks, except those at a considerable depth below the surface of the sea, are so little exposed to degradation as the tops of high snowy mountains. Whatever effect glaciers have produced in reducing their own limits, must have been effected, not by lowering the general level of the ridges of the Alps, but by deepening the hollows, and unfitting some portions of the area from serving as reservoirs for the accumulation of snow. Professor Tyndall, if I may venture to say so, has thrown additional light on the recent history of the Alps and other high mountain countries by directing attention to one of the causes that must have contributed to reduce within narrower limits the action of glaciers, but has not gone near to proving that the deepening of the main channels suffices, without other climatal changes, to account for the disappearance of glaciers that formerly reached to the level of the plains of Piedmont and Lombardy, to a distance of 30, 40, and even 80 miles from the limits of the existing ice-streams.

I pass over many facts of secondary importance which seem to me to confirm the views above advocated, merely glancing at one which is familiar to most persons who have studied the working of the ancient glaciers of the Alps—the record, namely, which is preserved by the harder rocks of the former limits of glacier-action. In scanning the rocky sides of the higher alpine valleys, the contrast between the surfaces that have once been subject to the passage of a glacier, and those that were not reached even by

the highest level of the ancient ice-flood, has struck every observer. Where the rock is hard enough to resist subsequent degradation, the eye follows the track of the former glacier as easily as it traces high-water mark by the fringe of sea-weed on the shore; and if this mark avails as positive evidence of the former extension of glacier action, it is not less a negative proof that that action did not surpass certain assignable limits.

I shall now offer some brief remarks upon Professor Ramsay's theory of the origin of alpine lakes. Although M. Mortillet had anticipated Professor Ramsay in attributing the formation of the lakes of Lombardy to the action of ancient glaciers, his speculations on the subject are far more guarded, and at the same time fall short of the generality which marks those of the eminent British geologist; it is to the latter, therefore, that I shall in the first place address myself.

Professor Ramsay's argument may be summarily stated in a few words. Each of the great alpine lakes lies in an area once covered by glacier; no satisfactory explanation of the origin of alpine lakes has yet been given; the glacier considered as a mechanical agent is competent to scoop out the rock basins in which the alpine lakes generally lie; therefore, the lakes have been formed by glaciers.

The first of these propositions, even though it be admitted with some reserve, does not hold inversely, as it should do if the scooping out of rock basins were one of the natural functions of glaciers. Why should not the glacier that flowed from Susa to beyond Turin, or the still vaster mass that descended from the Val d'Aosta, excavate a basin as deep and large as the Lago Maggiore or the Lake of Como? Why throughout the Dauphiné Alps, or the far more extensive region of the Tyrolese, Salzburg, Carinthian and Styrian Alps, are no lakes found in the path of the great extinct glaciers? Again, in admitting the proposition in its direct form, it is necessary to draw a marked distinction between the assertion that the existing lakes lie within an *area* once covered by glacier, and the fact that *some of them* lie in valleys which once gave passage to the *main stream* of an extinct glacier. Even though the efficiency of the glacier as an excavating tool were demonstrated, instead of being, as I feel sure, capable of disproof, it is not easy to conceive how it could have been applied to the hollowing out of such basins as the Lake of Lugano or the Lake of Zug. The mountain-valleys that are drained into the former lake are of the most trifling dimensions, and their height relatively insignificant. The main supply of ice to the basin of the lake was, on the one hand, from the glacier of the Adda, then occupying the site of the Lake of Como, across the ridge between Menaggio and Porlezza, and, on the other,

from the glacier of the Tessior over the pass of Monte Cenere. The relatively slender streams which flowed over these barriers might suffice under given climatal conditions to fill the lake-basin with ice, which would spread, as really occurred, beyond its present limits; but the result must have been to produce an ice lake rather than an ice river; and the boldest speculator in glacial theories must hesitate to assert that any agency was there present that could even tend to excavate a trench which, according to the latest measures, is 919 feet below the present level of the lake.

The difficulty of applying this theory, even if it were otherwise tenable, to such a lake as that of Zug, 1279 feet in depth, or to others that might be cited, need not be discussed in detail. The facts simply show that water, whether in the solid or liquid state, tends, under the action of gravity, to seek the lowest level.

With reference to that part of Professor Ramsay's memoir which tends to clear the way for the admission of glacier-action by denying the validity of other explanations of the origin of lakes, it would be rash in me to enter into controversy with so accomplished a geologist; yet I own to the belief that the causes may be various, and that, until we have acquired more accurate knowledge of the processes by which mountain chains have been uplifted, it is premature to declare ourselves incompetent to explain the depressions of the surface with which the mountain ridges are correlated. In any case we are not entitled to argue from our own ignorance to the admission of a new agent, until its competency shall be proved by direct evidence.

I am thus led to examine the point upon which the new theory really turns, and which, as it seems to me, has been taken for granted rather than cautiously investigated. The assumption, which is common to Professor Ramsay and to M. Mortillet, that the excavating power of a glacier in hollowing out a plain surface, or in deepening an existing basin, is proportioned to the weight of the mass pressing on its bed, seems to me to rest upon a superficial view of the mechanical conditions of glacier-motion. It has long been demonstrated that, even when lying on a slope considerably inclined, the friction of the rocky bed against the under surface of the glacier suffices to retard the motion of the lower portion of the ice-stream so that this bears but a small ratio to the velocity of the upper surface. It is impossible to doubt that this retardation would be increased very largely in the case of a glacier lying upon a level though irregular surface, though, if this were of slight extent, the weight and momentum of the glacier behind might impress upon it some slight progressive movement. But if we suppose a glacier of great thickness to lie on a level surface many miles in extent, it is easy to

see that the movement of the lower surface of the ice would be completely stopped, and that the onward movement would be effected by the flowing of the upper over the lower portions of the mass. It is forgotten that the resistance offered by friction would be increased to an almost infinite extent, while the resistance of the substance of the glacier to internal rearrangement is confined within moderate limits. M. Mortillet has estimated that the pressure of the ice upon the bed of certain ancient glaciers occupying lake-basins must have reached 300 and even 500 tons per square metre; but if this pressure were even ten times as great, the effect would only more certainly have been to force the ice of the lower surface into every inequality of the bed, and to make the resistance to onward movement more and more insuperable. When the front of the glacier is supposed to move up a slope, as in Mr. Ramsay's diagram of the Lake of Geneva, the above reasoning applies *à fortiori*. If, on further consideration, Professor Ramsay should feel doubtful on this point, I would urge him to make direct observations, which may tend to satisfy his own mind, and that of others who may naturally be swayed by his authority. It is not easy to find the conditions under which such observations could most usefully be attempted; but the glacier of La Brenva, and some others where the ice impinges against fixed obstacles, may furnish matter of instruction.

It will be obvious to the reader, that the question here discussed has a considerable bearing upon Professor Tyndall's views of the formation of valleys. The reason why the action of a glacier is more limited than appears probable to one who considers the enormous mechanical power developed in its progress, is because the greater part of that power is expended in overcoming the resistance of the mass to internal rearrangement, and a small portion only is applied to the onward motion of its lower surface, by which alone any mechanical effect is produced on the subjacent rocks.

If it should not appear superfluous to discuss at greater length the mechanical question involved in Mr. Ramsay's theory, I might ask how we are to believe that the glacier of the Rhone, after it had flowed out of its native valley, when, being no longer confined within a defined channel, it had covered with ice the plain of Switzerland as far as the Jura, was yet able to excavate a rock basin 50 miles and more in length, 12 miles broad, and nearly 1000 feet deep, while at the point where its mechanical power was at its maximum, in the defile of St. Maurice, it has failed to cut through rocks of no extreme hardness a more spacious opening than that which the traveller still sees between Bex and Martigny. It is a complete under-

estimate, to assert that the glacier must have moved through that defile at a pace twenty times greater than its rate of motion anywhere over the level of the lake; yet we see how limited its effect has been upon the containing walls of the valley.

A very interesting paper by M. Mortillet, referred to by Professor Ramsay, but not as generally known as it should be in England, is mainly devoted to prove that the lakes of Lombardy must have at one time been filled up by the post-pleiocene diluvium, which forms so important a deposit throughout the entire valley of the Po, and that they have been cleared out by the glaciers which descended into them during the subsequent glacial epoch. M. Mortillet is careful to admit that against solid rock the glacier would be comparatively inoperative, but he assumes rather than seeks to prove that the case would be different in regard to the incoherent masses of rolled pebbles, sand, &c. which make up the diluvium. M. Mortillet is a shrewd reasoner, and he has made out a good *primâ facie* case for his hypothesis, which does not present such formidable difficulties as that of Professor Ramsay; but I own myself to be sceptical as to the possibility of a glacier under the supposed circumstances excavating, even in yielding materials, trough-like basins which vary in depth from 900 to 2600 feet. I feel convinced that the whole effective work must have been accomplished during the advance of the glacier by the front of the ice, which might probably, to a limited extent, have ploughed out a furrow in soft materials; but from the moment when any portion of the diluvium became covered over by the glacier, the enormous weight of the ice would tend to consolidate it, and, for the reasons already given, the grinding action produced by the advance of the lower surface of the ice over its bed would disappear, or be reduced within very narrow limits.

An additional difficulty, fatal, as I think, to Professor Ramsay's theory, and hard to reconcile with that of M. Mortillet, arises from the presence in most alpine lakes of projecting points of rock, and sometimes of rocky bays and coves facing in the direction towards which the glacier formerly flowed. An instance which may be familiar to some readers is the rocky promontory, extending into the Lake of Como, south of Tremezzo, whereon stands the Villa Arconati. The glacier, whatever its efficacy as a tool may have been, worked in one direction only, and cannot have scooped out hollows in a direction contrary to its own current. Such irregularities as I speak of cannot be accounted for on Professor Ramsay's theory; and according to M. Mortillet we should expect to find them at least partially blocked up by diluvium, remaining *in situ* in those places where it had not lain in the path of the glacier stream.

The vast moraines which circle round the opening of the greater valleys on the southern side of the Alps, and extend beyond the limits of the existing lakes, rest upon diluvium formed of the same materials as the moraines, but extensively water-worn, the pebbles being rounded and usually smaller in size, and the materials partially sifted and imperfectly stratified. The moraines have evidently been left in their present position by the ancient glaciers which filled up for a long period the lake-basins; but before the glaciers had filled these basins, how was the diluvium carried across such long and deep hollows as those occupied by the great Italian lakes? M. Mortillet argues, with apparent justice, that in order to reach the plain it must have first filled up the intervening hollows. The conclusion does not, however, seem to me necessary; and I will venture to suggest two alternatives, either or both of which in succession seem to me more satisfactory.

It is quite certain that during the period when the diluvium was being reduced to its present condition, and was spread out over the valley of the Po and the plain of Friuli, that tract was the bed of a shallow sea; and there is good reason to suspect many oscillations of level between the meiocene period and the last great extension of the glaciers, during which the region in question may have been alternately uplifted and submerged. Geologists who have renounced the doctrines of the cataclysmal school have yet retained much of the language and habits of thought which were there learned. It is still their tendency to speak and think as if the history of the earth were that of a series of great events (revolutionary periods) separated by intervals of repose, rather than a slowly rolling cycle of incessant change wherein the same natural agents, perhaps in altered combinations and with varying intensity, constantly recur. Since the importance of ice as an agent in geological change has been clearly demonstrated, geologists have concentrated their thoughts upon a single period immediately preceding the establishment and actual distribution of the present fauna and flora, and have, in accordance with their habitual tendency, called this *the glacial epoch*. I am of course aware that some geologists have pointed out the traces of glacial action at former geological periods; but it cannot be said that they have generally recognized the reasonable conclusion, that if one of the latest conditions of the earth admitted of a great extension of glacial action, a similar combination of causes must probably have brought about the same effects at many recurring periods.

To my mind there is nothing strange in the supposition that the materials of the diluvium of the valley of the Po were brought down from the upper valleys of the Alps and across the

lake-basins by glaciers, and by them shot out as rubbish on the margin of the plain, before the latest invasion of the sea, exactly in the same manner as we know that the still existing moraines were at a later period deposited. If the sea rose before the glaciers retired to the upper valleys, ice-rafts of great dimensions laden with moraine would have floated down the fiord-like inlets, and would have rapidly melted on reaching the open sea. Whenever this did not rise much above the present level of the lakes, it is likely that the materials of the diluvium were accumulated near the lower end of each lake by the stranding of the ice-rafts on the shallow bar. It is even doubtful whether some supposed moraines have not been formed in this manner. In either or both of the two modes here indicated (by a solid causeway of ice, or by floating ice-rafts) I believe that the diluvium, as well as the more recent glacial deposits, have been borne from the central regions of the Alps to the plains of northern Italy; and I am the more persuaded of this because I know of no other means by which so vast a mass of solid materials could have been carried so great a distance. The action of running water has been perhaps underrated of late as a means of cutting a passage through even hard rock; but it is difficult to think too meanly of it as an agent for the transport, on a great scale, of solid fragments over a wide space, such as intervenes between the St. Gothard and the plain below Arona, or between the Stelvio and Monza, the one distance approaching near to, the other considerably exceeding, 100 miles. Fine sand and finer mud are carried in vast quantities by existing currents; but even where these appear to bring down rolled specimens of the rocks that surround their sources, it generally turns out that such pebbles are picked out from some ancient ice-borne deposits lying near the banks or in the bed of the stream. The effects even of such exceptional events as the well-known flood in the valley of the Drause, the like of which recur at intervals in every part of the Alps, are limited within very moderate limits; and, though, the Lake of Geneva were filled up to facilitate the transport, I doubt whether a million of Drause-floods would bear any appreciable amount of diluvium to the site of the city of Geneva.

If this were the proper occasion, there are many observations in M. Mortillet's valuable papers that deserve further discussion, and some of the details in his sketch of the ancient glaciers of northern Italy, to which I should be inclined to except; but I pass these over, and also forbear some remarks which I might offer upon two interesting papers by M. E. Desor, wherein the writer attempts a classification of alpine lakes, which, saving some details, seems to me founded on just views.

I might here close these remarks, which are directed rather to combat what appears to me excessive in recent theories, than to attempt a complete explanation of the origin of alpine valleys and lakes. Probably the time is not yet come for a solution of the difficulties connected with this great problem; but a writer with no scientific reputation to defend, who has often been led to reflect on the subject in the presence of those great monuments of the past history of the earth that survive in the Alps and in other mountain districts, may throw out for future discussion the result, though confessedly incomplete, of his own efforts to account for the phenomena.

I may mention that my ideas on this subject have been formed in all but utter ignorance of the views of those geologists who have maintained opinions somewhat similar, and in particular of those of Constant Prevost and Dana. I am very imperfectly aware of the progress of speculation even at present in the same direction; but it seems to me that, while both the writers above-named, and several others, have proceeded in the right track, they have not followed out the fundamental conception with the requisite strictness to its legitimate consequences. That the earth has gradually cooled down from a previous high temperature during the period which has intervened between the earliest geological record and the latest tertiary epoch, and that by a necessary consequence it has diminished in volume during the same period, is the belief of many, probably of nearly all geologists. This hypothesis, duly followed out, leads us towards an explanation of the origin of mountain chains, and the correlated depressions, which, if not quite complete, appears to me far more satisfactory than any other, and readily applicable to the leading points of the orography of the Alps. Those who have speculated on the subject seem to me to have erred in looking to local subsidence as the chief mechanical result of contraction by cooling, and more especially when they have supposed certain areas of the earth's surface to have been, in a special and exceptional sense, areas of cooling.

The postulates which may, as I think, be justly assumed for the purpose of reasoning on the hypothesis in question are these:—

1. From the period when organic life commenced on the surface of our planet, the outer portion of the crust must have assumed a temperature not far removed from that which it now possesses, and which depends mainly upon the ratio between the heat received from the sun and that lost by radiation.

2. The inner portion of the crust, and the nucleus, usually supposed to be viscous or fluid, being cooled with extreme slowness by conduction, would contract much more than the outer surface of the spheroid.

3. The process of cooling would be continuous and uniform, subject only to trifling local variations arising from the unequal accumulation of sedimentary strata over particular areas, and the unequal conductive power of certain portions of the crust.

4. No substance is absolutely rigid; and there is no reason to doubt that amorphous masses of mineral matter, such as constitute all rocks, are capable, under adequate force, of a considerable amount of flexure.

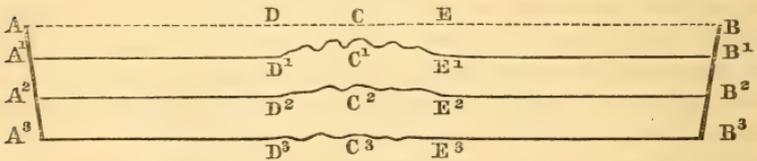
5. From a very early period in the earth's history, dating from the first appearance of water on the surface, if not earlier, inequalities in the outer surface, arising from denudation in some areas, and the accumulation of sedimentary strata in others, must have begun to exist, and have been continually extended and varied ever since by the action of the various causes that have brought the crust to its present condition.

It is conceivable, and even probable, that during some considerable period of the earlier history of the earth, the amount of contraction from cooling in successive concentric shells may have continued to be approximately proportional to their distance from the centre of the spheroid, in which case no mechanical forces would be called into play that would tend to alter the form of the outer surface. The conditions were altered from the time when the outer crust, having approached to a constant temperature, ceased to keep pace with the contraction of the interior, and I ask the reader to consider the necessary effects of that change.

To fix our ideas, let us take an area of 500 miles squared, and assume that during a certain period (which would be of enormous length if counted by years) the radius of the earth had contracted through cooling by 1200 yards, equal to about a 6000th part of its own length. Using round numbers, which suffice to illustrate the argument, the result of this change will have been to force the outer crust of the area under consideration to occupy a space less by 150 yards in every direction than before the contraction. If the crust were of uniform rigidity throughout the same area, and also in the adjoining regions, the result must be a general crushing and crumpling of the surface; but we have taken it for a certainty that in reality the condition of things has been otherwise, and that lines of least resistance have existed which must have determined the yielding of the crust in one direction rather than another. Let us assume that in the regions adjoining the given area the preponderating direction in which the crust had already yielded was N. and S., in which case the pressure transverse to that direction will have been diminished, and therefore that, during the given period within the same area, the forces acting on the crust must be

mainly lateral forces compressing the mass in the direction of the meridian. To form a more accurate idea of the changes which would result, let us take an imaginary meridional section 500 miles in length, and suppose that the least-resisting part of the crust lies at the centre of the section, the rigidity increasing gradually on either side. As we have already seen, the mechanical effect which will be produced by the action of gravity (assumed to be capable of overcoming the resistance of the mass) will be to force each extremity of the section to approach the centre by 75 yards. The first flexure having occurred at the centre, the ordinary laws of the resistance of imperfectly elastic and imperfectly rigid bodies lead to the formation of parallel ridges with intermediate depressions extending on either side of the first flexure within limits depending on the flexibility and compressibility of the crust. We shall assume the limit at 50 miles on either side of the centre. The annexed figure will give some idea of the nature of the vertical disturbances, the scale being very greatly magnified to make them sensible to the eye, and no attempt being made to exhibit the curvature of the surface.

Fig. 1.

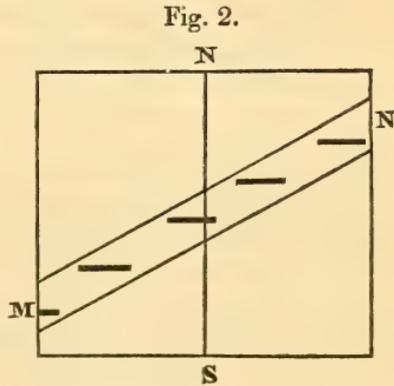


The dotted line AB shows the section of the original surface before the period of disturbance, and A^1B^1 that after subsidence, with a series of ridges and depressions, highest at the centre C^1 , and extending to D^1 and E^1 respectively 50 miles from C^1 . No vertical disturbance will have arisen in the portions A^1D^1 and E^1B^1 , but a displacement has nevertheless been caused which deserves attention. Having left out of view the changes going on in neighbouring areas, we must assume that the subsidence from A and B to A^1 and B^1 has been vertical; but in that case the portion of the section AD , which by hypothesis has not sensibly contracted by cooling, and which has not suffered flexure, can assume its new position A^1D^1 only by the relative displacement of D^1 60 yards nearer to C^1 , and a general displacement of the whole section A^1D^1 , which will gradually increase from A^1 where it is *nil*, to the maximum at D^1 . The same displacement in the opposite direction would occur in the subsidence of EB to E^1B^1 .

Between the surface AB and a stratum lying at some depth, probably considerable, where the rate of cooling would be sensibly the same as that of the nucleus, there must be an

intermediate condition of portions of the crust which well deserves notice. The lines $A^2 B^2$ and $A^3 B^3$ represent the condition of some portions of the crust where the amount of cooling during the assumed period has caused a contraction of 50 yards and 100 yards respectively in the length of the portion lying between the verticals $A B^3$ and $B B^3$. In each case the insufficient contraction of those portions of the crust must be supplemented by some flexure and disturbance of the strata, but the deeper we descend, and the more the rate of cooling has approached to that of the interior, the less disturbance will have arisen.

In the illustration here proposed, it will be seen that if the zone of least resistance had happened to lie due E. and W., every meridional section across the same area would show the point of maximum flexure at the centre, and we should have a single mountain chain running due E. and W. across the middle of the area; but if the zone of least resistance should happen to lie in some direction not perpendicular to the meridian, we should have a series of parallel ridges arranged as in the annexed figure, where $M N$ represents the zone of least resistance, and the broad black lines the axes of as many systems of parallel ridges. The fact that such a disposition of mountain ranges frequently occurs, is familiar to all who have studied the orography of the Alps and other high mountain districts.

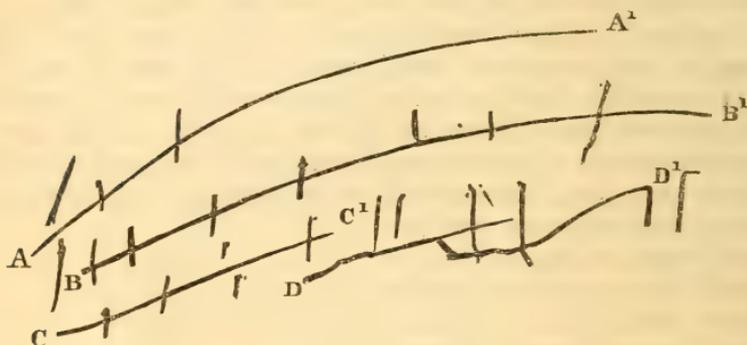


In the incalculable lapse of geological time during which the same causes must have continued to cause disturbances of the outer crust of the earth, but under varied conditions of resistance, it is clear, on the one hand, that the direction in which the force of compression would act on a given portion of the surface would be liable to continued but slow variation, and on the other, that there must be an interdependence between the disturbances arising in adjoining regions, even though these should cover very large areas. As a general rule it seems inevitable that a mountain district once formed would become for the surrounding regions a zone of least resistance, which would be liable to undergo new flexures according as pressure came to operate in new directions, and that we ought to find the traces of such transverse compression in the formation of parallel ridges lying at various angles to the main lines of flexure, and at the same time that these secondary ridges should be far less regular than

the primary ridges and furrows which mark the earlier corrugations of the surface.

In considering the mechanical effects of lateral compression applied transversely to the direction which has caused the earlier and predominant flexures of the surface, we are led to speculate on the probability of fracture owing to the inability of the ridges to yield to the necessary extent in a new direction. The actual

Fig. 3.



occurrence of such fractures would depend upon the plasticity of the materials; but the summit of each convex flexure would necessarily sustain a tension competent under given conditions to cause rupture. It can scarcely be by an accident that the annexed figure, which appears to be an ideal illustration, does actually represent, in a rude fashion, the main ridges and transverse valleys of western Switzerland and northern Savoy. If the lines A A¹, B B¹, C C¹, and D D¹ be taken to represent the axes of elevation of the Jura and the chief ranges of the Alps, the reader will have no difficulty in identifying in the short longitudinal lines the valley of Chambéry from Montmelian to Culoz, the valley of the Chéran, that of Annecy, the gorge of the Isère between Albertville and Montiers, the defile of Maglan, the valleys of Montjoie, Bonneval, and Tignes, the Val d'Entremont, and that of the Rhone from Martigny to Vevay, the Eringer Thal, Einfisch Thal, Saas Thal, the pass of the Simplon, the Valley of Hasli, the Valley of the Reuss, &c. It will not escape notice that nearly all the transverse valleys above enumerated, and many others of less importance that might be added to the list, are wholly or in part narrow defiles. There can be no doubt that water and ice have contributed largely to give them their present form; but the same causes have acted elsewhere without producing the same general character of valley.

This is not the occasion on which to attempt to apply in

detail the principles here advocated, nor do I pretend to have the requisite amount of special knowledge. They appear to me to account for a large number of the broader facts of general orography, and more especially for many of the peculiarities in the conformation of the Alps that have attracted the attention of geologists. Of the former class I may instance the almost continuous zone of mountains that traverse the eastern hemisphere between the 30th and 50th parallels of latitude, throughout which the prevailing tendency of the ridges approaches to parallelism with the equator, together with the general tendency towards a meridional direction in the ranges N. and S. of that zone.

Of the leading facts of alpine orography which find their explanation in the development of the views here propounded, I would in the first place note the connexion between the principal peaks and the secondary ridges that diverge from the main ranges of the Alps. The secondary ridges again, in their turn, very frequently show in their outline a succession of prominences and depressions which sometimes present a remarkable accordance on the opposite sides of a deep valley, and are most easily explained by considering them to result from the intersection of transverse lines of flexure—one set of furrows being represented by the existing valleys, and another by the inequalities in the ridges which enclose them. Another indication of the same phenomenon is apparent in the ancient lake-basins, very frequently filled up, which recur in succession as we ascend through so many valleys in the Alps. As already remarked, these lake-basins were formerly larger and deeper, being almost invariably separated by narrow defiles cut through barriers of rock which once enclosed them.

The formation of several of the greater lakes of Switzerland, which have been called lakes of erosion, may be shown to be the natural consequence of the continued action of the causes here pointed out. It will be obvious that whenever forces adequate to cause flexure act upon a mountain district in a direction different from that which has produced the pre-existing undulations of the surface, they will leave in the new ridges which they originate distinct evidence of their action, and, generally speaking, of the period at which they operated. But when the direction coincides with that of forces previously impressed upon the surface, the whole effect of the renewed action in the same direction will be to increase the existing inequalities, raising the ridges, and deepening the valleys, without leaving any indication of the period at which the action was resumed, or whether it had ever been interrupted. The great ridges and depressions which have determined the main features of the Alps, which in

Switzerland and Savoy lie between W.S.W. and E.N.E., and in the Eastern Alps are directed more accurately from W. to E., are the aggregate results of the action, whether constant or intermittent, of lateral compression ever since the elevation commenced, a period which probably extends from the origin of the earliest palæozoic rocks to the present time. Though the conception is fundamentally different from that of local subsidence, the effect may sometimes be scarcely distinguishable.

That the miocene period was one of immense duration is sufficiently proved even to geologists who know the formation only in Switzerland; and it is equally certain that it was accompanied by considerable disturbance of the surface. I can see no difficulty in admitting the probability of the action of forces transverse to the main chain of the Alps and the Jura during, and subsequent to, the miocene period, which would have deepened previously existing depressions on the site of the lakes of Neuchatel, Biemme, Morat, and the western part of the Lake of Geneva. The eastern part of the latter lake has probably a different origin. There is nothing, so far as I know, in the country between Bex and the Triassic rocks of Meillerie to make it unlikely that a lake may have existed there before the miocene epoch.

I shall very briefly notice a few of the difficulties which occur to some minds in regard to the main principles which are here very imperfectly developed. It is sometimes objected that the formation of ridges and furrows upon the rigid crust of the earth in the manner here suggested, without corresponding undulations of the more deeply situated portions of the crust, assumed to be viscid from high temperature, implies the formation of hollows and vacant spaces, owing to the want of conformity in the flexures of the upper surface with those of the inner mass. Without stopping to discuss the possibility of the occasional occurrence of such vacant spaces in certain strata, aided, it may be, by the more rapid cooling of the earth's nucleus which some suppose to have proceeded during preceding geological periods, I reply that the objection rests upon an imperfect idea of the action of force upon the solid materials of the globe. I have no doubt that all rocks possess in some measure that property which, when highly developed, we call *plasticity*, under the action of adequate force applied with the requisite slowness. During the process of flexure, as I understand it, a portion of the earth's crust lies, as it were, in a mould, which changes its form, it may be, at the rate of a few yards in a million of years; and the same quality which permits flexure permits that limited degree of mobility of the parts which suffices to fill up the space that would otherwise be vacated by the flexure. The fact that

such an effect has been produced is constantly exhibited on a small scale in the natural sections that abound throughout the Alps, wherein the existence of unconformable flexures is shown not to lead to the formation of hollows in the interior of a contorted mass of rock.

Another objection of a general character is derived from the existence of large portions of the earth's surface which exhibit no traces of considerable flexure, although the hypothesis requires us to admit the continued action, during an immense period of time, of forces competent to cause vertical disturbance. This objection has been partly anticipated in the preceding pages. It has been shown that at the time when the existing inequalities of the surface began, some adjoining regions may probably have escaped displacement, except a moderate amount of lateral shifting towards the area of disturbance; and it has been remarked that a mountain district, once formed, would probably continue to receive new flexures and thereby relieve the pressure on the adjoining areas. Furthermore, in reference to certain districts where very ancient sedimentary rocks appear to have remained nearly undisturbed, it is not impossible that overlying and more contorted strata may have been removed by denudation, and that the earlier strata may have partly escaped flexure for the reason already pointed out, viz. that they in some measure kept pace with the rate of cooling of the deeper portions of the crust.

The objections apparently most formidable, and of which I do not doubt that a numerous array may be adduced by men so well versed in the local geology of the Alps as M. Desor and Professor Ramsay—not to name others whose views I may controvert—rest upon the apparent discordance between the stratification of the sedimentary formations, and the flexures indicated by the present relief of the surface. I have no doubt that there are many facts apparently opposed to the views here advanced which I should find it difficult or impossible to explain: but I would observe that the problems raised are of extreme complexity; and if I am right in believing that the present conformation of the Alps is the complex sum of the operation of forces of compression in various directions, and under varied conditions, through an enormous period of geological time, I am entitled to assume that the unravelling of so tangled a network of causation must be a matter of all but insuperable difficulty. It will be obvious that, in studying the relations of the stratification in successive formations, we have to determine the probable condition of the surface at the time when each was deposited, and the various flexures which it has since undergone, which would differently modify the disposition of the beds according as they led to fracture, or bent the mass without breaking its continuity. Add to

these the effects of denudation on an immense scale, and the minor, but still important, mechanical action of water and ice, and it is not surprising if many difficulties long remain unsolved. At present the important point is to find the true clue through the labyrinth. Of some of the main difficulties in alpine stratification, and especially of what has been called the fan structure, I think that the hypothesis here advanced gives a better explanation than any other that I know. The final decision may probably await more minute and persevering study of the structure of the Alps than has even yet been given, although the names of great geologists, deceased or still living, seem to contradict the suggestion.

The hypothesis of lateral compression, in the form in which it presents itself to my mind, is absolutely inconsistent with the maxims of the cataclysmal school of geology. If mountain chains owe their elevation to the gradual cooling of the earth, it is a flat contradiction of the laws of physics to infer, as an American writer has done, the possibility of the sudden elevation of a mountain a mile in height, or even an incomparably less amount of rapid disturbance. More cautious reasoning should lead us to expect that the vertical disturbances would be absolutely insensible, but that the lateral displacements, encountering greater and more unequal resistance, might sometimes cause sensible effects. Whether we may here find a cause of earthquakes, indirectly connected with the elevation of certain mountain chains, is a question which I am not now prepared to discuss.

It is scarcely necessary to advert to exceptional causes which may have operated in the formation of certain mountain-valleys.

For instance, in the Eastern Alps there seems reason to suppose that the chemical changes which led to the formation of dolomite were accompanied by contraction in the mass of corresponding portions of the Jurassic rocks, causing irregular cracks and fissures, which, enlarged by marine action, have produced the irregular disposition of the valleys, and the characteristic forms of the peaks in that district.

To conclude, I am persuaded that there is no single valley of the Alps that does not owe its present form in great part to the action of glaciers, and it is a great gain that all the modes of action of this powerful agent should be fully studied; but, while believing that the main features of the surface have originated in more general causes, I feel surprise that Professor Tyndall has not referred more pointedly to another agent which has surely had even a larger share than that of glaciers in fashioning the great alpine masses. He rightly rejects running water as a means of extensive excavation, but he does not seem to have considered the effects of long-continued marine action upon steep

rocks. By that agency alone, exerted during the gradual emergence of the main chain of the Pennine Alps, does it seem possible to explain such an operation as the hewing out of the stupendous peak of the Matterhorn, whose origin must doubtless have occupied his speculative faculties during those daring expeditions that have so nearly led him to that summit which alone seems able to defy the utmost efforts of the present race of mountaineers.

Pisa, January 3, 1863.

XIII. *On the Chemical Composition of some Chilian Minerals.*

By DAVID FORBES, F.R.S. &c.*

Hydrous Bibasic Arseniate of Nickel and Cobalt.

THIS mineral, apparently a new species, occurs in veins in a semi-decomposed greenstone or dioritic rock, which breaks through the upper oolitic strata in the Desert of Atacama at about twenty leagues to the eastward of the port of Flamenco. The upper part of these veins contain it in abundance; but after a few yards in depth it appears to pass into chloanthite, from which mineral it appears to have been originally derived.

It occurs in fibro-crystalline masses, an agglomeration of crystalline fibres united longitudinally; colour greyish white; lustre non-metallic, dull up to silky or resinous; streak non-metallic, earthy; powder white; hardness 2·5.

The specific gravity of three different specimens was respectively as follows: 3·134, 3·069, and 3·054, consequently a mean of 3·086.

Before the blowpipe alone, upon charcoal in reducing flame, it evolves water, changes colour, fuses imperfectly, and produces metallic globules of a basic arsenide of nickel and cobalt, evolving at the same time arsenical fumes in abundance. In the oxidating flame alone it is nearly infusible. With borax in the oxidating flame it dissolves completely, forming a dirty brownish-blue glass, owing its colour to the oxides of nickel and cobalt dissolved; in the reducing flame with borax it yields a bluish glass, along with a metallic globule of basic arsenide of nickel and cobalt. When heated in a closed tube, it does not decrepitate nor change form, but evolves water and darkens in colour, becoming then of a dirty greenish- or brownish-grey colour; in open tube it gives the same reactions.

Upon analysis, the following percentage results were obtained:—

* Communicated by the Author.

Arsenic acid	44·05	} 28·95	} 73·00
Protoxide of nickel	19·71		
Protoxide of cobalt	9·24		
Water	26·98		
	<u>99·98</u>		

These results immediately identify themselves with the formula $(\text{NiO} + \text{CoO})^2 \text{AsO}^5 + 8\text{HO}$, which, expressed in percentages, would require

Arsenic acid	43·89	} 72·52
Protoxide of nickel and cobalt.	28·63	
Water	27·48	
	<u>100·00</u>	

From the above, it will be seen that this mineral is analogous to pharmacolite, $2\text{CaO}, \text{AsO}^5 + 6\text{HO}$, in which all the lime is replaced by the protoxides of nickel and cobalt, but differs in having eight instead of only six equivalents of water: in this last respect it resembles the other tribasic arseniates of nickel, cobalt, and zinc—Annabergite, erythrine, and Köttigite.

It is remarkable that this mineral is colourless notwithstanding its containing so large a proportion of the protoxides of nickel and cobalt, the combinations of which are so generally distinguished by their more or less prominent tints.

Along with the above-mentioned mineral is frequently found a sulphur-yellow compound in thin amorphous scales, or rather crusts: on qualitative examination it yielded arsenic, nickel, and cobalt, but was extremely difficult to obtain entirely free from the preceding mineral, from which reason it has not as yet been analysed. Possibly it may be the mineral from Johann-Georgenstadt examined by Bergmann*, with the following results:—

Arsenic acid	50·53
Protoxide of nickel	48·24
Protoxide of cobalt	0·21
Protoxide of copper	0·57
Sesquioxide of bismuth	0·62
	<u>100·17</u>

from which is deduced the formula $3\text{NiO}, \text{AsO}^5$.

Bismuthic Silver.

The locality of this mineral is the silver mine of San Antonio de Potrero Grande, about 35 miles to the south-east of Copiapo; the mine is situated in the beds of porphyry conglomerate and breccia of upper oolitic age, the interstices of one or more of

* *Journal für Praktische Chemie*, vol. lxxv. p. 239.

which have been filled in with metallic matter, doubtlessly injected at the time of the eruption of the dioritic rock in close proximity.

The bismuthic silver I found in conjunction with Domeykite, which latter is frequently difficult to separate entirely from it; otherwise, on examination with a magnifier, the bismuthic silver appeared to be quite free from any extraneous mineral substance. The specimen analysed by Domeyko afforded to him

Silver	60·1
Bismuth.	10·1
Copper	7·8
Arsenic	2·8
Gangue	19·2
	<hr style="width: 100%; border: 0; border-top: 1px solid black; margin: 0;"/> 100·0

In this analysis the copper and arsenic are so nearly in the proportion of Domeykite (which would require 7·2 copper to the 2·8 arsenic present) that it may safely be considered to be the case. Deducting therefore this amount, as well as the gangue, the proportion of silver and bismuth expressed in percentages will stand as follows:—

Silver	85·61
Bismuth	14·39
	<hr style="width: 100%; border: 0; border-top: 1px solid black; margin: 0;"/> 100·00

results which would appear to indicate the formula $Ag^{12}Bi$, which upon calculation should contain

Silver	86·17
Bismuth	13·83
	<hr style="width: 100%; border: 0; border-top: 1px solid black; margin: 0;"/> 100·00

The mineral called bismuthic silver, and classed under that head by Dana*, is in reality nothing more than a mere mixture of various metallic sulphides. The analysis by Klaproth is given as

Sulphur	16·3
Bismuth.	27·0
Lead	33·0
Silver	15·0
Iron	4·3
Copper	0·9
	<hr style="width: 100%; border: 0; border-top: 1px solid black; margin: 0;"/> 96·5

It is only necessary to calculate the equivalent amount of sulphur requisite to form sulphides with the respective quantities given of each metal, to see at a glance that the amount of sulphur

* Dana's 'Mineralogy,' 4th edit. vol. ii. p. 16.

found in the analysis is precisely sufficient to combine with the whole of the metals present and form sulphides with them, consequently leaving no surplus whatever either of uncombined metals or of sulphur.

This will be evident from the following statement:—

Sulphur . . .	16·30		
Bismuth . . .	27·00 +	6·11	Sulphur forms 33·11 Bi ² S ³
Lead . . .	33·00 +	5·07	„ 38·07 PbS
Silver . . .	15·00 +	2·22	„ 17·22 AgS
Iron . . .	4·30 +	2·45	„ 6·75 FeS
Copper . . .	0·90 +	0·45	„ 1·35 CuS
	<u>96·50</u>	<u>16·30</u>	Sulphur. <u>96·50</u>

Antimonial Silver.—Discrasite.

Amongst the argentiferous minerals found in Chanorcillo, Domeyko* enumerates, without further description, a combination of antimony and silver, which yielded to his analysis

Silver	0·64
Antimony	0·36
	<u>1·00</u>

From these results, it will at once be seen that this mineral must be totally different in composition from either of the varieties of discrasite hitherto described, and which have respectively the following formulæ and percentage composition:—

Ag ⁶ Sb {	Silver . . . 83·4	Ag ⁴ Sb {	Silver . . . 77·0
	Antimony . 16·6		Antimony . 23·0
	<u>100·0</u>		<u>100·0</u>

So that this new compound is probably represented by the formula Ag² Sb, which would require

Silver	62·61
Antimony	37·39
	<u>100·00</u>

Besides this mineral, another compound of antimony with silver is also found, and much resembles in appearance native silver, especially when burnished, but can easily be rubbed to powder in a mortar, which at once distinguishes it. A specimen of this mineral obtained from the mine Rosario contained, according to Domeyko,

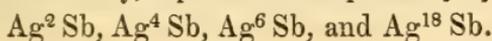
Silver	94·2
Antimony	5·8
	<u>100·0</u>

* *Tratado de Ensayes*, 2nd edit. p. 238.

with traces of arsenic; other specimens are stated to have yielded from 4 to 6 per cent. of antimony along with some arsenic. It would therefore appear probable that this mineral is represented by the formula Ag^{18}Sb , which requires

Silver	93·78
Antimony	6·22
	100·00

We consequently now have no less than four distinct compounds of silver with antimony, represented respectively by the formulæ



Arsenio-antimonide of Silver.

This mineral occurs in the form of grains disseminated throughout carbonate of lime, and is very similar in appearance to native silver. On treating the mass with acetic acid, the carbonate of lime dissolves and the mineral remains behind in the form of brilliant metallic grains, which, when submitted to the action of the blowpipe, emit abundant fumes of arsenic and antimony. The analysis by Domeyko afforded respectively,

Silver	53·6	53·3
Iron	3·0	3·0
Arsenic	23·8	22·3
Antimony	19·6	21·4
	100·0	100·0

Upon calculation, it will be found that the equivalents most approximating to these results are 10Ag, 2Fe, 6As, and 3Sb, which will give the following percentage composition:—

12	{	10Ag = 54·73 Silver.
		2Fe = 2·84 Iron.
9	{	6As = 22·81 Arsenic.
		3Sb = 19·62 Antimony.
		100·00

Considering the iron to replace a part of the silver, we have the formula $(\text{Ag Fe})^4(\text{As Sb})^3$. Domeyko looks upon the iron as being in the state of biarsenide*, and considers that the rest of the arsenic, along with the antimony and silver, are combined to form $\text{Ag}^2(\text{As Sb})^3$, which he considers is equivalent to a combination of one atom sesquiarsenide of silver with one atom sesquiantimonide of silver.

Arsenide of Silver.

Whilst in Copiapo several specimens were given me under this name, but, upon submitting these to chemical examination,

* *Elementos de Mineralogia*, 2nd edit. p. 190.

none of them were found to contain arsenic in true combination with the silver.

One specimen from the Retamo, north-east of Copiapo, was native silver in coarse interwoven threads, which upon inspection appeared to be regular octahedrons, as it were drawn out and elongated until their length equalled some fifteen times the measure of their short axis. Upon assay they yielded 99·86 per cent. silver.

Another specimen from Punta Brava, also near Copiapo, proved to be native arsenic massive, but curiously woven through, if the term may be used, with wonderfully fine filaments or threads of native silver free from arsenic.

When examining the Mina Margarita at Bandurias near Chanarcillo, I found, however, a peculiar dull grey mineral, semi-malleable, and which, on burnishing, received an almost silver-white polish, situated in quartz, in which it occurred as small grains or plates associated with smaltine. All the specimens I obtained appeared too impure to be then considered worthy of a quantitative analysis, but on examination by the blowpipe they gave a strong reaction of arsenic, and left a globule of silver behind on the charcoal.

Lately Domeyko has examined a mineral also from Bandurias, but mine not specified, which is stated to occur in a matrix of white argillaceous carbonate of lime, and in colour to vary from lead-grey to tin-white. Upon pulverizing the specimens, they were found to consist of a mixture of several metallic minerals independent of the gangue. A ductile mineral in granules flattened by the blows was easily separated from the more pulverizable portion, which in turn, by means of washing, was again separated into a heavy metallic powder, and a lighter, black, earthy-looking sediment.

These three products have been separately analysed by Domeyko, who, however, has contented himself with stating the numerical results of his analyses, without speculating as to their possible chemical constitution. The results obtained were as follows:—

	Metallic granules.	Metallic powder.	Black sediment.
Silver . . .	82·5	39·8	1·50
Mercury . . .	5·6	—	—
Iron . . .	0·3	13·8	1·90
Cobalt . . .	0·6	8·3	11·55
Nickel . . .	—	0·6	3·75
Arsenic . . .	10·1	27·1	53·70
Antimony . . .	0·8	1·0	—
Sulphur . . .	—	—	0·15
Gangue . . .	—	8·2	26·50
	<u>99·9</u>	<u>98·8</u>	<u>99·05</u>

In considering the composition of the first of these, or metallic granules, the ratio of the arsenic to the other bases is nearly that of the formula Ag^6As , which would give the following percentage composition :—

Arsenic	10·37
Silver	89·63
	100·00

And if we look upon the silver as being in small part replaced by the mercury, iron, and cobalt found in the analysis, the formula $\text{Ag, Hg Fe, Co}^6\text{As}$ doubtless represents the constitution of these metallic granules.

The second product (the metallic powder left behind in washing) is somewhat more complicated: deducting the gangue, and recalculating the results to percentages, we obtain the composition of the clean metallic powder as follows :—

Silver	43·93
Iron	15·24
Cobalt	9·17
Nickel	0·63
Arsenic	29·92
Antimony	1·11
	100·00

If now we unite the nickel with the cobalt as well as the arsenic and antimony, on calculation, the equivalents which approximate most closely to the terms of the analysis are $\text{Ag}^4, \text{Fe}^5, \text{Co}^3,$ and As^4 , which, expressed in percentages, gives

Silver	44·96
Iron	14·58
Cobalt	9·24
Arsenic	31·22
	100·00

The equivalents of the metals being to those of the arsenic in the ratio of 12 to 4, we have the formula $(\text{Ag Fe CO})^3\text{As}$, a tri-basic arsenide of silver with iron and cobalt, which represents the constitution of this compound.

We have now only to dispose of the black sediment, which was least in specific gravity. Deducting from the analysis already given of this product the gangue, and recalculating the percentages of the other components, the analysis will then stand as follows :—

Cobalt	15·92
Nickel	5·17
Iron	2·62
Silver	2·06
Arsenic	74·02
Sulphur	0·21
	<hr/>
	100·00

These results indicate at a glance the composition of smaltine, differing in no respect from the mineral usually met with, except in being somewhat more argentiferous. Notwithstanding the impurity of this mineral mixture, there can be no doubt as to the existence of one or more true compounds of silver with arsenic; and, as before stated, the formulæ Ag^6As and Ag^3As , which represent the compounds here described, very probably are definite mineral species, which on further search may be obtained in a purer condition.

Sulphide of Lead and Zinc.

This mineral is found in a mine at Ingahuas in the department of Huasco, where it occurs in the form of rather large lumps or nodules. It is lighter in colour, and possesses less lustre than galena, which mineral it in most points resembles, except in its peculiarly compacted and homogeneous saccharoidal structure, which, even when examined by a magnifier, discloses no intermixture of any other mineral, nor trace whatever of zincblende.

Its composition, according to Domeyko, is

Lead	48·6
Zinc	25·6
Sulphur	19·2
Gangue and loss . . .	6·6
	<hr/>
	100·0

Calculating the percentage of lead, sulphur, and zinc, after deducting the amount of gangue and loss, we have the composition of the pure mineral shown as follows:—

Lead	52·03
Zinc	27·41
Sulphur	20·56
	<hr/>
	100·00

which numbers approximate very closely to 2 equivalents lead, 3 zinc, and 5 sulphur, which require a percentage composition of

Lead	53·73
Zinc	25·58
Sulphur	20·69
	<hr/>
	100·00

so that the formula for this mineral would be $2\text{PbS} + 3\text{ZnS}$.

Taltalite.

This new mineral has, since the year 1858, been found in immense quantities in the mines of Señor Moreno, a short distance inland from Taltal in the Desert of Atacama, constituting the bulk of the supply of copper ores derived from those mines. It is of a black or brownish-black colour, with from a dull up to a satiny or vitreous lustre, and has a blackish-grey streak. It occurs in masses consisting of an agglomeration of long crystalline fibres apparently quite pure, but sometimes accompanied by Atacamite and copper-glance, which minerals are frequently interposed between the fibres. An analysis by Domeyko afforded

Protoxide of copper	44·5
Lime	2·4
Magnesia	0·8
Alumina	16·2
Sesquioxide of iron	11·3
Silica	20·8
Chlorine	0·7
Water (? as ignition loss)	2·5
	<hr/>
	99·2

from which results he has not deduced any formula.

The numerous specimens I obtained when in that part of Chile are all so extremely homogeneous and crystalline in structure as to put aside the idea of this mineral being a mere mechanical mixture, and enforce the conclusion that it is a definite compound.

The water which appears in the above analysis is most probably not essential to its constitution, and in part at least is doubtlessly united with the chlorine and some of the copper present, forming Atacamite, which, as before mentioned, is frequently found inserted between the fibres of the minerals. Calculating Atacamite as represented by the formula $\text{CuCl} + 3\text{CuO} + 5\text{HO}$, the amount of chlorine present (0·7) would be equivalent to 4·56 per cent. Atacamite, which, if deducted from the above results (along with the excess of water), and the quantities of the respective components recalculated to make up the hundred parts, will give the following composition as that of the pure mineral:—

Protoxide of copper.	44.56	containing Oxygen	8.88	} 9.93	} 21.78
Lime	2.58	„	0.72		
Magnesia	0.86	„	0.33		
Alumina	17.44	„	8.14		
Sesquioxide of iron.	12.17	„	3.71		
Silica	22.39	„	11.63	} 11.85	
	<u>100.00</u>				

The equivalents most approximating to the above composition are 9CuO , $1\text{Fe}^2\text{O}^3$, $2\text{Al}^2\text{O}^3 + 3\text{SiO}^3$, in which it is considered that the lime and magnesia present replace a portion of the protoxide of copper; these would give the following percentage composition for the mineral:—

Protoxide of copper.	49.28	containing Oxygen	9.91	} 11.54	} 21.45
Sesquioxide of iron.	12.73	„	3.90		
Alumina	16.36	„	7.64		
Silica	21.63	„	11.24		
	<u>100.00</u>				

As above stated, it will be seen that the amount of oxygen contained in the bases is double that of the silica, also that the oxygen contained in the protoxides is about equal to that of the sesquioxides; consequently we obtain the formula



in which the sesquioxide of iron replaces a part of the alumina, and the lime and magnesia a part of the protoxide of copper. This may probably represent the constitution of the mineral; but it is much to be desired that a new analysis be made of the mineral, and for this purpose that the purest portions obtainable be employed.

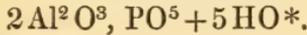
Hydrous Silico-phosphate of Alumina and Copper.

Under the names of cupreous phosphate of alumina and turquoise, Domeyko* has described a mineral whose chemical composition does not very correctly entitle it to either of these denominations. The mineral has a pale turquoise-blue colour, has a compact homogeneous earthy fracture, and is scratched by the nail. It is found on the estate of San Lorenzo de la Ligua, in the province of Aconeagua, where it forms blue veins in a white decomposed felspathic rock. When heated, it gives off water and changes colour, becoming of a yellowish-grey tint: it is decomposed by acids. Its chemical composition is stated as follows:—

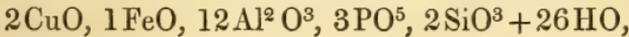
* *Elementos de Mineralogia*, 2nd edit. p. 425.

Protoxide of copper	6·3
Protoxide of iron	3·3
Alumina	46·3
Phosphoric acid	17·7
Silica	7·6
Water	18·8
	100·0

The presence of so large an amount of silica, and the small percentage of phosphoric acid (but little above one-half that contained in the turquoise), at once distinguishes this mineral from the true turquoise for which Dana has given the formula



The preceding analysis affords, on calculation, the equivalents



and considering the protoxide of iron present to replace a portion of the protoxide of copper, will give the following percentage results:—

Protoxide of copper	9·04
Alumina	48·39
Phosphoric acid	16·95
Silica	7·26
Water	18·36
	100·00

from which the formula



may be deduced as representing the constitution of this mineral, making it consequently to be a hydrated compound of tribasic silicate and sulphate of alumina, along with tribasic phosphate of copper.

Hayesine.—Borate of Lime.

In a report on the Geology of Bolivia and Southern Peru, made to the Geological Society of London†, I attributed the appearance of the borate of lime and other compounds of boracic acid occurring so abundantly in the northern part of the Desert of Atacama and province of Tarapaca in Peru, to the exhalations from the numerous volcanoes, whose vapours I supposed (although at that time this had not been proved to be the case in South America) to contain boracic acid; since then I have, by

* Mineralogy, 4th edit. p. 405, where, curiously enough, turquoise, although containing 20 per cent. of water, is classed along with the anhydrous phosphates.

† Quarterly Journal of the Geological Society, vol. xvii.

the discovery of the mineral Hayesite in Chile, as well as its mode of occurrence and formation, placed this question, I believe, beyond a doubt.

The mineral alluded to is found in suspension in the waters of the hot springs called the Baños del Toro situated in the Cordilleras of Coquimbo, and occurs in the form of beautiful snow-white silky or feathery flakes, which ultimately subside and form a light flaky sediment of this mineral in a perfectly pure state, as shown by a qualitative analysis made upon a portion of the same.

The process of its formation is at once most simple and interesting. The hot vapours, due, without doubt, to the proximity of the neighbouring volcanoes of this chain of mountains, here make their escape through channels or crevices which conduct them into these springs of water previously highly charged with carbonate of lime in solution, taken up by the water from the decomposing lime felspars of the porphyries and porphyritic tuffs (with intercalated limestones) and sandstones which here represent the upper oolitic period; as might be expected, the boracic acid gas contained in these vapours bubbling up through the calcareous waters combines directly with the lime, producing the silky flakes of borate of lime (Hayesine) thus seen in course of formation, whilst at the same time the liberated carbonic acid escapes into the atmosphere.

It is hardly possible to produce a more beautiful illustration of the actual formation of a mineral body. The Hayesine found as an incrustation in the Tuscan borax lagoons is, without doubt, the product of an analogous chemical action.

XIV. *On the Motion of Camphor towards the Light.* By CHARLES TOMLINSON, Lecturer on Physical Science, King's College School, London.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

DR. DRAPER'S letter, contained in your last Number, has surprised me not a little. On the strength of a question put in 1840, as to whether the side of the jar nearest the sun may not be the colder, he claims the merit of the theory advanced by me in your Number for November last.

Dr. Draper's question, put in 1840, was answered by himself in his quarto volume, 'On the Forces which produce the Organization of Plants,' published at New York in 1844. At page 124 of the Appendix he says, "It might be suggested that when a vessel is exposed to the sun, that part of the glass

which is nearest to him may actually be the *coldest*; such an opinion, it is evident, rests on no sufficient grounds." He then describes the experiment referred to by me in my paper, in which a differential thermometer, enclosed in a jar and exposed to the sun, proved that the side of the jar nearest to the sun was the warmer, and adds, "Hence we know that in all cases where crystals of camphor, dew of water, &c. are deposited on the side next the sun, they are so deposited in opposition to an energetic force which tends to remove them."

This is pretty strong evidence that Dr. Draper's theory is very different from mine.

Again, he says, p. 120, "The sun's rays have the power of causing vapours to pass to the perihelion side of vessels in which they are confined, but, as it would appear, not at all seasons of the year." He then describes a case in which, during December, January, and part of February, "a deposit was uniformly made towards the sun; during the months of March, April, and part of May next following, although every part of the arrangement remained to all appearance the same, yet the camphor was deposited on the side furthest from the sun. *It does not appear that any immediate cause can be assigned for this waywardness.*"

Could Dr. Draper have put forward such a statement as this in 1844, if he had had the slightest idea of the true theory of the motion of camphor towards the light?

Again, p. 122, when a circular plate of glass was put into a glass receiver above the camphor, no deposit was made on the plate. Dr. Draper says, "It was not without surprise I observed that, however long the plate was continued in the beams of light, no crystallization would ensue." Cases of this kind are perfectly explicable on my theory, and, indeed, I give several such.

Commenting on such a case, Dr. Draper says that "to reduction of temperature we cannot look for an explanation." I prove that reduction of temperature is the only means of explanation.

Again, at p. 122, Dr. Draper says, "Why does this condensation take place on the hottest surface, the side nearest to the plate? *We cannot admit that the rays of heat have any active part in bringing about the phenomenon.* On the other hand, they ought rather to exert a contrary effect, antagonizing the powers that solicit the camphor crystals to form, and driving them to the coldest surface. We are therefore reduced to the supposition that, when the light of the sun impinges on a surface of glass, it places that surface in such a condition that it exerts a pressure on the adjacent medium, immediately followed by a condensation of that medium."

I have no idea what this means; but I would ask how it is

possible Dr. Draper could sanction such statements in 1844, if, as he says, he had adopted my theory in 1840?

At page 124 he says that "light which has suffered reflexion at certain angles seems to have undergone a remarkable modification, being no longer able to put the glass into such a condition that it can cause motion towards the sun." Does this look like settled theory?

Nor is the action of metal screens and tinfoil rings in preventing a deposit at all explained. At page 126 it is stated that "the ring exerts a kind of protecting action," &c. Again, "This action of a ring formed of good conducting materials might be supposed to arise either from its adding something to the surface of the glass, or taking something away from the glass with which it is in contact; or it might be imputed to some change impressed on the ray of light," &c.

The electrical theory noticed in my paper is started at page 127. At page 128 it is stated that, if the inner surface of the glass receiver be rubbed with a glass rod, the camphor will deposit itself on the lines traced by the rod. Although the explanation of this fact is perfectly easy*, yet Dr. Draper compares the result with Lichtenberg's electrical figures, and proceeds to ask, "Are we to refer this singular action to the rays of light, to the rays of heat, or to the chemical rays?" He then proceeds to pass the light through solutions of ammonio-sulphate of copper, bichromate of potash, &c., and obtains what he calls "aphelion camphor deposits." He says, "It does not necessarily follow from the phenomena that any peculiar class of rays is emitted by the sun which bring about this action; but if there are such, it is a question of interest to find what is the reason that good conductors of electricity render their action nugatory."

Now is it conceivable that Dr. Draper could have published such statements as these in 1844 if he had had the faintest idea of the true theory? Or is it conceivable that so industrious and ingenious an experimentalist would not have devised experiments to test his theory had it been the same as mine, and which he now admits to be a sufficient one? I have looked in vain for any indication of such an attempt. Instead of any settled theory, I find a large number of theories, and a large number of experiments, but nothing is settled. In fact Dr. Draper makes a lottery of a number of theories, and draws out blanks so far as the explanation of the phenomena in question is concerned; and when, twenty years later, another inquirer draws a prize, he claims it as having come from his original lottery.

* The editorial note in the Philosophical Magazine for 1840, to which Dr. Draper refers, has reference to this fact.

If Dr. Draper advanced the true theory in 1840, it made no impression on himself or on others. No scientific writer, to my knowledge, either in America or Europe (and I have searched far and wide for the purpose), has ever had a doubt that the camphor deposits are produced by the action of light. Nor do I now see any cause to alter one of the conclusions of my paper, viz. that the result of Dr. Draper's labours was "to multiply phenomena, and to leave the theory as it was."

Apologizing for the length of this letter,

I remain, &c.,

C. TOMLINSON.

King's College, London,
January 5, 1863.

XV. *A Theory of the Zodiacal Light.*

By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

THE following theory of the zodiacal light is the same as that of which I gave an outline at the Meeting of the British Association held in October last at Cambridge. I propose now to produce it in more detail, and to exhibit the mathematical reasoning by which it is sustained. So far as the facts to be explained may be regarded as phenomena of light, reference will be made for their explanation to those motions of an elastic fluid medium which I have investigated in several recent communications to this Journal, and have especially applied in the Number for last December in the explanation of various properties and laws of light. That the zodiacal light, regarded merely as a phenomenon of light, comes under the class of phenomena explained in that communication, which are distinguished from those of reflexion, refraction, &c. by not being immediately related to visible and tangible substances, will be made to appear by reference to direct observations. There are other characteristics of it, such as its form and position, the proposed explanations of which depend on motions of the æthereal medium different in kind from those which account for the properties of light. The laws of these motions will be deduced by mathematical investigation. But it is necessary first of all to state distinctly the facts and appearances which it is proposed to explain.

In the northern hemisphere the zodiacal light is seen in the spring months in the west, after the departure of twilight, as a very faint light about 12 degrees broad at its base in the horizon, converging to an apex, and having its axis nearly coincident with the ecliptic. It is most conspicuous in the months of February and March, at which season the apex is near the Pleiades. Similar phenomena are visible before morning twilight in the east in the

* Communicated by the Author.

autumn months. It is evident that the light seen in the autumn lies generally in the same direction from the sun as the light seen in the spring; and as the earth is at these periods in opposite parts of its orbit, the appearances would be satisfied, so far as regards northern observations, by supposing the light to be of a conical or pyramidal form with the base near the sun, and not to be equally and similarly extended in the opposite direction from the sun. To ascertain whether this is really the case, I wrote, in default of available observations, to a friend resident in the interior of Brazil for information respecting the southern aspects of the phenomenon, and in a letter dated October 2, 1848, received the answer that he "had frequently remarked, particularly the last few days, about an hour *after sunset*, a strong light *in the west*, some 10 degrees broad, rising to the height of 50 degrees at least above the horizon, diminishing in breadth, but not converging to a definite point." This was evidently the zodiacal light stretching out from the sun just in the opposite direction to that in which it is seen in northern latitudes. From this observation it may be inferred that the light is symmetrically disposed with reference to the sun, and that it is not visible in northern latitudes in autumn evenings and spring mornings, solely because the small inclination of the ecliptic to the horizon at those epochs brings it into positions in which it must be viewed through dense parts of the atmosphere, which its light is too feeble to penetrate. This inference is placed beyond doubt by the valuable observations of Professor C. Piazzi Smyth made at the Cape of Good Hope in the years 1843, 1844, and 1845, and communicated in February 1848 to the Royal Society of Edinburgh (Transactions, vol. xx. p. 489). About the time of the solstices the inclination of the ecliptic to the horizon will be nearly the same in the morning and in the evening hours, and consequently the zodiacal light, if seen at all, should be seen both morning and evening at the June solstice in southern latitudes, and at the December solstice in northern latitudes. Professor Smyth has in fact recorded observations of the positions of the apices on June 5 and July 6, 1845, both in the morning and in the evening. The two apices are situated in opposite directions from the sun, and the line joining them and conceived to pass through the sun, will be at right angles to the line similarly connecting the apices seen in spring, or in autumn, in opposite hemispheres. Subsequently to my having knowledge of the observations above mentioned, I found from a list of observations of the zodiacal light collected by M. Houzeau, and published in the *Astronomische Nachrichten* (vol. xxi. col. 187), that Cassini had taken morning and evening observations of the apices on two days not far from the winter solstice, namely, on

January 6 and December 25, 1685. The line joining these apices will be approximately coincident with the line joining the apices observed at the Cape near the summer solstice.

The foregoing observations, taken together with all those collected by M. Houzeau, indicate that the zodiacal light is always visible before sunrise and after sunset, whenever the ecliptic makes a large angle with the horizon of the place of observation, and accord very well with the supposition that its form is that of a double convex lens having the sun at its centre, and its principal plane inclined at a small angle to the plane of the ecliptic. After making the hypothesis that the position of the apex in space is always on a straight line through the sun perpendicular to the earth's radius vector, M. Houzeau has found by the method of least squares applied to the fifty-eight positions in his list, that the longitude of the node of the principal plane is $1^{\circ} 59'$, and the inclination to the plane of the ecliptic $3^{\circ} 35'$. He has also compared the apparent inclinations to the ecliptic of the planes passing through the earth, the sun, and the observed positions of the apex, with the corresponding inclinations calculated from these elements on the above hypothesis. And having made the same comparison on the supposition that the principal plane coincides with the plane of the sun's equator, adopting $75^{\circ} 8'$ for the longitude of the ascending node, and $7^{\circ} 9'$ for the inclination, as determined by M. Laugier (*Comptes Rendus*, vol. xii. p. 649), he finds that the sum of the squares of the differences in the former comparison is to that in the other as 100 to 121. Notwithstanding that this result seems to be in favour of M. Houzeau's elements, it appears, on inspecting the respective differences between the calculated and observed apparent inclinations, that the *law* of the inclinations is not so well represented by these elements as by the elements of the sun's equator. This will become still more apparent after correcting the signs of the inclinations deduced from the observations taken in the evenings of December 25 and January 5, 1685, which by some mistake are *plus* instead of *minus*. It is also to be considered that the hypothesis on which the calculations rest is inexact except for positions of the earth very near the nodes, and that the tendency of the error, in the comparison with the elements of the sun's equator, is to augment the *plus* differences in August and September, and the *minus* differences in February and March. When these circumstances are taken into account, it will be found that the apparent inclinations corresponding to positions of the apex on the north side of the ecliptic in the evening and the south side in the morning, prevail generally from September to March, and the opposite inclinations during the rest of the year; which is just what would take place if the nodes of the zodiacal

light are coincident with the nodes of the sun's equator. In the same case the maximum apparent inclinations would occur at June 6 and December 6, which, as will presently be shown, is in sufficient accordance with the results of observation; while, according to M. Houzeau's elements, about seventeen days before those epochs the inclinations are at zero.

In order to test the above inferences, I have calculated the apparent inclinations from as many observations as I could collect of positions of the apex taken within moderate intervals from June 6 and December 6, and have compared them with the inclinations, deduced in the manner above stated, from the elements of the sun's equator. The observations used for this purpose are fourteen in M. Houzeau's list from October 26 to January 8 inclusive; two taken by Prof. Smyth on July 9, 1845 (those of June 5, which for some reason are wholly discordant, being omitted), and five taken by Capt. W. S. Jacob in 1862, on May 17 and 26, and June 11, 16, and 21. These last are contained in the 'Monthly Notices of the Royal Astronomical Society' (vol. xxiii. No. 2, p. 55), in a letter from the observer communicated by Prof. Smyth, who has added concluded right ascensions and declinations of the apex, which I have adopted. Of the four remaining observations by Capt. Jacob, those of July 16, 17, and 22 were thought to be too uncertain, and that of July 30 taken advisedly when Venus was behind a cloud, and giving a consistent result, was considered to be too distant from the epoch of June 6. The mean of the inclinations given by the twenty-one observations is $4^{\circ} 16'$, and the mean of those deduced from the elements of the sun's equator $6^{\circ} 29'$. The difference, $2^{\circ} 13'$, being subtracted from $7^{\circ} 9'$, the assumed inclination of the solar equator, the remainder, $4^{\circ} 56'$, may be regarded as the apparent inclination at the epoch of December 6, and, in fact, the maximum apparent inclination, very approximately deduced from the observations, if the nodes are coincident with those of the sun's equator. The apparent inclination at the same epoch by the other elements is $0^{\circ} 54'$, and the maximum apparent inclination, occurring at September 25, is $3^{\circ} 35'$. The above difference of $2^{\circ} 13'$ may be attributable partly to the rough and uncertain character of the observations, partly to the error of the hypothesis of the calculations, and partly to error in the determination of the inclination of the solar equator. I take the occasion to remark that the measures of the positions of solar spots by which this determination has been made, do not take account of any displacement of those positions which may be due to the passage of the rays by which the spots are seen through the sun's atmosphere.

Upon the whole, it may be concluded from the above discus-

sion that the coincidence of the principal plane of the zodiacal light with the plane of the sun's equator may be assumed as a probable deduction from observation, till the point be settled by additional observations of greater exactness than those hitherto made. For greater accuracy, the observations should be taken, if possible, by instrumental measurement, rather than by estimation with reference to planets and bright stars. Good observations of the direction of the axis of the light, made in the months of June and December, would most immediately tend to decide this question.

I have now to refer to certain observations of recent date which have a bearing on the *extent* of the zodiacal light. Vol. iv. of the 'Astronomical Journal,' edited by Dr. Gould, contains, in p. 94, a letter dated May 17, 1855, addressed to the editor by the Rev. George Jones, Chaplain on board the U.S. steam-frigate 'Mississippi,' in which he states that in a voyage chiefly in the China and Japan seas, and extending from 41° north latitude to 50° south latitude, he had excellent opportunities for observation of the zodiacal light. The observations are not given in detail, but the following important statement is made: "I was also fortunate enough to be twice near the latitude of $23^{\circ} 28'$ north when the sun was at the opposite solstice, in which position the observer has the ecliptic at midnight at right angles with his horizon, and bearing east and west. Whether the latter circumstance affected the result or not I cannot say; but I then had the extraordinary spectacle of the zodiacal light simultaneously at both east and west horizons from 11 to 1 o'clock for several nights in succession." From this observation, of the certainty of which there seems to be no reason to doubt, it must be concluded that *the zodiacal light extends beyond the radius of the earth's orbit*; for otherwise it would have been entirely below the horizon of the observer at midnight. Consequently the earth is either at all times enveloped by it, or, at least, when passing across the line of its nodes. This fact, which stands directly opposed to the idea which some have entertained that the zodiacal light originates in a vast number of minute bodies circulating about, and descending towards, the sun, necessitates the inference that it is simply *luminosity*. It will presently appear that the theory I am now about to propose conducts to the same conclusion.

The theory essentially rests on a law of the motion of fluids, elastic or incompressible, which may be called *the law of the coexistence of steady motions*. This law, I venture to predict will eventually be found to have the same important bearing on the theories of electricity, galvanism, and magnetism that the law of the coexistence of small vibrations has in the theory of

light. I have given a proof of it in art. 15 of "The Theory of Magnetic Force" contained in the Philosophical Magazine for February 1861; which, as it has probably attracted no attention, I will here repeat, with some difference in the course of the argument, and in somewhat more detail.

The known general hydrodynamical equation applicable to steady motion when no extraneous force acts, is

$$a^2 \cdot \text{Nap. log } \rho = C - \frac{V^2}{2}, \dots \dots \dots (1)$$

in which C is an absolute constant when the fluid is of unlimited extent, as will be the case in the application we are about to make of the equation. Consequently we obtain by differentia-
tion

$$\frac{d\rho}{\rho dx} = - \frac{V}{a^2} \cdot \frac{dV}{dx}.$$

Hence $\frac{u d\rho}{\rho dx}$, and, by parity of reasoning, $\frac{v d\rho}{\rho dy}$ and $\frac{w d\rho}{\rho dz}$ are each of the order of the *third* power with respect to the velocity. For cases of steady motion the general hydrodynamical equation which expresses constancy of mass, viz.

$$\frac{d\rho}{dt} + \frac{d \cdot \rho u}{dx} + \frac{d \cdot \rho v}{dy} + \frac{d \cdot \rho w}{dz} = 0,$$

becomes, to the *second* order of approximation,

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0, \dots \dots \dots (2)$$

because for that kind of motion $\frac{d\rho}{dt} = 0$, and, as just proved,

$\frac{u d\rho}{dx}$, &c. are each of the third order. Now if $u_1, v_1, w_1; u_2, v_2, w_2$, &c. be velocities due to different disturbances acting separately and causing steady motion, each set of values will separately satisfy the equation (2). And from the form of the equation it is evident that if $u = u_1 + u_2 + \&c.$, $v = v_1 + v_2 + \&c.$, $w = w_1 + w_2 + \&c.$, u, v, w will satisfy the same equation. That is, the resultant of the separate steady motions, supposing them to coexist, satisfies the equation. But since the component motions, being steady, are functions of coordinates only, the resultant motion must also be a function of coordinates only, and consequently be steady motion. Hence the resultant will satisfy the general equation (1) applicable to steady motion; and as it has thus been shown that it satisfies *both* equations, the law of the coexistence of steady motions is a necessary consequence.

The application of this hydrodynamical law to the problem of

the zodiacal light depends on the following hypotheses, which are the same that I have previously made for the explanation of facts of a very different kind. First, I assume the existence of the æther, that is, of a uniform elastic fluid pervading space and pressing always proportionally to its density. Secondly, the æther is assumed to be the medium of the generation and transmission of light, this assumption being abundantly justified by the explanations deducible from it of a great variety of phenomena of light, as is shown in the Number of the Philosophical Magazine for last December. Thirdly, the æther is supposed to permeate freely all visible and tangible substances, filling without change of its density all the space not occupied by the atoms of the substances.

It is an ascertained fact that the vast body of the sun revolves uniformly about an axis in twenty-five days nearly. Without inquiring into the particular disturbing effect of this movement on the æther, we may with certainty draw from the above hypotheses the following inferences, which are all that are necessary for my present purpose: (1) that as the disturbing cause is constant, the motion of the æther produced by it is steady; (2) that the motion decreases with increase of distance from the sun, and with increase of distance from a plane coincident with that of the sun's equator; (3) that the motion is symmetrical with respect to the sun as its centre, and is alike on the two sides of the above-mentioned plane. These inferences are true if the sun's centre have a fixed position in space. But there is good reason to conclude, from astronomical observation, that the sun and the whole solar system is progressing through space with a uniform rectilinear motion. To take account of the effect of this motion, we may conceive to be impressed on the æther an equal contrary motion. The sun will thus be reduced to rest, and we shall have the case of two coexistent steady motions having independent origins. According to the hydrodynamical law proved above, the steady motion caused by the sun's rotation will not be altered by this circumstance, and will consequently be the same when the sun is in motion as if it were at rest.

It is now important to explain in what manner the coexistence of these steady motions gives rise to the appearance of *light*. This is a theoretical consequence of the analytical circumstance that, in the investigation of the law of coexisting steady motions, terms of the *third* order were omitted. These terms correspond to parts of the motion which, so far as they are effective, produce disturbances of the æther superadded to the steady motions; and the disturbances, since they arise from the mutual action of the parts of the fluid, are productive of light-undulations, according to the mathematical principles of the undulatory theory of

light which I have so fully explained in the pages of this *Journal*. The space through which the disturbances extend will only be that which is occupied in common by the two steady motions, which, without doubt, will be of much larger dimensions than the space from which the light that is of sensible amount comes to the eye of a spectator. Evidently, however, the form, position, and density at different parts of this light will be just such as are exhibited by the zodiacal light. Its principal plane will coincide with the plane of the sun's equator; which condition, as we have seen, is at least very approximately fulfilled by the phenomenon; but, as before stated, the observations are not yet adequate to decide whether or not it is exactly fulfilled.

The facts and arguments that have now been adduced may suffice for giving a distinct idea of the proposed theory of the zodiacal light, and of the evidence on which it rests. It may be mentioned as a circumstance confirmatory of the truth of the theory, that it essentially depends on the reality of the movement of the solar system, which movement accounts also for the observed tendency of the proper motions of stars to mean directions diverging from a certain point of the heavens. This reference of widely different phenomena to a common cause is an instance of that "consilience," which is one of Bacon's tests of the truth of a theory. But not in this respect alone does the proposed theory receive such confirmation. A considerable time before I had concluded from observations that the form of the zodiacal light is that which has been described in this communication, I had proposed a theory of magnetic force, in which the cosmical magnetic influence resident in the sun, which has been so satisfactorily educed by General Sabine from the discussion of a large number of magnetic observations, was referred to those very gyratory motions of the æther about the sun, of which, according to the present theory, we have ocular evidence in the zodiacal light. The magnetic theory is expounded in two communications contained in the *Numbers of the Philosophical Magazine* for January and February 1861, and the particular applications alluded to are made in articles 34, 35, and 36. I take this opportunity of adverting to the theoretical explanation of General Sabine's "semiannual inequality" of the diurnal variation of the declination, which is given very imperfectly in art. 35, for the purpose of making an important addition to it, which will at the same time serve to exhibit in a very distinct manner the consilience of explanations just spoken of. As this inequality is, at the same local hours, very nearly of the same amount at places on the earth's surface widely different as to latitude, it would seem to depend only on the earth's heliocentric longitude. But since, according to the theory, no semi-

annual inequality independent of latitude would exist if the sun's equator coincided with the ecliptic, the inequality in question must be attributed to the inclination of the solar equator, in consequence of which the effect of the gyratory motions about the sun will be at a maximum each time the earth passes through the nodes, that is, as we have seen, about the times of the summer and winter solstices.

There is another phenomenon similar, and perhaps related to the zodiacal light, which is described by Mr. Jones in a second letter, dated from Quito, Nov. 18, 1856, and published in vol. v. of the 'Astronomical Journal' (p. 28). He says, "I see here every night, and all through the night, a luminous arch, from east to west, quite across the sky." The arch is also stated to be 20 degrees broad, and of uniform intensity throughout; and apparently it stretches along the ecliptic, although this is not expressly mentioned. The phenomenon described by Prof. Brorsen of Senftenberg, in Nos. 998 and 1166 of the *Astronomische Nachrichten*, appears to be identical with this. I have purposely abstained from any attempt to account for this luminosity, because it seems to require for its explanation considerations distinct from those which apply to the zodiacal light, although, like the latter, it is in some way related to the sun.

Cambridge, January 16, 1863.

XVI. *On the Perception of Relief.*

By Professor EDWIN EMERSON, of Troy University, U.S.*

PROFESSOR CIMA of Turin has sent us the description (says the editor of the *Cosmos*) of a stereoscopic experiment which is not without interest. He takes the picture of a front view of a human head, executed either in crayon, or lithograph, or copper-plate, and which is 3 or 4 centimetres in height; this he cuts into two parts along a line which coincides with the vertical axis of the nose; he takes one of these halves in each hand, and holding them in the same perpendicular plane, he brings them before the eyes at a distance which is less than that of distinct vision; he then allows the optic axes to converge, and thus causes the drawings to approach or recede until he is able to see two pictures of each half, and until the two middle ones overlap so that they make the impression of an entire countenance. "When one makes this experiment for the first time," says Professor Cima, "he will see with astonishment that the full face which is produced by the overlapping of the two halves makes, in a high degree, the impression of a solid body; the half tones melt and mix together

* From Silliman's American Journal for November 1862.

as in a modelled figure; the nose rises well from the face; the eyebrows, lips, and chin stand out very well; and the entire figure raises itself from the ground upon which it is drawn, and assumes in a remarkable degree a living expression. The necessary distance of the two half-pictures from each other, and also the proper distance from the eyes of the observer for the production of the greatest effect, can only be ascertained by trial. The more steadily one gazes at the pictures, the more the sensation of relief is strengthened”*.

The foregoing extract from the *Cosmos* has been reproduced in Poggenдорff's *Annalen*, vol. cii. p. 319; in *Il Nuovo Cimento*, vol. vi. p. 185; in *Die Theorie des Sehens und räumlichen Vorstellen*, bei Dr. Cornelius, Halle, 1861; and in *Monographie du Stéréoscope*, par Blanchère, Paris, 1862. Seemingly endorsed by such a high authority as Moigno, the alleged fact passes through scientific treatises unquestioned, and is now apparently regarded as established. We consider it, therefore, important to refute the conclusions involved in the experiment as described by Professor Cima, and at the same time point out some analogous mistakes as to the perception of relief.

When the experiment of Professor Cima is carefully performed and analysed, it will be found that the right eye sees the right half of the middle picture, and the left eye the left half. Now, as these two dissimilar masses are not superposed upon each other, as is the case with the dissimilar complementary figures in ordinary vision, but are merely joined together at the line passing through the centre of the resultant picture, it is evident that, if such an effect is realized as that “the nose rises well from the face,” or that there is any “sensation of relief,” we have here an experiment which refutes the established theory of binocular vision, and leaves the effects of the stereoscope without any adequate explanation.

The fact is, however, that in Professor Cima's experiment there is no real perception of relief. All that is really seen is the perspective, which is mistaken for relief or solidity. To prove this, let the observer, while looking at the two half-pictures in the mode alleged to produce the effect of solidity, close one eye, the right for instance; the right half of the picture disappears, but the left retains exactly the same appearance it had before; it loses no appearance of solidity, simply because it had none. Or let the observer join the two halves together, and closely and continuously observe them with one eye; the effect will be the same as in Professor Cima's experiment. Or, to vary the test, take a *single* photographic picture, for instance the right-hand side of a stereograph, cut it in two by a vertical line

* *Cosmos*, vol. ii. p. 353.

through the centre, and place the halves the proper distance apart in a stereoscope so as to unite them readily into a whole ; the same effect, claimed by Professor Cima to be a sensation of relief, will be observed : that it is not relief will be most manifest by comparing it with a stereograph of the same scene.

But the reader will very naturally inquire, How did Professor Cima, and those who have unquestioningly quoted his experiment, fall into this error with regard to the presence of relief? This reasonable question we will endeavour now to answer.

The ability to perceive relief, or solidity, is a natural one. To those who have the proper use of their eyes and can walk, it is an intuitive faculty ; we cannot help seeing solidity, where it exists, if we try, no more than we can help hearing sounds or seeing colours. The common idea that this faculty is the *result* of experience, and is therefore acquired, is opposed by the whole analogy of our being. The infant does not learn to hear ; it hears, it hears intuitively if it is a perfect child, but learns as it grows to know what it hears ; it feels a blow, but may be too young and feeble to know what that blow is. So it has but to open its eyes and the scene enters, it is painted properly and instantaneously upon the retina ; but it may require a long education before the child will have an intelligent idea of what it sees : indeed it may go through life and never be able to give more than *one* name to a great variety of very different colours, such as red, vermilion, scarlet, orange, and crimson. It is unphilosophical to confound a faculty with its use. We have the natural faculty of seeing solidity ; but the acuteness with which it is employed depends greatly upon the intelligent attention with which it is exercised.

It is no answer to this to say that we can analyse the optical conditions upon which the perception of relief depends. This has been splendidly done by Wheatstone, Dove, and others, and is beautifully illustrated by the stereoscope ; but this has no necessary connexion with the question before us. When I say we hear intuitively, it is nothing, in the way of refutation, to explain to me the acoustic conditions upon which hearing depends, or to assert that Mozart had no intuitive perception of melody or harmony because the laws are fixed by which a melody ought to proceed, and harmony, to be such, must be according to the formula of thorough bass, whereas the child Mozart could not know all this. So with the matter in question ; all men see solidity who have the proper use of their eyes : very few indeed know how it is effected, or are able to distinguish acutely between the perception of binocular relief and the perception of mere perspective, or the appearance of distance without relief.

The perception of *relief* depends upon the angle formed by the rays which proceed from any object of sight to the right and

left eyes respectively; the larger this angle, the more relief is apparent, provided the eyes can unite the dissimilar images; but when by reason of distance this angle becomes nothing practically, and the rays are parallel as they enter the eyes, relief vanishes.

The perception of the *perspective* depends upon very different conditions, such as the direction of the lines that compose a view, the light and shade, the apparent size, the tint, &c.

When we consider the matter, it is not surprising that these two modes of perception should often be confounded. True relief diminishes so gradually, and melts so gently away, leaving perspective entirely master of the field, that the essential difference between them is likely to be lost sight of. That this is the case may be shown by the following examples:—

It requires a series of very careful experiments to determine how far, under ordinary conditions, we can perceive relief. Experiments of my own lead me to believe that the distance is under three hundred yards. The only reason a good painting, whose foreground is represented as it appears at the distance of two or three hundred yards, is not a complete illusion when seen under favourable conditions, is that we can change our point of view; and motion to one side or the other will impart the idea of relief in nature, but as there is no relief, properly so called, in a painting, as soon as we shift the point of view, we detect this, and the illusion is at an end. Hence paintings ought to be observed by one eye, and from one point of view, to obtain the maximum effect. Hence, also, stereographs of scenes which lie at a distance of over three hundred yards from the observer will give no stereoscopic effect, will not *give the impression* we are able to get with our eyes, assisted by our capacity to move from one point of view to another; they ought therefore to be photographed from stations more or less distant from each other, but always exceeding considerably the distance between the eyes.

Persons *not accustomed* to experimenting with the stereoscope cannot distinguish readily between stereoscopic and pseudoscopic effect: they are also constantly imposed upon by views which have no stereoscopic effect whatever. I have repeatedly mounted two identical or right-eye views of the same scene, side by side, as though they were right- and left-eye views, and have never failed to get the verdict that they exhibited stereoscopic effect; which was impossible, of course. Not only are ordinary observers thus mistaken, but they constantly manifest an opposite peculiarity, being unable to see the greatest relief when it is exhibited in an unusual manner. In *Das Stereoscop*, C. G. Ruete, Leipsig, 1860, Dove's illustration of this point is republished in

such a way as to destroy the object in view, showing that his commentator had not a fine perception of relief.

A remarkable instance of the uncertainty attending the perception or non-perception of stereoscopic relief, even in cases where we might suppose there could be no want of knowledge, is shown by the controversy now going on in Europe over the *Chimenti pictures*. Sir David Brewster thinks he has in these pictures a specimen of real stereoscopic drawings produced about the middle of the 17th century; and this opinion is endorsed by Prof. Tait, Prof. Macdonald, and others in decided terms. I have made a careful examination of the photographs of these pictures; and the truth is that the trifling stereoscopic and pseudoscopic qualities about them are evidently accidental. To prove this, let anyone execute a pen-and-ink sketch, and then let him make as perfect a copy of it as he can without careful measurements: now place these two drawings in the stereoscope, and you get the same kind of effect seen in the Chimenti drawings, and for the same reason: the drawings will vary more or less from each other; all that is necessary, then, to impose upon ordinary eyes is to find out which way the sum of the variations preponderates; mount the drawings accordingly, and, *mirabile dictu!* you have produced a stereoscopic picture (the pseudoscopic portion being overlooked) drawn by hand; you have done that very thing that Sir David Brewster has repeatedly declared was quite beyond human skill! If Prof. Wheatstone gets no heavier blow than this, his fame as a discoverer is secure.

As a further confirmation of our views, we may point to the fact that but few persons can properly locate the optical position of reflexions from curved surfaces, and in particular the images from concave surfaces.

During the last year or two, large assemblages have been drawn together in our principal cities to see with delight the effects produced by what is called the *Stereopticon*, which is merely another name for a magic lantern of good quality, with *one* side of a glass stereograph for a slide. Nearly all in these large assemblages have agreed in believing that they saw, what they were told they saw, excellent stereoscopic effect in the single picture which alone is exhibited. The truth is they made the popular mistake; they saw nothing but perspective.

Stereoscopic effect on a large scale may be obtained by exhibiting the right and left pictures of a glass view side by side by the magic lantern, and then uniting the magnified pictures by means of prisms. This I have recently demonstrated by experiment. The idea was also suggested some years ago by Dr Wolcott Gibbs to Mr. Pike of New York, but not put to the test.

We conclude, then, from the foregoing—

1. That Professor Cima's experiment is only another instance showing how easily we can mistake one thing for another, and induce others to do the same.

2. That intuitive perception of relief may be indefinitely increased in degree by exercise—showing that this sense follows the same law under which we employ our other faculties.

XVII. *New Method of determining the Thermal Conductibility of Bodies.* By A. J. ÅNGSTRÖM*.

TO the properties of matter which have been the subject of continuous investigation, the thermal conductibility of the metals undoubtedly belongs; but our knowledge of this important element is by no means so accurate or complete as we are entitled to expect, and the following contribution may therefore not be uninteresting.

The methods hitherto used for determining the conducting power are especially two. It has either been attempted, starting from the formula

$$k \frac{u-u'}{\Delta x} = Q, \quad (1)$$

to determine the heat which traverses a metal screen of the thickness Δx , when its two surfaces have the temperatures u and u' ; or the distribution of heat has been observed in a metal bar of constant temperature, the differential formula

$$\frac{d^2u}{dx^2} - \frac{hp}{kw} u = 0, \quad (2)$$

first proposed by Biot, being taken as a basis; in which case, as well as in what follows, u indicates the temperature of a given point of the bar, h the radiating power of the surface, k the conducting power, p the perimeter of the bar, and w its section.

The first method appears to promise no great accuracy, and, from a theoretical point of view, is not unobjectionable. For if both surfaces are kept at a given temperature by contact with steam or water, the conducting power of the metal screen, or, more correctly, the value of Q , is modified to such an extent that, as Péclet has found, the difference between various metals quite disappears in comparison with the small conductibility which water possesses. Péclet has endeavoured to obviate this error by renewing, by means of a special apparatus, the layer of water in contact with both surfaces as often as 1600 times in a minute. In this way this source of error must doubtless have been lessened, although it cannot be said to have been entirely destroyed.

* Translated from Poggendorff's *Annalen*, vol. cxiv. p. 513.

Besides which it appears to me that a rotating apparatus which rubs with such velocity will itself produce heat, and hence complicate or extinguish the phenomenon which is to be investigated. The results, also, to which various experimenters have attained by the formula (1) by no means agree.

If as unity the quantity of heat is taken which heats 1 kilogramme of water 1° C., there passes in a second through a copper disc of 1 square metre superficies and 1 millim. in thickness, and 1° difference in temperature between the two surfaces,

According to Clement	0·231
„ Thomas and Laurent	1·22
„ Pécelet with (friction of the surfaces)	19·11

The last value, which so considerably exceeds the first two, is yet, as is clear from what follows, still considerably too small.

According to the latter method, which depends upon the application of the formula (2), the process has been as follows: bars of the substance to be investigated have been procured and have been heated at one end until the temperature had become stationary, and the temperature of the bar investigated at different parts, either by thermometers, or by the contact of a thermo-electric element. This method gives greater accuracy than the foregoing, but labours under the defect that it does not give the value of k separately, but only the ratio between h and k , by which means the value of the latter magnitude is expressed in a function, namely, that of the radiation from the surface, which is not known. Hence it is that the value of h is variable, and depends not only on the difference from the temperature of the space, but also on the absolute temperature of the bar, as Dulong and Petit's investigations on the law of cooling have shown; it is thus evident that in this way only relative values of the conducting power of the various bodies can be obtained—and only this under the supposition that the bars retain the same surface, and the observations be made between the same limits of temperature. Principally by taking these circumstances into account, have Wiedemann and Franz obtained concordant results in their valuable investigation.

Besides the above methods, others have been used which may be called mixed, like that of Tyndall for the conducting power of different kinds of wood, or that of Calvert and Johnson for metallic alloys. For these experiments short bars of the substances in question were used. They were heated at one end, and the heat observed which during a given time they imparted to a mass of mercury or water surrounding the other. Since in this case, as has been already remarked in reference to the first method, the specific heat of the bars and the conductivity from

the lateral faces must influence the results obtained, they could not stand in a simple ratio to the conducting power.

§ 2.

From what has been said, it is clear that a method for the determination of the value of k is needed by which this shall be expressed in known magnitudes, or at any rate in such as are more easily determined than the radiation from the surface. I think I have found such a method by using a general formula for the propagation of heat in a bar of parallelepipedal form, that is,

$$\frac{du}{dt} = K \frac{d^2u}{dx^2} - Hu, \quad (3)$$

where

$$K = \frac{k}{c\delta} \text{ and } H = \frac{hp}{c\delta w},$$

and c denotes the specific heat of bar, and δ its density.

If a metal bar be taken so long that, in determining the law of the propagation of heat in it, regard need not be had to its terminal faces, and if it is *heated or cooled during fixed periods*, its periodical changes of temperature must be transmitted along the entire bar; and hence, in consequence of radiation from the surface, not only will the amplitudes diminish, but the maxima and minima will occur later at a greater distance from the points of heating. If we suppose these periodical heatings and coolings sufficiently long continued, so that the periods can develop themselves completely, in which case the mean temperature of a given point of the bar acquires a constant value, equation (3) is satisfied by the assumption

$$\begin{aligned} u = & me^{-\sqrt{\frac{H}{K}}x} + ae^{-gx} \sin \left(\frac{2\pi t}{T} - g'x + \beta \right) \\ & + be^{-g\sqrt{2x}} \sin \left(\frac{4\pi t}{T} - g'\sqrt{2x} + \beta' \right) \\ & + ce^{-g\sqrt{3x}} \sin \left(\frac{6\pi t}{T} - g'\sqrt{3x} + \beta'' \right), \quad . \quad (4) \end{aligned}$$

in which

$$\begin{aligned} g &= \sqrt{\sqrt{\frac{\pi^2}{K^2T^2} + \frac{H^2}{4K^2}} + \frac{H}{2K}}, \\ g' &= \sqrt{\sqrt{\frac{\pi^2}{K^2T^2} + \frac{H^2}{4K^2}} - \frac{H}{2K}}, \end{aligned}$$

and T denotes the length of the period.

To show the application of this formula (4), let $T=24'$, and assume that the bar is heated during half this time, and cooled during the other half, and this process continued so long that the changes become regular. If now for *each minute* during one or more of these periods the temperature of the bar at a given point is observed, for which it can be assumed that $x=0$, these observations, calculated according to the method of least squares, must be expressed by the following formula:—

$$u_n = m_1 + A_1 \sin(15^\circ n + \beta) + B_1 \sin(30^\circ n + \beta') + C_1 \sin(45^\circ n + \beta'') + \dots \quad (5)$$

For an analogous point of the bar, corresponding to $x=1$, an entirely analogous formula is obtained:—

$$u_n = m_2 + A_2 \sin(15^\circ n + \beta_1) + B_2 \sin(30^\circ n + \beta'_1) + C_2 \sin(45^\circ n + \beta''_1) + \dots \quad (6)$$

The constants $m_2, A_2, \beta_1, \&c.$ have other values than in the formula (5), but stand to the constants of these formulæ in a definite ratio expressed by the formula (4). Hence we have

$$\frac{A_1}{A_2} = c^{\delta l} = f \text{ and } \beta - \beta' = g'l;$$

and if we make $gl = \alpha$, and $g'l = \alpha'$,

$$\alpha\alpha' = gg'l^2 = \sqrt{\sqrt{\frac{\pi^2}{K^2 T^2} + \frac{H^2}{4K^2} + \frac{H^2}{2K}}} \times \sqrt{\sqrt{\frac{\pi^2}{K^2 T^2} + \frac{H^2}{4K^2} - \frac{H^2}{2K}}} l^2;$$

that is,

$$\alpha\alpha' = \frac{\pi l^2}{KT}, \quad (7)$$

a result remarkable for its simplicity. If in formula (7) the value of the magnitude K is substituted, we finally obtain the conducting power

$$k = c\delta \cdot \frac{\pi l^2}{\alpha\alpha' T}. \quad (8)$$

It is then seen that H entirely disappears from the expression for $\alpha\alpha'$, so that the value of k is obtained expressed in the specific heat of the body, reduced to the unit of volume, and independent of the numerous changes to which the radiating power is subject.

As now the specific heat is one of the elements most accurately known, and which may be determined with the greatest accuracy, there is a possibility of obtaining the absolute value of k .

The coefficients $B_1, B_2, \beta_1, \beta_2, \&c.$ may be treated in the same

way to obtain the value of k ; but the value which is obtained from them must, from readily explicable causes, be less reliable.

By altering the length of the periods, the accuracy of the values obtained for k may be controlled; k might also be obtained if α were known for two different periods, T_1 and T_n , without needing the α' .

For if T_n is made $= nT_1$, the two equations are obtained,

$$\alpha_1^4 - \alpha_1'^2 l^2 \frac{H}{K} = \frac{\pi^2 l^4}{K^2 T_1^2}$$

$$\alpha_{II}^4 - \alpha_{II}'^2 l^2 \frac{H}{K} = \frac{\pi^2 l^4}{K^2 T_{II}^2}$$

which, if both the members which contain H are eliminated, give

$$k = c\delta \frac{\pi l^2}{T_1 \alpha_1 \alpha_1'} \sqrt{\frac{n^2 \alpha_{II}^2 - \alpha_1^2}{n^2 (\alpha_1'^2 - \alpha_{II}'^2)}}; \quad \dots \quad (9)$$

yet this formula for the determination of k is much less advantageous than the preceding one (8).

§ 3.

The utility of a method is best seen in its application. I therefore give a short account of some experiments which were made on the conductivity in copper and iron.

To determine the temperature, I preferred to use thermometers, but of very small dimensions, sunk in the bar itself. To determine the temperature of the bar on the surface itself by a thermo-electric pile, as Langberg and subsequently Wiedemann and Franz have done, can scarcely be applied in any other cases than those in which the bars are very thin, which, however, is not advantageous for the method.

Besides, in the passage of heat from the bar to the thermo-electric element irregularities arise in the transmission of heat, which are quite comparable to those which might result from the cavities in the bar.

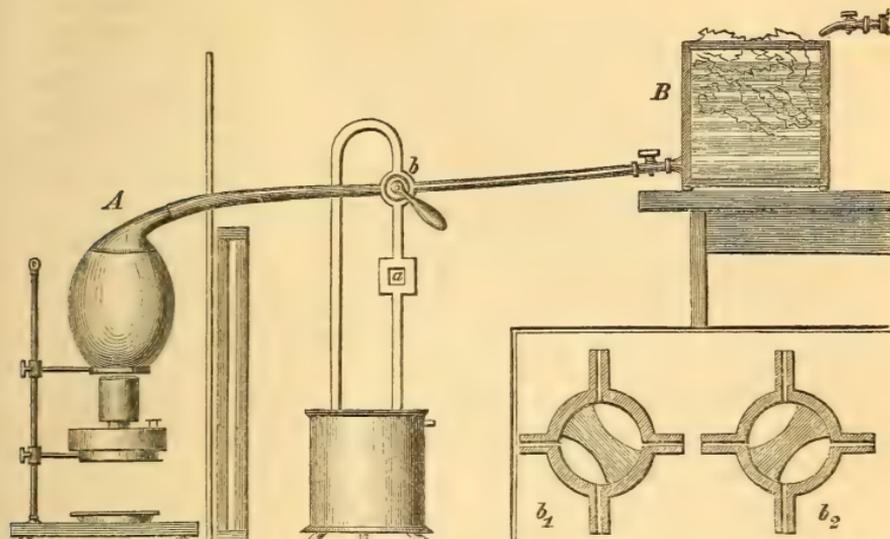
The thermometers had cylindrical reservoirs 1.5 to 2.0 millims. in diameter, and 15 millims. in length; they were provided with arbitrary scales, and were read off by means of telescopes.

The thickness ($\frac{1}{4} p$) of the bars was 23.75 millims., and the cavities, at a distance of 50 millims. from each other, were 2.25 millims. in diameter. As these bars were originally intended for an altogether different investigation, they could be screwed together in a special apparatus; and thus the two united copper bars were 570 millims. in length.

The heating and cooling of the bar could be effected by alternately surrounding it with aqueous vapour from the boiler A, or with cold water from the vessel B*; this was effected by

* In observing the iron bar, the cooling was not effected by the use of cold water, but simply by radiation.

turning the cock b , which in the position b_1 allowed steam, and in the position b_2 cold water, to reach the bar, the section of which is indicated in the figure by a .



By a special investigation the following relations were obtained between the divisions of the thermometers used :—

$$\left. \begin{aligned} \log n_1^\circ &= \log n_4^\circ - 0.18253 \\ \log n_1^\circ &= \log n_2^\circ + 0.02120 \\ \log n_2^\circ &= \log n_4^\circ - 0.20373 \\ \log n_3^\circ &= \log n_2^\circ - 0.03685 \\ \log n_3^\circ &= \log n_1^\circ - 0.01565 \end{aligned} \right\} \dots \dots \dots (10)$$

in which n_1° denotes the n° of the thermometer No. 1, n_4° the n° of the thermometer No. 4, and so on.

The absolute temperature of the bar needs only be known in so far as it is necessary to calculate the mean temperature, that is, the temperature to which the final value of k refers; otherwise it is enough to know the relative values of the degrees of the thermometer, and even this is not necessary if in the observation the places of the thermometers are changed.

By a similar change another source of error is avoided. For as glass is a bad conductor, it may be foreseen that the thermometers do not instantaneously and completely indicate the temperature of the bar at a given moment; it may, however, be assumed that these small deviations will occur in an entirely analogous manner when both thermometers are simultaneously observed, and that if, in consequence of the unequal form, mass, &c. of the thermometer-bulbs, a difference occurs, this can be eliminated by the exchange just mentioned. A third source of

error, depending on the so-called personal equation in different observers, can of course be removed from the results in a similar manner.

§ 4.

After these observations I give in Tables I. and II. the observations, or rather the mean values calculated from them. Each observation is the mean of observations during 2 to 5 periods, in which the readings of the thermometers made before the periods were complete and had assumed a constant character were always excluded from the calculation of the mean*.

Although in general the coefficients of the members which contain the double and threefold angle are so small that there can be no hope of obtaining from them a reliable value of k , they yet furnish an interesting confirmation of the theory, and hence deserve to be adduced.

If the respective coefficients in No. 1 and No. 2 are divided by one another, the quotients obtained multiplied, and the square root extracted, we get

$$\sqrt{\frac{31.745 \cdot 25.203}{13.010 \cdot 23.885}} = 1.6046 = f,$$

$$\sqrt{\frac{4.587 \cdot 2.186}{1.591 \cdot 1.665}} = 1.9425 = f'; \quad \sqrt{\frac{3.717 \cdot 4.334}{1.187 \cdot 2.969}} = 2.0654 = f'',$$

which values are quite independent of the values of the scales of the thermometers used.

If, further, the respective angular measures are subtracted from one another, we get

$\Delta\beta.$	$\Delta\beta'.$	$\Delta\beta''.$
$25^{\circ} \quad 3.5$	$37^{\circ} \quad 16.1$	$42^{\circ} \quad 25'$
$24 \quad 34.0$	$36 \quad 1.0$	$41 \quad 44$
<hr style="width: 50%; margin: 0 auto;"/>	<hr style="width: 50%; margin: 0 auto;"/>	<hr style="width: 50%; margin: 0 auto;"/>
$24 \quad 48.7$	$36 \quad 38.5$	$42 \quad 5$

Meanwhile from formula (4) we have

$$f = f' \sqrt{\frac{1}{2}} = f'' \sqrt{\frac{1}{3}},$$

$$\Delta\beta = \sqrt{\frac{1}{2}} \cdot \Delta\beta' = \sqrt{\frac{1}{3}} \cdot \Delta\beta'';$$

and if the values of $f, f', \Delta\beta', \Delta\beta''$ are introduced,

$$f = 1.6046, \quad f' \sqrt{\frac{1}{2}} = 1.5994, \quad f'' \sqrt{\frac{1}{3}} = 1.5201,$$

$\Delta\beta = 24^{\circ} 48.7, \quad \sqrt{\frac{1}{2}} \cdot \Delta\beta' = 25^{\circ} 55', \quad \sqrt{\frac{1}{3}} \Delta\beta'' = 24^{\circ} 19,$
an agreement as close as can be wished.

* M. Thalén had the goodness to help me in these observations; his name is indicated by Th in the Tables.

TABLE I.
Length of the Periods 24 minutes to 12 minutes.
Copper Bar.

No.	Therm.	1 m. to 13 m.	2 m. to 14 m.	3 m. to 15 m.	4 m. to 16 m.	5 m. to 17 m.	6 m. to 18 m.	7 m. to 19 m.	8 m. to 20 m.	9 m. to 21 m.	10 m. to 22 m.	11 m. to 23 m.	12 m. to 24 m.	Observers.	Distance between thermometers. millims.	
1.	.VI.	107.50	102.62	93.55	82.91	72.27	63.30	56.83	52.89	50.13	48.00	46.73	45.53	A.	100	
		50.57	68.78	80.22	87.62	93.05	97.02	100.09	102.54	104.50	106.19	107.54	108.85			
	I.	I.	100.96	98.88	91.87	84.07	78.80	75.56	Th.	100
			73.51	81.31	88.67	93.53	96.86	99.25		
2.	IV.	98.45	88.84	77.47	68.28	60.85	57.12	54.55	52.90	51.78	50.95	50.32	49.82	A.	100	
		54.17	68.35	76.95	82.48	86.49	89.40	91.24	93.38	94.88	96.11	96.89	98.04			
	I.	I.	107.36	98.85	81.68	70.42	63.70	59.76	Th.	100
			58.05	72.28	85.39	93.88	99.91	103.89		
3 (a).	I.	68.20	61.82	57.27	54.42	52.47	51.30	50.40	49.72	49.20	48.82	48.50	48.15	A.	100	
		50.75	56.90	60.12	62.15	63.62	64.80	65.90	67.00	67.65	68.32	69.10	69.75			
	II.	II.	69.80	65.87	61.75	59.40	57.92	56.92	Th.	100
			56.30	60.50	63.62	65.75	67.35	68.70		
4.	IV.	69.80	65.87	61.75	59.40	57.92	56.92	Th.	150	
		56.30	60.50	63.62	65.75	67.35	68.70			
	I.	I.	93.68	86.90	76.38	67.10	60.98	57.55	60.30	72.87	80.92	85.95	89.52	92.10	Th.	150
			100.85	101.07	95.73	88.03	80.32	74.45	70.35	74.08	81.07	87.18	92.28	90.22		

TABLE II.—Length of the Period 16 minutes.
Copper Bar.

No.	Therm.	1 m. to 9 m.	2 m. to 10 m.	3 m. to 11 m.	4 m. to 12 m.	5 m. to 13 m.	6 m. to 14 m.	7 m. to 15 m.	8 m. to 16 m.	Observers.	Distance between thermometers.
5.	I.	{ 92.40	86.18	75.28	66.63	61.57	57.97	55.62	53.98	} Th. { Heating Cooling	} {
		{ 57.03	68.07	76.13	81.12	84.82	87.60	89.67	91.33		
	II.	{ 83.90	84.73	83.57	80.53	77.18	74.22	74.22	71.80	} Th. { Heating Cooling	} {
		{ 68.27	68.65	71.07	73.80	76.45	78.78	80.63	82.33		
6.	II.	{ 79.51	83.59	86.37	88.39	89.94	91.26	92.32	93.31	} Th. { Heating Cooling	} {
		{ 94.06	93.89	89.97	86.55	84.02	82.08	80.47	79.17		
	I.	{ 86.46	87.05	88.50	89.90	91.20	92.31	92.31	93.35	} Th. { Heating Cooling	} {
		{ 95.04	95.59	95.09	93.45	91.66	90.01	88.64	87.48		

Iron Bar.

7.	I.	{ 75.42	74.23	72.46	70.95	69.72	68.81	67.96	67.35	} Th. { Heating Cooling	} {
		{ 66.98	67.35	68.22	69.37	70.62	71.89	73.08	74.24		
II.		{ 77.57	77.97	77.75	77.13	76.51	75.89	75.28	74.79	} Th. { Heating Cooling	} {
		{ 74.58	74.21	74.17	74.43	74.88	75.41	76.04	76.71		
I.		{ 74.30	73.25	71.26	69.68	68.39	67.43	66.61	65.96	} Th. { Heating Cooling	} {
		{ 65.52	65.82	66.83	68.07	69.44	70.69	71.94	73.07		
II.		{ 76.25	76.73	76.55	75.97	75.28	74.62	74.05	73.53	} Th. { Heating Cooling	} {
		{ 73.12	72.85	72.81	73.05	73.55	74.11	74.82	75.49		

If the numerical values obtained in No. 1 and No. 2 are calculated according to the method of least squares, the following trigonometrical series are obtained:—

$$\begin{aligned} \text{No. 1. } & \left\{ \begin{array}{l} \text{Th. (IV).} \dots \dots \dots t^n = 80.39 + 31.745 \sin(15. n + 134.6.2) + 4.578 \sin(30. n + 14.31.8) + 3.717 \sin(45. n + 104.33) + \&c. \\ \text{Th. (I).} \dots \dots \dots t^n = 88.86 + 13.010 \sin(15. n + 109.2.7) + 1.591 \sin(30. n + 337.15.7) + 1.187 \sin(45. n + 61.58) + \&c. \\ \text{Th. (I).} \dots \dots \dots t^n = 74.57 + 25.203 \sin(15. n + 142.21.2) + 2.186 \sin(30. n + 54.28.7) + 4.334 \sin(45. n + 112.25.3) + \&c. \\ \text{Th. (IV).} \dots \dots \dots t^n = 82.93 + 23.885 \sin(15. n + 117.47.2) + 1.665 \sin(30. n + 18.27.3) + 2.969 \sin(45. n + 70.41) + \&c. \end{array} \right. \end{aligned}$$

From the values of f and $\Delta\beta$ those of α and α' are readily obtained according to the formulæ

$$f = e^\alpha \text{ and } \Delta\beta \frac{2\pi}{300} = \alpha';$$

and these values substituted in (8), in which case in the preceding example $T=24$ and $l=10$, gives finally

$$k = c. \delta. 64.0 \text{ at } 50^\circ \text{ C.}$$

§ 5.

After having exemplified in this manner, not only the agreement of theory with practice, but also the manner in which the value of k is obtained, I collate in the following Table III. all the values of m , A , and β calculated from Table I. and Table II., as these are the only values which require to be known in the calculation of k .

TABLE III.

No.	Thermo- meter. No.	m .	A .	β .	$\Delta\beta$.	Length of Period.	Distance between Thermo- meters.
1	{ IV.	80.39	31.747	134° 6.2	25 3.5	24 <i>m. t</i>	100
	{ I.	88.89	33.010	109 2.7			
2	{ I.	74.57	25.203	142 21.2	24 34.0	24 <i>m. t</i>	100
	{ IV.	82.93	23.885	117 47.2			
3 (a)	{ I.	58.60	10.710	146 5.1	25 2.4	24 * . <i>t</i>	100
	{ II.	62.82	6.251	121 2.7			
3 (b)	{ IV.	76.36	3.559	111 8.3	24 54.4	24 * . <i>t</i>	100
4	{ I.	77.02	17.467	123 26.5	37 22.2	12 <i>m. t</i>	150
	{ IV.	86.80	14.345	86 4.3			
5	{ I.	75.38	18.780	129 51.5	46 1.0	16 * . <i>t</i>	150
	{ II.	76.61	7.728	83 50.5			
6	{ II.	87.18	6.999	294 42.3	30 58.1	16 * . <i>t</i>	100
	{ I.	91.32	4.290	263 44.2			
7	{ I.	70.42	3.713	275 37.4	36 28.1	16 <i>m. t</i>	50
	{ II.	75.83	1.770	239 9.3			
8	{ I.	69.24	3.891	275 41.6	37 9.4	16 <i>m. t</i>	50
	{ II.	74.56	1.852	238 32.2			

If k be calculated from the values of A and $\Delta\beta$, collated in the foregoing Table, in which formula (10) is used to find f , the following results are obtained if the mean temperature of the bar is expressed on the Centigrade scale, and the centimetre is taken as the unit of length.

	Number of observation.	Length of period.	Mean temperature.	$\frac{k}{c\delta}$.
Copper	3a	24 *.t	67.9	62.07
	3b	24 *.t	62.9	64.00
	1	24 m.t	50.0	63.44
	2	24 m.t	49.9	64.41
	5	16 *.t	49.0	65.81
	4	12 m.t	46.5	64.97
	6	16 *.t	33.0	67.92
			51.3	64.66
Iron	7	16 m.t	52.5	11.14
	8	16 m.t	54.1	10.92
			53.3	11.03

If the value of $c\delta = 0.84476$ is taken for copper,

„ $= 0.88620$ is taken for iron,

and these values are substituted, we have finally, k ,

For copper 54.62

For iron 9.77

at a temperature of 50° in round numbers.

If therefore we suppose a metal screen of copper or iron a centimetre thick at a mean temperature of 51° to 52° C., whose faces differ, however, in temperature by 1° , there passes in each second of time through each square centimetre of surface as much heat as is necessary to raise a gramme of water

through 54.62 C. if the screen is of copper,

and 9.77 C. if it is of iron.

To control the accuracy of the values thus found for the conducting-power of copper and iron, I have also determined the *relative* conducting-power of the bars, and thus obtained two series. Distance of the apertures 50 millimetres. The temperature of the room taken as starting-point.

Copper.		Iron.	
25.18		38.27	2.0423
23.48	2.0051	31.20	2.0424
21.90	2.0114	25.45	2.0452
20.57	2.0019	20.78	2.0370
19.28	2.0109	17.05	2.0417
18.20	2.0073	13.85	

From the quotients obtained, the relation between the conducting-power of copper and of iron is found to be 5.65, while the absolute determinations give the number 5.59, than which a closer agreement can hardly be expected. If the values ob-

tained by Péclet for the conducting-power of the two metals be expressed in the same units, namely, 1 grm., 1 minute, and 1 centimetre, we get for

Copper	11·4
Iron	4·35

values which materially differ from the above.

§ 6.

As it may not be uninteresting to know also the conductivity of different soils, I have endeavoured to use the results obtained from the observations made in Upsala with the earth thermometer to ascertain the conductivity of those layers in which the thermometer was sunk.

From the observations* were obtained

$$\sqrt{\frac{c}{k}} = \begin{cases} 0\cdot070282 \text{ with 4 and 6 feet thermometer} \\ 0\cdot068996 \text{ with 6 and 10 feet thermometer.} \end{cases}$$

In this case the Swedish foot and the year are taken as units; if instead of them the centimetre and minute are introduced, we get

$$\frac{k}{c\delta} = \begin{cases} 0\cdot26952 \\ 0\cdot27958 \end{cases}.$$

The highest layer in which the thermometers were immersed, consisted of a mixture of sand and clay; the lower (5 to 10 feet) of moist clay, which, on being heated, lost 19 per cent. of its weight. The specific gravity and specific heat were found by a determination to be

	δ.	c.	cδ.
Argillaceous sand	1·725	0·4416	0·7618
Moist clay	1·821	0·4448	0·8100

from which

$$k = 0\cdot2053 \text{ for argillaceous sand,} \\ = 0\cdot2264 \text{ for moist clay.}$$

If it be assumed that the mean temperature of the earth's layers decreases about 1° C. for 30 metres of depth, and the conducting-power is equal to the value obtained for *k*, we can easily calculate the loss of heat of the earth's surface during a year. Suppose the earth covered with a layer of water 282·5 millims. in height, the heat communicated to it by the internal layers during a year would be enough to raise the temperature of this aqueous layer by 1° C.

Postscript.—The above experiments on the conductivity of

* "Mém. sur la Température de la Terre," &c., *Act. Reg. Soc. Scient. Upsala*, S. 3. vol. i. p. 211.

copper and iron I have subsequently continued on bars of larger dimensions*.

The length was 1180 millims., the breadth and thickness 35 millims. The results obtained agreed with the above; for the copper bar there was obtained—

At the temperature	$30^{\circ}5$. .	$\frac{k}{c\delta}$ 66·80
”	33·9	. .	65·34
”	41·0	. .	65·77
”	41·8	. .	66·76
”	44·0	. .	65·13
Mean	<u>38·2</u>		<u>65·96</u>

while the previous investigation gave

At a temperature $51^{\circ}3$. . 64·66.

These two values, reduced to the same temperature, are almost identical, if it be assumed that the coefficient of temperature for k has the same value for heat as for electricity.

XVIII. On the Composition of Samarskite.

By Professor H. ROSE †.

THE numerous analyses of this remarkable mineral which have been made in my laboratory do not agree very well in their results. Whilst M. von Peretz found in three analyses 14·16, 16·70, and 16·77 per cent. of oxide of uranium, the amount of this oxide, according to Chandler, is 17·87 and 20·56 per cent. The former found 9·15, 11·04, and 8·36 per cent. of yttria; the latter only 5·10 and 4·72 per cent.

This want of agreement proceeds from the defective methods employed in the separation of several of the constituents. The separation of the peroxides of uranium and iron from the yttria was effected by means of carbonate of baryta—a mode of separation of which I subsequently ascertained that it gives no certain results, as it is difficult to avoid throwing down yttria together with the precipitated oxides. This is the reason why the amount of yttria appeared so small in Chandler's analyses. The separation may, however, be well effected by means of oxalic acid.

When minerals containing niobium and tantalum are decom-

* The details of these investigations will be given in the *Nov. Act. Soc. Upsal.* S. 3. vol. iv.

† Translated from the *Monatsber. der Akad. der Wiss. zu Berlin*, Nov. 1862, p. 622.

posed in the ordinary mode by fusion with bisulphate of potash, the acids of niobium and tantalum are, indeed, very well separated by treatment of the fused mass with water; but if the separated metallic acids are not examined with the greatest care, errors may be fallen into; for these acids may be contaminated with many substances, the presence of which in them may often be unsuspected. It is well known that they always contain no inconsiderable quantities of peroxide of iron, which cannot be separated from them by acids, but only by converting it into sulphide of iron by sulphide of ammonium, and dissolving the latter in very dilute hydrochloric acid, during which process there is always danger of dissolving at the same time a small quantity of the metallic acids, especially tantalic acid. As peroxide of iron, after fusion with bisulphate of potash, dissolves completely, although slowly, in water, it is the tantalic acid and the acids of niobium which, after they have lost their sulphuric acid by ignition, expel the sulphuric acid from the sulphate of iron, and combine to form salts, from which the peroxide of iron cannot be extracted by dilute acids, but only by heating with concentrated sulphuric acid. It is only strong bases, from the compounds of which with sulphuric acid, tantalic acid and the acids of niobium are incapable of expelling the sulphuric acid, that can be perfectly separated from the above-mentioned acids by fusion with bisulphate of potash.

Other oxides, as well as peroxide of iron, may remain undissolved during the treatment of the mineral fused with bisulphate of potash with water, and not only weakly basic oxides, but also such as form compounds with sulphuric acid or with sulphate of potash, which are insoluble or difficult of solution, especially in the solution of sulphate of potash, or, if they are soluble in sulphuric acid at ordinary temperatures, separate from the solution when heated, or on the addition of a large quantity of water. Of this kind are silicic acid, stannic acid, zirconia, thorina, tungstic acid, titanitic acid, and also the oxide of cerium (and those of lanthanum and didymium). From many of these oxides it is difficult to separate the acids of niobium and tantalum by decomposition with bisulphate of potash; and if they are not particularly sought for, or their presence is not suspected, they may readily escape detection. Zirconia and thorina, especially, may either be entirely overlooked, or their quantity incorrectly determined in the analysis of minerals containing tantalum and niobium, if these are decomposed by fusion with bisulphate of potash. Baryta, strontia, and oxide of lead (the latter at least not in appreciable quantities) have not hitherto been found in those minerals, but their separation also would be attended with no small difficulty.

When, therefore, the composition of minerals containing tantalum, and especially niobium, has not been thoroughly ascertained by experiment, it is as well to give up altogether the decomposition by bisulphate of potash, and to effect the decomposition by potash. By this means the zirconia and thorina, as also titanitic acid and oxide of cerium, which are insoluble in an excess of potash, may be separated from the acids of tantalum, and especially of niobium, which dissolve as potash salts, are readily soluble in an excess of potash, and can only be contaminated with tungstic acid and stannic acid, from which they are easily separated, and also by silica. The decomposition is best effected by fusion with hydrate of potash. As, however, this must take place in a silver crucible, the employment of which is attended by many inconveniences, and by which a contamination of the fused mass with oxide of silver cannot be avoided, it is more advisable to employ carbonate of potash, with which the mineral may be fused in the platinum crucible. If the fusion be effected at first over a lamp, and then only for a short time with a small blast, the decomposition is perfect.

I had a particular interest in establishing the correct composition of Samarskite, as I had been furnished, by the liberality of M. von Samarski, with a very large quantity of this rare mineral for investigation. The mineral also is interesting in many respects. As the analyses of Samarskite made in my laboratory differ so considerably from each other, I induced M. Finkener to repeat the analysis of the mineral; and only by his invincible perseverance has it been possible, notwithstanding the partly imperfect methods of separation, to arrive at satisfactory results, and detect substances previously overlooked.

After the decomposition of the mineral by carbonate of potash, and the treatment of the fused mass with water, the hyponiobic acid was precipitated from the solution by sulphuric acid, and separated from small quantities of tungstic and stannic acids. Small quantities of peroxide of copper were precipitated from the solution by sulphuretted hydrogen; the solution was then slightly supersaturated with ammonia, and the bases, except lime and magnesia, were thrown down by sulphide of ammonium, partly as oxides, partly as sulphides. From the solution of these in hydrochloric acid, after saturation with ammonia, the oxides were again precipitated by carbonate of ammonia and sulphide of ammonium, and only oxide of uranium was dissolved; this, as appeared on examination, contained zirconia. The separation of these is attended with great difficulty, and could only be effected by neutralizing the solution in sulphuric acid by ammonia and boiling it, when zirconia, containing, however, oxide of uranium, was precipitated, and the greater part of the

oxide of uranium, although contaminated with a little zirconia, remained in solution. It was only by the repetition of this process that a separation could be effected.

From the solution of the precipitated oxides and sulphides in nitromuriatic acid, after neutralization with ammonia, oxalate of ammonia precipitated yttria and the oxides of cerium, whilst peroxide of iron and protoxide of manganese remained in solution. The precipitate produced by oxalic acid was dissolved in sulphuric acid, the excess of the latter driven off, and the residue dissolved in water. This solution, when concentrated, exhibited the property of depositing a crystalline salt when heated, which again dissolved on cooling—a property by which, as is well known, thorina is distinguished. But its separation from the oxides of cerium, as also from small quantities of zirconia, was very difficult, and could only be approximately effected, partly by adding to the solution of the oxalates so much hydrochloric acid that only the oxalates of protoxide of cerium and yttria dissolved, and oxalate of thorina (which, of all the oxides precipitated by oxalic acid, is most difficult of solution in hydrochloric acid) remained undissolved, and partly by treating the oxalates with a solution of acetate of ammonia, to which a little free acetic acid had been added, in which oxalate of thorina dissolves readily, but the other oxalates with difficulty.

In order to obtain a certain result, I had the analysis repeated once more by Mr. Stephens. The found quantities of thorina and zirconia agreed in the two analyses more closely than could have been expected, as the two substances could only be separated by imperfect methods. Finkener obtained 4.35 per cent., and Stephens 4.25 per cent. of zirconia; the former 6.05 per cent., the latter 5.55 per cent. of thorina.

To the rare bodies which had already been found in Samarskite, we have, therefore, according to these analyses, to add zirconia and thorina. Except in Berzelius's thorite, the latter has hitherto been found only in monazite by Kersten, and in pyrochlore by Wöhler; the latter mineral also contains niobium. It is, however, to be expected that thorina will be found in other tantaliferous and niobiferous minerals.

XIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 73.]

May 1, "ON the Termination of Nerves in Muscles, as observed 1862. in the Frog; and on the disposition of the Nerves in the Frog's Heart."—The Croonian Lecture. By Prof. A. Kölliker, For. Memb. R.S.

Phil. Mag. S. 4. Vol. 25. No. 166. Feb, 1863.

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May 8.—Major-General Sabine, President, in the Chair.

The following communication was read:—

“Appendix to the Account of the Earthquake-wave Experiments made at Holyhead.” By Robert Mallet, Esq., C.E., F.R.S.

This communication contributes the sequel of the author’s “Report on Earthquake-Wave Experiments” (made at Holyhead), as published in part 3 of the ‘Philosophical Transactions’ for 1861. At the conclusion of that paper the author expressed his hope of being able soon to lay before the Royal Society some experiments for the determination of the modulus of elasticity of perfectly solid portions of both the slate and the quartz rock formation through which his wave-transit experiments had been made at Holyhead, with a view to throw light upon the relations between the theoretic velocity of transmission (if the rocks were all solid and homogeneous) and the actual velocity as determined by experiment.

He has now determined the elastic modulus for both rocks, and for each rock in two directions, viz. parallel to and transverse to its lamination; and he has extended his determinations to specimens of each rock of maximum and of minimum compactness and hardness, so that the series of experiments upon the compressibility of these rocks (from which the modulus is derived) assumes the following divarication, viz. :—

Slate rock ..	{	Hardest .	{ B. Parallel to laminæ, Table 2.
		Softest ..	{ A. Transverse to laminæ, Table 1.
Quartz rock .	{		Hardest .
		Softest ..	
			Hardest .
			{ G. Transverse to laminæ, Table 7.

Involving thus eight distinct series of experiments.

The compressions were conducted at the Royal Arsenal, Woolwich, by the aid of the excellent American machine belonging to the Royal Gun-factories, permission to use which was accorded to the author.

The specimens of rock submitted to pressure were all equal cubes of 0·707 inch on the edge, presenting thus a surface on each side of 0·5 square inch—a dimension presenting facilities for tabular reduction, &c.

The cubes were cut from the chosen rock specimens (selected with care as fairly representative) by means of the lapidary’s wheel, and had opposite faces rigidly parallel and equal.

The pressures advanced by 1000 lbs. per square inch of surface, from zero up to the crushing point of the specimen; and at each advance the actual compression of the column of rock was measured by instrumental arrangements that admitted of reading space to ·0005 of an inch. The results are given in Tables numbered 1 to 8, referred to above, and these are compared in two Tables numbered 9 and 10.

The following are the mean compressions for each 1000 lbs. per square inch:—

Slates.				Quartz.			
A.	B.	E.	F.	C.	D.	G.	H.
inches. ·000627	inches. ·0025000	inches. ·0039144	inches. ·0037000	inches. ·0007085	inches. ·0010947	inches. ·0014666	inches. ·0172666
up to 23,000 lbs.	up to 26,000 lbs.	up to 14,000 lbs.	up to 7000 lbs.	up to 35,000 lbs.	up to 19,000 lbs.	up to 12,000 lbs.	up to 6000 lbs.

Crushing usually took place at 1000 to 2000 lbs. additional pressures beyond the above limits, up to which the compressions were tolerably uniform.

The discussion of these Tables fully presents some interesting and novel results.

Generally the quartz rock is less compressible than the slate; the softest quartz, however, is much more compressible than the softest slate in a direction parallel to the lamination of both. In this direction also the hardest slate is more than double as compressible as the hardest quartz. Transverse to the lamination, however, both the hardest slate and quartz have nearly the same coefficient of compressibility, which is very small for both. In the latter direction also the *softest* slate and quartz have almost the same coefficient, but one about *four times* as great as for the *hardest* like rocks.

The author points out several conclusions of much interest deducible from these experiments as to the physical and geological conditions under which these rocks were formed and consolidated. The compression by natural forces has already been greatest in directions transverse to the lamination. The great compressibility in the opposite directions, or parallel to the lamination, appears to arise chiefly from the mass of the rock being made up of minute wedge-shaped mineral particles, deposited all with their largest dimensions on the plane of lamination, and so acting on each other like wedges.

Some curious circumstances in the mode of giving way of the rocks under pressure are shown by the author to be probably connected with their mass being formed of an aggregate of several simple minerals.

He points out the great differences in wave-transmissive power in directions transverse to and parallel to the lamination which these experiments disclose. The specific gravities of the several specimens of rock are then given, to enable the modulus of elasticity to be obtained in feet, and the general results of the experiments are comprised in the following Table (p. 148):—

The author then proceeds to apply these results to the comparison of the theoretic and actual transit-periods of the wave of impulse.

The general expression for elastic wave-propagation in a homogeneous medium may be expressed by an equation of the form

$$V = \sqrt{g\bar{L}} = 8\cdot024 \sqrt{\bar{L}},$$

HOLYHEAD ROCK COMPRESSION.
General results reduced, modulus of cohesion and of elasticity, &c.—Slate and Quartz.

No.	Class of rock, and direction of pressure in relation to structure.	Coefficient of compression on unit surface for 1000 lbs.	Elastic limit for compression.	Crushing load on the unit of surface.	Modulus of cohesion (compression).	Modulus of elasticity.	Modulus of elasticity.	Coefficient T_r .
		inches.	lbs.	lbs.	feet.	lbs.	feet.	
1	Slate <i>hardest</i> across lamination	·006217	22,000	24,000	20,014	8,042,464	6,706,524	1·2432
2	Quartz <i>hardest</i> across lamination	·007085	32,000	37,000	32,095	7,037,163	6,121,758	2·1830
3	Slate <i>hardest</i> parallel to lamination	·0025000	18,000	27,000	22,515	1,667,778	1,667,778	5·6241
4	Quartz <i>hardest</i> parallel to lamination	·0010947	17,000	20,000	17,349	4,567,461	3,962,013	1·8240
5	Slate <i>softest</i> across lamination	·0039144	12,000	15,000	12,586	1,277,335	1,071,769	4·8930
6	Quartz <i>softest</i> across lamination	·0014666	11,000	14,000	12,158	3,409,246	2,960,699	1·7108
7	Slate <i>softest</i> parallel to lamination	·0037000	6,000	9,000	7,552	1,351,351	1,133,874	2·7747
8	Quartz <i>softest</i> parallel to lamination	·0172666	7,000	8,000	6,948	289,576	251,477	11·6112
	<i>Calculated Means.</i>							
9	Slate, mean for hard and soft across lamination	·0022680	17,000	19,500	16,311	2,204,585	1,844,069	3·6855
10	Quartz, mean for hard and soft across lamination	·0010875	16,500	25,500	22,132	4,597,701	3,990,455	2·3103
11	Slate, mean for hard and soft parallel to lamination	·0031000	12,000	18,000	15,056	1,612,903	1,349,145	4·6494
12	Quartz, mean for hard and soft parallel to lamination	·0091806	12,000	14,000	12,151	544,627	472,684	10·7100
	<i>Calculated Means of Means.</i>							
13	Slate, hard and soft, mean for both directions (Nos. 9 and 11)	·0026840	14,500	18,750	15,684	1,862,880	1,566,541	4·1914
14	Quartz, hard and soft, mean for both directions (Nos. 10 and 12)	·0051340	16,750	19,750	17,141	973,899	845,252	8·4490
15	General mean for slate and quartz, hard and soft, and in both directions (Nos. 13 and 14)	·0039090	15,625	19,250	16,398	1,279,099	1,089,615	6·2697

where L is the modulus of elasticity in feet. Where, from want of homogeneity or shattering, &c., as found in nature, the experimental value of V differs from this, we may express it by the same form of equation,

$$V' = \alpha \sqrt{L},$$

the coefficient α having to $\sqrt{2g}$ the rate that the actual bears to the theoretic value of V .

He then determines the value of α for three of his mean experimental transit-velocities at Holyhead, and obtains as follows:—

Feet per side.	
$V' = 1089$	$\alpha = 0.637$
$V' = 1352$	$\alpha = 0.791$
$V' = 1220$	$\alpha = 0.714$

The actual velocity of wave-transmission in the slate and quartz rocks, taken together, was to the theoretic velocity due to their materials, if perfectly solid,

$$\alpha : \sqrt{2g}, \text{ or as } 1.00 : 8.89 ;$$

so that nearly eight-ninths of the full velocity of wave-transmission due to the solid material is lost by reason of the heterogeneity and discontinuity or shattering of the rocky mass as it is piled together in nature.

The author then shows that were the rocks *quite solid*, the velocity of wave-transmission would be—

Mean of slate and quartz transverse to lamination $V = 13,715$ feet per second.

Mean of slate and quartz parallel to lamination $V = 7659$ feet per second.

This difference is probably reversed *in nature* by reason of the greater discontinuity in the former direction. The author then shows that his results, which appear at first sight to conflict with those of an analogous character obtained by Helmholtz and others for wood, in the three principal directions of its section, are strictly in accordance and analogy with the results of these experimenters.

The author concludes by deducing some conclusions as to the bearing power, safe load, and proper direction as to lamination when exposed to pressure, of these rocks, of a practical character, and valuable to the civil engineer or architect.

May 15.—Major-General Sabine, President, in the Chair.

The following communications were read:—

“On the Sensory, Motory, and Vaso-motory Symptoms resulting from Refrigeration and Compression of the Ulnar and other Nerves in Man.” By Augustus Waller, M.D., F.R.S.

“On the Rigidity of the Earth.” By Professor William Thomson, F.R.S.

The author proves that unless the solid substance of the earth be on the whole of extremely rigid material, more rigid for instance than steel, it must yield under the tide-generating influence of sun

and moon to such an extent as to very sensibly diminish the actual phenomena of the tides, and of precession and nutation. Results of a mathematical theory of the deformation of elastic spheroids, to be communicated to the Royal Society on an early occasion, are used to illustrate this subject. For instance, it is shown that a homogeneous incompressible elastic spheroid of the same mass and volume as the earth, would, if of the same rigidity as glass, yield about $\frac{7}{9}$, or if of the same rigidity as steel, about $\frac{2}{5}$ of the extent that a perfectly fluid globe of the same density would yield to the lunar and solar tide-generating influence. The actual phenomena of tides (that is, the relative motions of a comparatively light liquid flowing over the outer surface of the solid substance of the earth), and the amounts of precession and nutation, would in the one case be only $\frac{2}{9}$, and in the other $\frac{3}{5}$ of the amounts which a perfectly rigid spheroid of the same dimensions, the same figure, the same homogeneous density, would exhibit in the same circumstances. The close agreement with the results of observation presented by the theory of precession and nutation, always hitherto worked out on the supposition that the solid parts of the earth are perfectly rigid, renders it scarcely possible to admit that there can be any such discrepancy between them as 3 to 5, and therefore almost necessary to conclude that the earth is on the whole much more rigid than steel. But to make an accurate comparison between theory and observation, as to precession, it is necessary to know the absolute amount of the moment of inertia about some diameter; and from this we are prevented by the ignorance in which we must always be as to the actual law of density in the interior. Hence the author anticipates that the actual deformation of the solid earth by the lunar and solar influence may be more decisively tested by observing the lunar fortnightly and the solar half-yearly tides*. These tides, it may be supposed, will follow very closely the "equilibrium theory" of Daniel Bernoulli for all oceanic stations, and the author suggests Iceland and Teneriffe as two stations well adapted for the differential observations that would be required.

The earth's upper crust is possibly on the whole as rigid as glass, more probably less than more. But even the imperfect data for judging referred to above, render it certain that the *earth as a whole must be far more rigid than glass*, and probably even more rigid than steel. Hence the interior must be on the whole more rigid, probably many times more rigid, than the upper crust. This is just what, if the whole interior of the earth is solid, might be expected, when the enormous pressure in the interior is considered; but it is utterly inconsistent with the hypothesis held by so many geologists that the earth is a mass of melted matter enclosed in a solid shell of only from 30 to 100 miles thickness. Hence the investigations now brought forward confirm the conclusions arrived at by Mr. Hopkins,

* High tide, as far as the influence of either body is concerned, is produced at the poles, and low (average) water at the equator, when its declination, whether north or south, is greatest, and low water at the poles and high water at the equator, when the disturbing body crosses the plane of the equator.

that the solid crust of the earth cannot be less than 800 miles thick. The author indeed believes it to be extremely improbable that any crust thinner than 2000 or 2500 miles could maintain its figure with sufficient rigidity against the tide-generating forces of the sun and moon, to allow the phenomena of the ocean tides and of precession and nutation to be as they are.

“On the Difference in the Properties of Hot-rolled and Cold-rolled Malleable Iron, as regards the power of receiving and retaining Induced Magnetism of Subpermanent Character.” By George Biddell Airy, Esq., F.R.S., Astronomer Royal.

The author states that he had been desirous of examining whether differences in the degree of change of subpermanent magnetism, such as are exhibited by different iron ships, might not depend on the temperature at which the iron is rolled in the last process of its manufacture. By the good offices of Mr. Fairbairn he had received gratuitously from Richard Smith, Esq., Superintendent of Lord Dudley's Iron Works at the Round Oak Works near Dudley, twenty-four plates of iron, each 16 inches long, 4 inches broad, and $\frac{1}{4}$ inch thick; twelve of which, after having been manufactured with the others in the usual way, had been passed through rollers when quite cold. Each set of twelve was divided into two parcels of six each, one parcel being cut with the length of the bars in the length of extension of the fibres of the iron, the other being cut with the length of the bars transverse to the length of extension.

For experimenting on these, a large wooden frame was prepared, capable of receiving the 24 bars at once, either on a plane transverse to the direction of dip at Greenwich, or on a plane including the direction of dip. In some experiments, these planes were covered with flag-stones, and the bars were laid upon the flag-stones; in others, the bars were laid immediately upon the wood. While there lying, they were struck with iron or wooden hammers of different sizes. The bars of the different classes were systematically intermingled, in such a way that no tendency of the arm to give blows of a different force or kind in special parts of the series could produce a class-error in the result. For examination of the amount of polar magnetism in each bar, it was placed at a definite distance (5 inches) below a prismatic compass, which was used to observe the apparent azimuth of a fixed mark; the bar was then reversed in length, and the observation was repeated in that state.

The number of experiments was 21. They were varied by difference in the succession of positions of the bars, difference of time allowed for rest, difference in the violence of the blows, &c.

The principal results appear to be the following:—

1. The greatest amount of magnetism which a bar can receive, appears to be such as will produce (on the average of bars) a compass-deviation of about 11° , the bar being 5 inches below the compass. It was indifferent whether the bars rested on stone or on wood, or whether they were struck with iron or with wood, the bars lying on the dip plane while struck.

2. When the bars, thus charged, lay on the plane transverse to the dip, they lost about one-fifth of their magnetism in one or two days, and lost very little afterwards.

3. When the charge of magnetism is smaller than the maximum, the diminution in a day or two is nearly in the same proportion as for the maximum.

4. The effect of violence on the bars, when lying on the plane transverse to the dip, is not in all cases to destroy the magnetism completely, sometimes it increases the magnetism.

5. The Cold-Rolled Iron receives (under similar violence) or parts with (under similar violence) a greater amount of magnetism than the Hot-Rolled Iron, in the proportion of 6 to 5.

6. There is some reason to think that the Hot-Rolled Iron has a greater tendency to retain its primitive magnetism than the Cold-Rolled Iron has.

7. There is some reason to think that, when lying tranquil, the Hot-Rolled Iron loses a larger portion of its magnetism than the Cold-Rolled Iron loses in the same time.

“On the Analytical Theory of the Conic.” By Arthur Cayley, Esq., F.R.S.

May 22.—Major-General Sabine, President, in the Chair.

The following communication was read :—

“On the Constitution of Sea-water, at different Depths, and in different Latitudes.” By George Forchhammer, Ph.D., Professor of Mineralogy in the University of Copenhagen.

Professor Forchhammer was present at the Meeting, and, by request of the President, gave a statement of the principal results of his researches. He first, however, took occasion to express his great satisfaction in being allowed the opportunity of personally and gratefully acknowledging the liberality with which men of science in this country had entered into his views and supplied him with specimens requisite for carrying on his inquiries; and he particularly mentioned the name of a late distinguished Fellow of this Society, Sir James Clark Ross, who had kindly furnished various samples of sea-water procured in his Antarctic voyage.

The number of elements hitherto found in sea-water the author stated to be thirty-one, viz. *Oxygen*, *Hydrogen*, *Azote* in ammonia, *Carbon* in carbonic acid, *Chlorine*, *Bromine*, *Iodine* in fuci, *Fluorine* in combination with calcium, *Sulphur* as sulphuric acid, *Phosphorus* as phosphoric acid, *Silicium* as silica, *Boron* as boracic acid, discovered by the author both in sea-water and in sea-weeds, *Silver* in the *Pocillopora aleicornis*, *Copper* very frequent both in animals and plants of the sea, *Lead* very frequent in marine organisms, *Zinc* principally in sea-plants, *Cobalt* and *Nickel* in sea-plants, *Iron*, *Manganese*, *Aluminium*, *Magnesium*, *Calcium*, *Strontium* and *Barium*, the latter two as sulphates in fucoid plants, *Sodium*, *Potassium*. These twenty-seven elements the author himself had ascertained to occur in sea-water; the presence of the next four elements, viz. *Lithium*, *Cæsium*, *Rubidium*, and *Arsenic*, has been shown by other chemists.

Of these elements only a few occur in such quantity that their determination has any notable influence on the quantitative analysis of sea-water, viz. Chlorine, Sulphuric acid, Magnesia, Lime, Potash, and Soda. The others, as far as their existence has been determined in the sea-water itself, are found in the residue which remains after evaporation to dryness and redissolution of the salts in water.

The author next stated that in the water of the ocean far from the shores the principal ingredients always occur very nearly in the same proportions. If we assume chlorine=100, the mean proportion of the other leading constituents is as follows:—

	Mean proportion.	Maximum.	Minimum.
Sulphuric acid ..	11·89	12·09	11·65
Lime	2·96	3·16	2·87
Magnesia.	11·07	11·28	10·95
All salts	181·1	181·4	180·6

These proportions apply only to specimens obtained at a long distance from shores, or in the open ocean. In the interior of the Baltic, for instance, the proportion of chlorine to sulphuric acid is as 100 to 14·97—to lime as 100 to 7·48; and the proportion of chlorine to all salts as 100 to 223·0. This constant proportion of the different constituents in the ocean depends evidently not upon any chemical combination and affinity between the different substances, but upon the enormous quantity of salts in the whole ocean, which renders imperceptible any difference that might otherwise arise from the different proportion in which salts are carried into the sea by rivers. It depends, besides, on the uniform action of the numberless organic beings inhabiting the ocean which abstract sulphuric acid, lime, potash, and magnesia from the water, and render them insoluble.

The mean quantity of solid matter in the water of the ocean generally, the author found to be 34·304 per 1000. To determine this mean quantity he has divided the ocean into regions, viz. :—

1st Region. Atlantic, from the Equator to 30° N. lat.; mean 36·169.

2nd Region. Atlantic, from 30° N. lat. to a line from the north of Scotland to the north of Newfoundland; mean 35·976.

3rd Region. From the northern boundary of region 2 to the south coast of Greenland; mean 35·556.

4th Region. Davis's Strait and Baffin's Bay; mean 33·167.

5th Region. Atlantic, between 0 and 30° S. lat.; mean 36·472.

6th Region. Atlantic, between 30° S. lat. and a line from the southernmost point of Africa to the southernmost point of America; mean 35·038.

7th Region. Between Africa and the East Indian Islands; mean 33·868.

8th Region. Between the East Indian and the Aleutic Islands; mean 33·506.

9th Region. Between the Aleutic and the Society Islands; mean 35·219.

10th Region. The Patagonian stream of cold water ; mean 33·966.

11th. The Antarctic region ; mean 28·563.

Besides these regions of the great ocean, the author enumerates some other regions, which are under the decided influence of the surrounding land. Such are the North Sea, with a mean quantity of solid matter of 32·806 per 1000 ; the Kattegat and Sound, with a mean of 15·126 ; the Baltic, mean 4·807 ; the Mediterranean, mean about 37·5 ; the Black Sea, mean 15·894. Of the proportion in the large bays of America the author had only one observation, viz. in water from the Caribbean Sea, in which the quantity of saline matter was found to be 36·104 per 1000.

The author then showed that the equatorial regions contain the greatest percentage of saline matter, and that this peculiarity is owing to the evaporation under and in the neighbourhood of the line being greater than the quantity of water supplied by the rain falling on the sea and by the rivers flowing from the land ; that the equilibrium is maintained by polar currents, which bring water with less saline matter to the equatorial regions. The mean quantity of saline ingredients in the equatorial regions of the ocean is about 36·2 per 1000, while in the polar regions it is about 33·5.

The North Atlantic Ocean contains much more salt than the South Atlantic, which the author explains by the prevailing influence of the Gulf-stream ; and from his analyses of many samples of water taken in the current which flows from N.E. to S.W., between Iceland and the east coast of Greenland, he thinks it highly probable that this East Greenland current is in reality not a polar current, but a returning branch of the Gulf-stream, its mean quantity of salt being nearly the same as in the northern part of the Atlantic Ocean, viz. 35·5 per 1000.

The author then compared the Mediterranean with the Baltic, and stated that there is a double current at the entrance of the Baltic as well as in the Straits of Gibraltar ; but with this difference, that the under-current of the Mediterranean runs out of, and the surface-current generally runs into, that sea ; whereas the under-current of the Baltic is an entering one, and the surface-current of the Sound generally runs out into the Kattegat and North Sea. He showed, moreover, that the deep water in both seas is richer in salt than that from the surface, and consequently has a greater specific gravity.

In the Atlantic he found the reverse, viz. that the quantity of saline ingredients in the water *decreases* with the depth, if the samples are taken at some distance from the shore ; and as his analyses are sufficiently numerous, and include specimens from great depths (12,000 feet), he considers this unexpected result to be tolerably well established. He thinks that this fact would prove the existence of a polar current in the depths of the Atlantic, as well as in some parts of its surface.

In the sea to the east of Africa he found the quantity of saline matter slightly increasing with the depth.

XX. *Intelligence and Miscellaneous Articles.*

ON THE FORMS OF LENSES PROPER FOR THE NEGATIVE EYEPieces OF TELESCOPES. BY G. B. AIRY, ESQ., ASTRONOMER ROYAL.

IN the 'Monthly Notices' for June and November last, there are discussions on the forms of lenses proper for the negative eyepiece. Perhaps I shall not do wrong in stating to the Society that as long ago as the year 1827 I made a most elaborate investigation of the properties of eyepieces as depending on the curvatures of their surfaces. The paper is entitled "On the Spherical Aberration of the Eyepieces of Telescopes," and is printed in the 'Transactions of the Cambridge Philosophical Society,' vol. iii. This paper had been preceded by one "On the Chromatic Aberration of the Eyepieces of Telescopes," from which I had been able to infer the proportions of the focal lengths and intervals of the lenses which (independently of their curvatures) destroy colour at the sides of the field, using but one kind of glass; and had selected some of these as examples to which the formulæ for spherical aberration were to be applied. One of these is the Huyghenian eyepiece, with the following very common proportions:—focal length of first lens or field-glass = $3M$; focal length of second lens or eye-glass = M ; interval between the two lenses = $2M$.

The principal results as applying to the cases before us are the following:—

First, as regards distortion.

I must refer to page 15 of the Memoir for the general formula; but I may quote the following special results:—

(a) It is possible to destroy distortion entirely, but not by the use of common forms (equiconvex or plano-convex).

(b) The most favourable combination of common lenses is,—the first, equiconvex; the second, plano-convex, with its convex side towards the first lens, or with its plane side next the eye.

Second, as regards indistinctness at the edge of the field.

From the general formulæ in page 35, it appears that it is impossible in any eyepiece whatever, in which the lenses are all convex, to secure distinctness in the approach to the edge of the field, except in some cases by movement of the eyepiece; and the problem always is to diminish the indistinctness as much as possible. For this purpose, in the Huyghenian eyepiece,

(c) The different points of the image may be made distinct by a little sliding of the eyepiece, by a form very nearly the same as the following:—

(d) The best form is, for the field-glass, a meniscus, with convex side towards the object-glass, and radii as 4 : 11; and for the eye-glass a convex lens, the mere convex side towards the field-glass, and radii as 1 : 6. The constants by which the indistinctness in the two dimensions is expressed are $\frac{95}{126}$ and $\frac{61}{126}$.

(e) The best combination of common lenses is two plano-convex lenses, the plane sides of both towards the eye. The constants of indistinctness then are $\frac{133}{126}$ and $\frac{175}{126}$.

(f) For a single lens to produce the same power, the smallest values of constants of indistinctness would be $\frac{196}{126}$ and $\frac{476}{126}$.

The following rule, though not strictly accurate, will be found sufficiently accurate to give a very good practical determination of the curvatures of all eyepiece lenses in all cases. Trace the course of an excentric pencil through the eyepiece. Consider separately the convergence, &c. of the axis of the pencil with regard to the axis of the telescope, and that of the rays of the pencil with regard to the axis of the pencil. When both these convergences fall on one side (as in the Huyghenian field-glass), the lens ought to be meniscus. When they are at equal distances on opposite sides, the lens ought to be equiconvex. When the convergence of either is much nearer (the other being on the opposite side), the side of the lens next it ought to be plane.—*Monthly Notices of the Roy. Astronom. Soc.* Dec. 12, 1862.

ON THE DURATION OF THE COMBUSTION OF FUSES UNDER DIFFERENT ATMOSPHERIC PRESSURES. BY M. DUFOUR.

Hitherto we have had but few data in reference to the influence of the atmospheric pressure on the activity of combustion. Moreover, what data we have sometimes appear contradictory; thus in 1841 M. Triger noticed a more rapid combustion of candles in a medium where the air was under a pressure of three atmospheres; while Prof. Frankland in a recent ascent of Mont Blanc did not perceive any essential difference between the combustion of candles at Chamounix and on the top of the mountain.

In 1855 Quarter-Master Mitchell of the English Navy contributed to the Royal Society experiments made at different heights in the Himalayas with fuses. His results show that the duration of the combustion increases as the pressure decreases; the combustion appeared less active under a less pressure. Prof. Frankland repeated and confirmed Mr. Mitchell's experiments. He used fuses of six inches from the Woolwich Arsenal; these fuses were burned in a close vessel in an atmosphere which could be artificially exhausted. In Frankland's experiments the pressure necessarily varied a little between the commencement and the end of the combustion; and, spite of the ingenious arrangements which he adopted, it was to be feared that the combustion was influenced by the restricted dimensions of the space in which it took place. In the month of last July I investigated the duration of the combustion of fuses under conditions different to those under which the English physicist worked. I operated in the open air, seeking at different heights on the Alps gradually lower pressures.

The determination of the duration of the combustion of a fuse is rather uncertain if the observer himself wishes to note on any chronometric apparatus the moment at which the fuse kindles, and that at which it finishes. There is great risk of the introduction of personal errors, which are different in different cases. In order to avoid these errors, I measured the duration of the combustion by means of an electric register. The fuse was lit by a pistol, in which the fall of the lock broke the current. The end of the combustion exploded a small quantity of powder, which, by letting a metallic rod fall, again completed the circuit. The duration of the phenomenon was thus indicated on the register by the interval between two continuous marks; this interval is easily transformed into time by a known process, which it is needless to develop here. Preliminary experiments were made in order to ascertain the degree of exactitude of which the method and the apparatus were susceptible. These experiments show that the possible error did not exceed $\frac{1}{15}$ th of a second.

Two groups of fuses were submitted to experiment under five different pressures, varying between 730 millimetres and 538 millimetres. The fuses were not so identical as to burn for exactly the same time under the same pressure. To know the influence of the density of the external air, it was necessary to burn a certain number of fuses in the same place, and then to take the mean: a comparison of the means ought to show the influence of the pressure. In order to neutralize as much as possible the inconvenience arising from the difference in the fuses, the means of eight to ten experiments were taken at each station. Altogether 68 fuses were burned in the five stations. The crest of the Chenalletes, which commands the Convent of St. Bernard at 9700 feet above the sea-level, is the highest station of the series. With some difficulty the instruments were moved to and fixed at this elevated position.

The fuses of the first group unfortunately presented considerable individual differences. Those of the second group were much more regular. The following is an abstract of the means:—

First Group.

Station.	Height.	Pressure.	Mean	Mean
	metres.	millims.	Duration.	Difference.
			s.	s.
Ouchy	380	728	9.96	0.52
Gonize	920	685	10.11	0.54
St. Pierre	1640	628	10.52	0.50
St. Bernard . .	2478	568	11.20	0.55

Second Group.

	metres.	millims.	s.	s.
Ouchy	380	731	9.15	0.23
St. Pierre	1640	628	10.12	0.29
Chenalletes . .	2890	538	11.09	0.26

Thus in both groups the duration increases as the pressure diminishes.

To determine the magnitude of this variation between two determinate pressures, it is enough to divide the increase of the duration by the total duration at the higher pressure, and by the difference of the pressures. A coefficient is thus obtained which expresses the mean increase of the unit of duration (1^s) for a diminution of 1 millimetre in the pressure. The differences in the fuses of the first group were too great to be safely used in calculating this coefficient; those of the second group give the following values:—

Between Ouchy and St. Pierre.....	0·00104
Between St. Pierre and Chenallettes	0·00108

These two coefficients, which are almost identical, show that the increase in the duration of the combustion is proportional to the diminution in pressure. This very simple law has been already enunciated by Prof. Frankland.

To compare the values I have obtained with those of Messrs. Mitchell and Frankland, it is enough to calculate by the aid of their results the millimetric coefficient between limits of pressure nearest those under which I operated. Taking Mr. Mitchell's observations in the Himalayas at pressures of 752 and 584 millims., the coefficient is 0·00161; between the pressures 752 and 609 millims. it is 0·00140. Thus the increase was a little greater in these fuses. Of the six pressures in Prof. Frankland's experiments, the two which approach most closely to the limits within which I worked are the second, 716·8 millims., and the fourth, 570·2 millims. He found for the duration of the combustion:

	millims.	s.
A	716·8	32·25
A	570·2	37·75

from which is deduced the coefficient of variation.....0·00116
Between Ouchy and Chenallettes my experiments give... 0·00111

This is certainly a remarkable agreement, and the more interesting as Prof. Frankland's fuses differ both in dimensions, shape, and duration from those which gave the above results. Hence it may be admitted that the duration of the combustion of one of these fuses increases on the average by 0·0011 of its value for each diminution of a millimetre on the pressure.

These facts have an importance which cannot be neglected in a military point of view, and they ought to be taken into account whenever the duration of the combustion of a fuse is an essential element in its use.

As to the cause of this increase of the duration of combustion when the density of the surrounding air is less, one would at first sight be tempted to ascribe it to a diminution of the oxygen. This idea, however, is not tenable, as the fuses contain enough burning substances in the form of nitrate. To convince myself directly that atmospheric oxygen does not intervene, I burned three fuses in a large

bell-jar filled with pure carbonic acid. Ignition was effected by means of an electric current. The mean duration of the combustion under a pressure of 715 millims. was 8^s.57. Hence it is the purely physical part of the change in pressure which so strongly influences the activity of the combustion of the fuses.—*Comptes Rendus*, Nov. 24, 1862.

SUPPLEMENT TO MR. DRACH'S PAPER "ON THE CIRCUMFERENCE OF THE CIRCLE" (PHIL. MAG. SUPPL. DEC. 1862).

I have since discovered a more rapid series for $\frac{1}{\pi}$,

$$\text{viz. } 3 \left(1 + \frac{1}{20} \right) \cdot \left(\frac{1}{10} + \frac{3}{4 \cdot 10^6} + \frac{1}{10^{13}} + \frac{4}{9 \cdot 10^{16}} + \frac{4}{3 \cdot 10^{20}} \&c. \right) \\ + \frac{3}{10^{19}} - \frac{1}{9} \left(\frac{1}{10^9} + \frac{4}{10^{22}} + \frac{7}{10^{24}} \right),$$

which must be multiplied by $1 + \frac{1}{100} + \frac{1}{2000}$. If the last, $\frac{1}{2000}$, be multiplied by $\frac{1}{10^{23}} + \frac{2}{10^{26}} + \frac{11}{10^{29}}$, we get a value true to the thirty-first decimal.

Example.

10000	07500	00100	04445	77777			
31500	23625	00315	14004	20000	do.	× 315	
			+ 30	00000			
	- 11	11111	11111	11563			
31500	23613	89204	02923	08436	66666	66666	
+ 315	00236	13892	04029	23084	36666	66666	
15	75011	80694	60201	46154	21833	33333	
					1575	01811	
					3	15002	
Finally,						17325	
= 31830	98861	83790	67153	77675	26745	00164	

In my mode of finding π ,

$$A = 3000000 - 8007 = 2991993 = 3 \times 127 \times 7853;$$

this $\times \frac{21}{20} = 3.14159265 =$ square of circle whose diameter is 2, to the 8th decimal place. $A = 3(1000^2 - 50^2 - 13^2)$.

As 127 is decomposable 5 ways into four squares, as 7853 is thus decomposable 185 ways, and 315 in 17 ways, it is evident that by properly applying Euclid 47, I., we can construct the side of a square which up to the above numerical limit is equivalent to the aforesaid circle: $7853 = 67^2 + 58^2$.

Has it ever been noted that, whereas in polyhedrons the three peculiar angles are $\cos 109^\circ 28' 16'' = \frac{1}{3}$, $\cos 116^\circ 33' 54'' = \frac{1}{\sqrt{5}}$, and

$\cos 138^\circ 11' 23'' = \frac{1}{3}\sqrt{5}$, these must form the hypotenuse and legs of a spherical triangle, with the respective angles of 90° ,

$$\tan^{-1}(-3) = 106^\circ 26' 06'' \text{ and } 135^\circ = 90 + 45.$$

The second angle = $225^\circ - 116^\circ 33' 54''$. Further,

$$\cos 109^\circ \&c. : \cot 116^\circ \&c. : \sin 138^\circ \&c. : : 2 : 3 : 4.$$

London, January 8, 1863.

FRESH-WATER LAKES WITHOUT OUTLET.

To the Editors of the *Philosophical Magazine and Journal*.

Linfield, Belfast,
January 21, 1863.

GENTLEMEN,

In your Number for April 1858, Mr. Jennings mentions two lakes near Damascus as being without an outlet and yet fresh, which he regards a unique exception to the rule that lakes without an outlet are salt. I think the lake of Tacarigua, or Valencia, in the north of South America is another instance of the same kind. Humboldt, in his 'Personal Narrative,' vol. iv., gives a description of the lake, which he distinctly states to be without outlet; and though I cannot find any statement in so many words as to whether it is salt or fresh, I infer the latter from the following circumstances:—

Were it salt, an observer like Humboldt would mention its degree of saltiness.

He compares it with the alpine lakes without mentioning saltiness as a point of contrast.

Its vegetation is that of fresh water. "The banks, shaded by tufts of *Coccoloba barbadensis*, and decorated with fine liliaceous plants (*Pancreatium undulatum*, *Amaryllis nervosa*), remind us, by the appearance of the aquatic vegetation, of the marshy shores of our lakes in Europe. We find there pondweed (*Potamogeton*), *Chara*, and cat's-tails 3 feet high."

Its fishes are those of fresh water. It contains three kinds, "the guavina, the vagra, and the sardina; the two last descend into the lake by the streams that flow into it."

Humboldt also remarks, "It is somewhat remarkable that the lake of Valencia, and the whole system of small streams which flow into it, have no large alligators, though this dangerous animal abounds a few leagues off in the streams that flow either into the Apure or the Oroonoko, or immediately into the Caribbean Sea."

I remain

Your obedient Servant,

JOSEPH JOHN MURPHY.

1863

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MARCH 1863.

XXI. *On a New Determination of the Mercury Unit of Electrical Resistance in Dr. Siemens's Laboratory.* By ROBERT SABINE, Esq.*

THE result arrived at by the Committee appointed by the British Association to report on standards of electrical resistance has induced Dr. Siemens to take up the subject again, and he has in consequence had the mercury unit determined for a third time.

Following the original determination described by Dr. Siemens in his paper "Vorschlag eines reproducibaren Widerstandsmaasses"†, the comparison of six tubes, of very different resistances, with one Jacobi's unit gave rise in some instances to an employment of the measuring apparatus beyond the limits of its exactness. This occasioned the observed resistances of the tubes numbered 1, 4, 5, and 6 to appear greater or less than their calculated values, according as a was less or greater than b ‡; where this was not the case, the differences were inconsiderable.

At the time of the first reproduction §, only three of the six

* Communicated by the Author.

† Poggendorff's *Annalen*, vol. cx. p. 1.

‡ a and b were the readings of the bridge wire. This will completely answer the criticism of Dr. A. Matthiessen (*Pogg. Ann.* vol. cxiv. p. 312), who repeats Dr. Siemens's figures of the differences in question as a proof that the method of a reproducible standard by means of mercury is not more to be depended upon than that of his gold-silver alloy, but which indeed only prove that the *observed* resistances are not so exact as the calculated,—in other words, that the measure produced was *more* exact than the instruments by which it was employed.

§ The reproduction alluded to took place in the summer of 1861 (*Pogg. Ann.* vol. cxiii. p. 94).

tubes used in the original determination remained unbroken. These, with two new ones (numbered 7 and 8), were exactly measured, and the weights of their contents of mercury, reduced to 0° C., found to agree with the former reduced weights to within 0·05 per cent. The resistances of the tubes were compared this time with spirals of glass tube, also filled with mercury. Their agreement was also within the limits which may be ascribed to errors of observation and of the measuring-apparatus.

The unit of this reproduction has found employment by many physicists, and very extensively in telegraph measurements, both in England and on the continent.

For the present and second reproduction, ten tubes were used—four of those of the previous determinations remaining unbroken, and six new ones.

The new tubes were selected after a rough calibrating, and ground off in lengths of a metre or less. They were then cleaned with concentrated sulphuric acid, rinsed with distilled water, dried by pumping dry air through them, and finally wiped by drawing through them a little lump of cotton wool attached to the end of a couple of spirally-twisted silk-covered wires.

The tubes were finally calibrated as follows: a drop of mercury was sucked into one end, its length not exceeding 40 millims. The tube was then layed upon a metre scale and the lengths (L) of the mercury thread measured, with aid of a small finely-divided scale and telescope, from one end to the other, at intervals of 20 millims.

The readings are collected in the following Table:—

TABLE I.

Place.	3.	5.	7.	8.	10.	11.	12.	13.	14.	15.
10	14·1	20·0	7·8	15·0	14·4	12·0	11·2	17·0	16·1
30	14·1	19·9	7·8	34·8	14·95	14·45	11·9	11·3	17·0	16·1
50	14·1	20·1	7·85	34·9	14·9	14·45	12·0	11·4	17·0	16·1
70	14·1	20·2	7·9	35·0	14·8	14·5	12·0	11·5	17·1	16·1
90	14·1	20·2	7·9	35·1	14·7	14·6	12·0	11·4	17·1	16·1
110	14·2	20·2	7·85	35·3	14·6	14·6	12·0	11·3	17·1	16·1
130	14·4	20·1	7·8	35·6	14·5	14·6	12·0	11·3	17·1	16·1
150	14·5	20·2	7·7	36·0	14·5	14·6	11·9	11·35	17·1	16·1
170	14·5	20·3	7·7	36·0	14·4	14·6	11·9	11·4	17·1	16·05
190	14·6	20·3	7·8	35·9	14·3	14·6	11·8	11·4	17·1	16·0
210	14·7	20·2	7·8	35·75	14·2	14·6	11·75	11·4	17·15	16·0
230	15·0	20·3	7·7	35·7	14·1	14·6	11·65	11·4	17·2	16·0
250	15·0	20·6	7·65	35·6	14·1	14·6	11·5	11·4	17·2	16·0
270	15·0	20·7	7·7	35·7	14·2	14·6	11·6	11·3	17·1	16·0
290	15·2	20·7	7·75	35·8	14·2	14·6	11·7	11·3	17·1	16·0
310	15·4	20·7	7·75	36·0	14·0	14·6	11·7	11·3	17·1	15·9
330	15·5	20·7	7·8	36·0	13·9	14·6	11·8	11·25	17·05	15·8
350	15·5	20·7	7·8	36·1	13·8	14·6	11·8	11·2	17·0	15·7
370	15·5	20·7	7·8	36·0	13·8	14·6	11·9	11·2	17·0	15·7

TABLE I. (*continued*).

Place.	3.	5.	7.	8.	10.	11.	12.	13.	14.	15.
390	15.5	20.7	7.9	35.8	13.8	14.6	11.9	11.2	17.0	15.7
410	15.5	20.8	7.9	35.6	13.9	14.6	12.0	11.3	17.0	15.6
430	15.6	21.0	7.95	35.4	13.9	14.6	12.0	11.3	17.1	15.5
450	15.6	21.2	7.95	35.0	13.8	14.6	12.0	11.4	17.2	15.45
470	15.7	21.2	7.95	35.0	13.7	14.6	12.0	11.5	17.2	15.4
490	15.7	21.1	7.95	35.2	13.7	14.6	12.0	11.55	17.2	15.4
510	15.7	21.0	7.95	35.3	13.9	14.6	12.05	11.6	17.1	15.2
530	15.8	21.0	7.9	35.5	14.0	14.6	12.05	11.6	17.1	15.3
550	15.8	21.0	7.9	35.6	14.0	14.6	12.0	11.6	17.1	15.4
570	15.8	21.0	7.8	35.7	13.9	14.6	12.0	11.6	17.1	15.5
590	15.8	20.9	7.8	35.8	13.9	14.6	12.0	11.5	17.1	15.5
610	15.8	20.8	7.8	35.9	13.8	14.5	12.0	11.5	17.1	15.6
630	15.8	21.0	7.75	35.9	13.7	14.6	12.0	11.4	17.1	15.6
650	15.8	21.0	7.7	36.0	13.7	14.6	12.0	11.3	17.1	15.5
670	15.9	21.3	7.65	36.05	13.6	14.6	12.0	11.3	17.1	15.5
690	15.9	21.4	7.6	36.1	13.6	14.6	12.0	11.3	17.0	15.5
710	16.0	21.2	7.6	36.15	13.7	14.6	12.0	11.4	17.0	15.5
730	16.0	21.1	7.6	36.15	13.7	14.6	11.95	11.3	17.0	15.3
750	16.0	20.9	7.55	36.05	13.8	14.5	11.9	11.3	17.0	15.2
770	16.0	20.8	7.5	36.0	13.9	14.45	12.0	11.2	17.0	15.1
790	16.0	20.8	7.5	36.0	14.0	14.4	12.0	11.2	17.0	15.1
810	16.1	20.8	7.5	36.0	14.1	14.4	11.8	11.2	17.0	15.0
830	16.4	20.7	7.5	35.9	14.1	14.2	11.7	11.2	17.0	15.0
850	16.7	20.7	7.5	35.8	14.1	14.2	11.6	11.1	17.0	15.0
870	16.8	20.7	7.5	35.8	14.0	14.2	11.55	11.1	17.0	15.1
890	16.8	20.7	7.5	36.0	14.0	14.2	11.5	11.1	17.0	15.1
910	16.8	20.7	7.5	36.2	14.1	14.2	11.65	11.1		
930	16.8	20.5	7.6	36.6	14.2	14.2	11.75	11.2		
950	16.9	20.5	7.6	37.0	14.3	14.25	11.9	11.2		
970	16.9	20.5	7.7	37.3	14.5	14.3	11.9	11.2		
990	16.9	20.5	7.8	14.4	14.25	12.0	11.2		

Readings with other lengths* were made in the same way at the places of greatest and least section, as shown by the preceding Table, and the coefficients (C) for correction for conicalness calculated from means of the values of *a* given by the different readings.

The formula employed is the same as developed by Dr. Siemens, and used in calculating the correction-coefficients in the first

* In the original determination the values of C were calculated a little too small, the result of employing a too long thread of mercury (*Pogg. Ann.* vol. cx. p. 8. Table I.), which has obviously the effect of hiding the smaller inequalities. The differences are, however, not considerable, as will be seen by comparing the numbers.

Tube.	C according to	
	Original determination.	Present reproduction.
3	1.002820	1.002775
5	1.000289	1.000393

determination, viz.

$$C = \frac{1 + \sqrt{a} + \frac{1}{\sqrt{a}}}{3},$$

in which

$$a = \frac{R^2}{r^2} = \frac{\text{max. } L}{\text{min. } L}.$$

The following Table gives the means of several readings with each length :—

TABLE II.

Number of tube.	L.		a.	Mean.	C.
	Max.	Min.			
3	millims.	millims.			
	16.9	14.1	1.1986	} 1.20017	1.002775
	25.0	20.8	1.2019		
30.6	25.5	1.2000			
5	20.0	18.8	1.0638	} 1.07073	1.000393
	21.4	19.9	1.0754		
	29.4	27.4	1.0730		
7	7.95	7.5	1.0600	} 1.05325	1.000224
	14.1	13.43	1.0499		
	22.92	21.82	1.0504		
	30.35	28.83	1.0527		
8	9.65	9.0	1.0722	} 1.07282	1.000412
	11.6	10.8	1.0741		
	14.65	13.65	1.0732		
10	37.3	34.8	1.0718	} 1.09850	1.000735
	15.0	13.6	1.1029		
	19.9	18.2	1.0934		
11	28.9	26.3	1.0989	} 1.02823	1.000065
	14.6	14.2	1.0282		
	15.4	15.0	1.0267		
12	24.2	23.5	1.0298	} 1.04525	1.000163
	12.05	11.5	1.0478		
	8.55	8.2	1.0427		
13	11.6	11.1	1.0451	} 1.04077	1.000133
	16.2	15.6	1.0385		
	29.5	28.4	1.0387		
14	17.2	17.0	1.0118	} 1.0119	1.000012
	21.9	21.6	1.0139		
	25.2	24.95	1.0100		
15	16.1	15.0	1.0733	} 1.07347	1.000419
	25.0	23.3	1.0730		
	29.0	27.0	1.0741		

The capacities of the tubes were ascertained by filling them with mercury, which was afterwards weighed.

In the operation of filling, care was taken to exclude air-bubbles.

An iron frame (A) was attached to one end of the tube D, as

is shown in fig. 1, in which B is a screw carrying a round plate E of polished iron on a ball-and-socket joint, F a piece of india-rubber pipe by which the glass tube is held fast in the frame, and *f, g* screws for putting the frame together. On the opposite end was fixed a rectangular bent piece of glass tube 20 millims. diameter, held tightly on the tube by means of an india-rubber stopper. The tube was placed horizontally on a table and filled very slowly. If no bubbles could be seen in the tubes, the plate E was pressed tightly by turning the screw B against the end.

The rectangular bent glass was then removed, and, leaving a small globe of mercury protruding from the opening of the tube, the whole was hung up with a delicate thermometer for an hour in the open air, protected by screens from radiant heat.

The temperature was then read off, and the protruding hemisphere of mercury removed by pressing a plate of ground glass on the top of the tube.

The mercury was allowed to run very slowly out into a little porcelain crucible, so as to leave no globules behind, and weighed.

In the following Table, column 2 gives the temperatures; column 3 the weights (in air of mean temperature) in grammes, after deducting the weight of the crucible; column 4 the same reduced for expansion of glass and mercury and for vacuum to 0° C.; column 6 the lengths, and column 7 the average inner radius.

The formula by means of which the weights were reduced is

$$P_0 = P_t \{ 1 + (\gamma - g)t \} 1.00009,$$

and is correct within the limits of their measurements.

γ = coefficient of cubic expansion of mercury, per degree Centigrade = 0.00018018.

g = the same of glass = 0.00002586.

I have taken (σ) the specific gravity of mercury at 0°C. = 13.557. The constant 1.0000908 reduces the weights for vacuum.

The lengths of the tubes were measured on the brass metre scale of our resistance bridge.

Fig. 1.

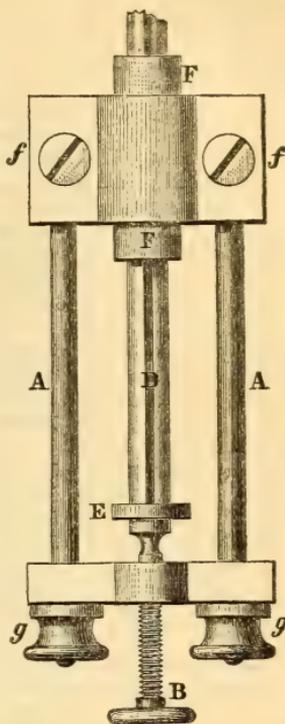


TABLE III.

No.	<i>t</i> .	P_t .	P_0 .	Mean.	<i>l</i> .	<i>r</i> .
3	2.3	24.4391	24.4500	} 24.44855	millims. 1000	millim. 0.7576
	2.75	24.4363	24.4489			
	3.0	24.4346	24.4481			
	6.5	24.4205	24.4472			
5	-2.1	70.0200	70.0037	} 70.00766	1000	1.2821
	-1.5	70.0160	70.0062			
	3.2	69.9727	70.0136			
	3.4	69.9655	70.0086			
	18.6	69.7995	70.0062			
7	0.7	7.0698	7.0712	} 7.07160	1000	0.4075
	3.0	7.0680	7.0719			
	3.2	7.0679	7.0720			
	15.9	7.0533	7.0713			
8	-3.7	5.2156	5.2131	} 5.21345	1000	0.3499
	-3.5	5.2157	5.2134			
	-2.9	5.2148	5.2129			
	2.75	5.2117	5.2144			
10	-4.7	8.8089	8.8033	} 8.80343	1000	0.4546
	-4.2	8.8083	8.8034			
	-3.7	8.8078	8.8036			
11	-3.6	3.0960	3.0946	} 3.09470	1000	0.2696
	0.3	3.0943	3.0947			
	0.3	3.0943	3.0947			
	0.4	3.0943	3.0948			
12	-3.6	8.21165	8.2078	} 8.20720	1000	0.4390
	-3.15	8.2100	8.2067			
	3.45	8.2113	8.2077			
	20.5	8.1800	8.2066			
13	5.55	8.2757	8.2835	} 8.28398	999.8	0.4411
	6.1	8.2760	8.2845			
	6.6	8.2750	8.2842			
	6.9	8.2743	8.2839			
	7.1	8.2740	8.2838			
14	2.9	7.8848	7.8890	} 7.88900	908.8	0.4514
	5.4	7.8816	7.8889			
	5.7	7.8815	7.8891			
	5.9	7.8811	7.8890			
15	6.5	7.4881	7.4963	} 7.4962	918.05	0.4379
	6.6	7.4879	7.4962			
	7.1	7.4874	7.4962			
	7.3	7.4870	7.4961			

Substituting the values given in the foregoing Tables for the respective tubes in the formula

$$W = \frac{1000l^2\sigma}{P_0} \cdot C,$$

we obtain the respective resistances* at 0° C. in millimetres, that is to say, the resistance of a cubic millimetre of mercury at 0° C.

TABLE IV.

No.	millims.
3	556·051
5	193·726
7	1917·54
8	2601·46
10	1541·10
11	4381·00
12	1652·11
13	1636·10
14	1419·33
15	1524·88

But, in practice, the resistances with which the tubes enter into the measuring-apparatus are *greater* than their calculated values by so much as is due to the passage of the current from their openings into the cups of mercury for the connecting wires.

This resistance can, without sensible error, be considered as the resistance of a hemispherical shell whose inner radius is equal to r , the inner radius of the tube, and whose outer radius is infinitely great in comparison with r . The resistance dy of a shell of the thickness dx and radius x is expressed by

$$dy = \frac{dx}{2x^2\pi},$$

whence

$$y = \int_r^\infty \frac{dx}{2x^2\pi} = \frac{1}{2r\pi} = \frac{r}{2r^2\pi}.$$

It therefore amounts to increasing the length of each of the tubes by the length of its radius †.

* An idea of the exactness of this method of reproduction will be best obtained by a direct comparison of the calculated values according to the two previous and present determinations.

Tube.	Original determination ¹ .	First reproduction ² .	Present reproduction ³ .
3	555·87	555·99	556·05
5	193·56	193·73	193·73
7	1917·32	1917·54
8	2600·57	2601·46

¹ Poggendorff's *Annalen*, vol. cx. p. 9.

² *Ibid.* vol. cxiii. p. 95.

³ The above Table.

† The value of the resistance y is a little too great, from the supposition that the radii are in the proportion $r : \infty$, and a little too small, from the

The real resistances (W_1) of the tubes at 0° C. are therefore represented by

$$W_1 = W + 2y = W \left(1 + \frac{r}{l} \right).$$

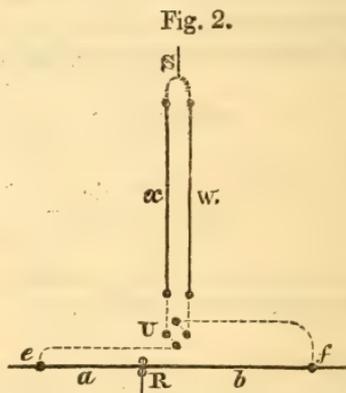
TABLE V.

No.	millims.
3	556.47
5	193.97
7	1918.32
8	2602.37
10	1541.80
11	4382.18
12	1652.84
13	1636.82
14	1420.04
15	1525.61

It now remained to compare the tubes as resistances with each other, in order to find in how far the calculated values would then agree.

This was done by employing successively two of the tubes as the two branches x and W_1 of a Wheatstone's resistance bridge,

the opposite branches being formed by a fine tightly drawn platinum wire a metre long, divided into the two parts a and b by the platinum contact-rollers R . A Daniell's cell and key were inserted between the union (S) of the two tubes and the contact-rollers, and a mirror galvanoscope between the ends e and f of the platinum wire. The lengths of a and b were read off by means of a nonius carried by the block of the contact-rollers along a metre scale at the back of the platinum wire*.



surface being introduced into the calculation as hemispherical instead of plane.

This correction was omitted in the two previous determinations as being unimportant.

* A detailed description, with a drawing of this instrument, may be found in Poggendorff's *Annalen*, vol. cx. p. 9. The common error, arising from resistance to passage of the current from the ends of the wire to the clamps, was lessened by employing a wire of nearly 20 metres resistance. The 0

In these measurements care was taken to fill the two tubes which were to be compared from the same bottle of mercury, and to keep them during the measurement at the same temperature. For this purpose a zinc trough was provided, 2 yards long and $2\frac{1}{2}$ inches broad and deep, held in a wooden box, with sawdust tightly packed between the zinc and the wood. On the bottom of the trough, from one end to about three-fourths of its length, were cemented a series of half a dozen glass tubes, and a water-tight partition was put just over their ends so as to divide the trough into two unequal parts, whose only connexion with each other was through the flooring of glass tubes. The trough was kept three-fourths full of water, which could be made to circulate by spooning it from one division into the other, and letting it run back again through the glass tubes at the bottom.

The resistance tubes were furnished at their ends with rectangular bent pieces of wide glass pipe, such as was used in filling them with mercury to ascertain their capacity, held fast by india-rubber stoppers. They were filled very slowly with mercury, and laid into the water-trough as closely as possible, side by side. The contacts between the tubes and with the measuring apparatus were made by copper wires. The whole was protected by screens of vulcanite against rays of heat from the lamps and stoves of the laboratory. The temperature of the water in the trough varied during a single measurement not more than 1° C., and during the whole series between 12° C. and 16° C., but did not fall below, nor rise beyond these limits. During each measurement, the water was kept circulating in the way described, and equality of temperature of the platinum wire was secured by uninterruptedly fanning it.

By these arrangements were gained some important advantages: for example, all reductions for temperature and their concomitant errors were dispensed with, the thermo-currents (the unavoidable result of the employment of ice) in the systems were avoided, and the mercury was not required to be more than ordinarily pure.

When the currents of the system were balanced (that is to say, when no effect was perceptible on the galvanometer by closing the battery circuit), if the contact-rollers were moved 0.1 millim. out of their place, the galvanometer mirror showed a deflection represented by 5 millims. of the reflected scale passing by the fibre of the telescope.

The copper wire used in making the connexions between the different parts was above 8 millims. diameter. It was softened and the ends amalgamated. The same length was inserted in the circuit of each tube (shown in the figure by dotted lines).

of the nonius was placed on the 500 line, when, with $x=W_1$, no deflection was observed by inverting these resistances. The wire had been drawn with great care through stone, and its conicalness was entirely inappreciable.

The proportion of the balanced system was therefore

$$\frac{a}{b} = \frac{A+w}{W_1+w}$$

in which equation W_1 represents the calculated real resistance of the tube with which x is compared, and w the resistance of the connecting wires on each side = 0.37 millim. The effect of variations of temperature on the connexions may, without sensible error, be disregarded.

A mercury commutator (U) between the tubes and the measuring-wire facilitated the inversion of the former for reading off the values of a and b from the same end.

The tubes were refilled between each measurement.

The following triangular Table gives the readings of the nonius and the metre scale when the tubes at the heads of the columns were compared with those in the first column:—

TABLE VI.—Readings of the Bridge Wire.

No. of tube.	7															
	a.	b.														
	424.4	575.8	8													
	424.4	575.75														
	424.4	575.7														
Mean.	424.4	575.75	a.	b.												
10	554.65	445.75	627.8	371.9	10											
	554.6	445.75	627.8	372.0												
	554.65	445.75	627.8	371.95												
Mean.	554.63	445.75	627.8	371.95	a.	b.										
12	537.05	462.8	611.55	388.4	482.5	517.35	12									
	537.05	462.7	611.55	388.5	482.55	517.40										
	537.1	462.75	611.55	388.45	482.55	517.35										
Mean.	537.07	462.75	611.55	388.45	482.53	517.37	a.	b.								
13	539.4	460.2	613.85	386.15	484.8	514.75	502.3	497.5	13							
	539.4	460.25	613.8	386.1	484.7	514.7	502.3	497.5								
	539.4	460.15	613.85	386.15	484.75	514.7	502.3	497.5								
Mean.	539.4	460.2	613.83	386.13	484.75	514.72	502.3	497.5	a.	b.						
14	574.65	425.4	646.45	352.85	520.6	479.5	537.8	461.95	535.4	464.5	14					
	574.65	425.4	646.45	352.85	520.65	479.5	537.85	462.0	535.5	464.4						
	574.65	425.4	520.65	479.4	537.75	462.05	535.45	464.5						
Mean.	574.65	425.4	646.45	352.85	520.63	479.47	537.8	462.0	535.45	464.47	a.	b.				
15	556.8	442.8	630.4	369.45	502.55	497.35	520.1	479.9	517.65	482.35	482.0	517.9				
	556.9	442.8	630.45	369.45	502.45	497.30	520.0	479.9	517.70	482.3	482.1	517.9				
	556.9	442.9	502.55	497.35	519.8	480.0	517.7	482.2	482.1	518.0				
Mean.	556.87	442.83	630.42	369.45	502.52	497.33	519.97	479.93	517.68	482.28	482.07	517.93				

The resistances, calculated by means of the formula

$$x = \frac{a}{b} (W_1 + w) - w,$$

are collected in the following Table:—

TABLE VII.

Tubes.	7.	8.	10.	12.	13.	14.	15.
	millims.						
7	2602·56	1541·66	1652·81	1636·60	1419·99	1525·39
8	1918·18	1541·67	1652·86	1636·88	1420·28	1524·93
10	1918·49	2602·60	1653·15	1637·15	1419·88	1525·87
12	1918·55	2602·33	1541·51	1637·04	1419·83	1525·54
13	1918·58	2602·27	1541·49	1652·62	1419·79	1524·87
14	1918·39	2601·94	1541·97	1653·09	1637·11	1525·70
15	1918·59	2603·52	1541·53	1652·92	1637·62	1419·96
Mean ...	1918·43	2602·54	1541·64	1652·91	1637·07	1419·96	1525·38
Calcul....	1918·32	2602·37	1541·80	1652·84	1636·82	1420·04	1525·61

The vertical columns contain the resistances of the various tubes compared with those which stand in the first column.

At the bottom I have added the means of the observed resistances, and set under them the calculated values.

The differences between the single observations are not more than can be attributed to errors of observation, in some cases increased, and in others diminished by the individual errors of the measuring-apparatus.

By reason of their very different values, the tubes 3, 5, and 11 were compared separately. The results are as follows:—

TABLE VIII.

Tubes.	a.	b.	W ₂ .	Resistance.	
				Observed.	Calculated.
3	509·2	490·8			
	509·0	490·8			
	509·1	490·8			
Mean ...	509·1	490·8	536·32	556·33	556·47
5	415·25	584·55			
	415·2	584·55			
	415·20	584·55			
Mean ...	415·22	584·55	273·11	193·89	193·97
11	493·95	505·9			
	493·85	505·85			
	493·90	505·95			
Mean ...	493·90	505·90	4487·45	4381·00	4382·18

The resistances (W_2) were produced by means of combinations.

For tube 3, resistances of 10, 12, and 13 parallel; for tube 5, resistances of 3, 10, 12, and 13 parallel; and for tube 11, resistances of 10, 14, and 15 one after the other.

Such resistances as I have described are, however, incommodious on account of their length and the difficulty of keeping them, without special apparatus, at a constant temperature during the measurements. Dr. Siemens's method of copying them in the form of glass spirals, however, removes all difficulty on this head, particularly as it is evident they may be copied with great exactness. The only modification which it was thought advisable to introduce, was in double-winding the spirals to prevent induction currents, and in making the cups or pieces of wide glass tube at their ends separate, to be cemented on or fixed with thick india-rubber rings, instead of being fused on as was done previously. The spirals are easier made, and are not so easily broken. Copies of the normals have been made approximating to 0.1, 0.2, 0.5, 1.0, and so on, up to 10 metres' resistance, by direct comparison with combinations of the normal tubes; and beyond 10 and up to 50 metres, two or three spirals of nearly equal resistances were first compared with each other, and then their combined (parallel) resistance with a combination of normals of about the same value.

By grinding off the ends, they can, of course, be made exact multiples of the unit.

The subsequent process of copying these spirals in metal wire for resistance-scales is obvious.

Following this method, every electrician may inexpensively and with little trouble make himself a standard measure. It is of course needless to determine the capacities of ten tubes, as was done in this reproduction, the only purpose in view in the multiplicity being to ascertain the maximum disagreement between the tubes when compared as resistance-measures. This maximum has been shown to be within the amount of errors of observation in the comparison. The mercury unit has therefore been produced in Dr. Siemens's laboratory twenty-one times—six times in the first determination, five times in the second, and ten times in the present. And, allowing for the unfortunate misrepresentation of the measure by individual errors of the measuring-apparatus used in the comparison of the first tubes, the agreement between them all is greater than could be *guaranteed* between any two single electrical measurements with different measuring-apparatus.

From the foregoing results it follows that, by the method of direct production proposed by Dr. Siemens, much greater exactness has been attained than by means of any of the other methods of determination or copying.

The copies of an arbitrarily chosen unit of resistance, subject to the errors incidental to copying, must be less exact than the resistances directly calculated according to the definition of Dr. Siemens. In addition to this, we have no certainty that coils of wire do not change their resistance in course of time.

I believe it is generally the disposition of English physicists to accept as common unit of resistance the absolute unit of the beautiful system of Weber. The advantages of this unit are, however, limited to facilitating the solution of some purely scientific problems, and may be completely reached, without materially increasing the difficulties, by a single careful determination of the mercury unit in absolute measure; and its general adoption by electricians would only be serviceable in so far as it would be the means of introducing a common measure. But, to its disadvantage, the reproduction of the absolute unit is attended with expense and time which few physicists would be in a position to sacrifice to it. Then, again, its determination depends upon the measurements of several forces, of which each is burthened with comparatively great sources of error. Attending each subordinate measurement, in fact, the possible error is greater than that of the simple comparison of two nearly equal resistances. It is evident, therefore, that the determination of resistance in absolute units is not adapted for the production of normal coils. The differences between the values of the mercury unit, according to the determinations of absolute measure by Weber and Thomson, entirely confirm this opinion; for we can look for the causes of differences between the results of two of the most world-renowned physicists only in the system, not for a moment in the manipulation.

Supposing, however, the absolute unit to be reproducible with certainty within 0.1 per cent., and supposing that the electrician overcame the inconvenience of the mass of figures required

to express a $\frac{\text{millimetre}}{\text{second}}$ -unit by employing as unit of resistance

the same multiplied with 10^{10} (which would bring it to about the value of one mercury unit)*, recourse must still be had to the arbitrary system as a ready means of reproduction. A second definition of the 10^{10} absolute unit, as "the resistance of a prism of mercury a metre long and 1.0257 square millimetre section at 0° C.," would inevitably sooner or later creep into general use.

The necessity of accepting mercury as unit of conducting-

* In computing resistances of insulation, a still more capacious unit is necessary; for example, $10^{16} \frac{\text{millimetre}}{\text{second}}$, or a kilo-kilometre (mercury) unit. The mercury unit at 0° C., according to Weber's last determination (*Zur Galvanometrie*, Göttingen, 1862, p. 58), is equal to about 10,257,000,000 absolute units.

power, since its molecular condition in common temperatures renders it alone capable to take that position, is an argument in favour of its adoption also as unit of resistance. It is, indeed, in practice almost a necessity to define the common resistance unit as the resistance of a body of the same material as is chosen for unit of conductibility, by which means calculations with it are facilitated, and a perfect notion of the measure secured.

An absolute unit might be adopted as measure of electromotive force even were a defined body of mercury to hold the place of resistance unit: it must, however, be confessed that the system of measurement would lose in consistency and conformity to the purpose; besides which the determination of electromotive force in absolute measure is burthened with material sources of error, and is therefore opposed by the same objections which are tenable against the absolute resistance unit.

Dr. Siemens has lately given his attention to the construction of a reproducible unit of electromotive force, capable of being exactly defined, and commodious, and will shortly publish the results of his endeavours.

XXII. *On the Pressure Cavities in Topaz, Beryl, and Diamond, and their bearing on Geological Theories.* By Sir DAVID BREWSTER, K.H., F.R.S.*

IN the years 1823 and 1826 I communicated to this Society two papers "On the Existence of Two New Fluids in the Cavities of Precious Stones and other Minerals." These two fluids † were generally found together in the same cavity, though sometimes the cavities were occupied by only one of them. They were perfectly transparent and immiscible. The denser of the two occupied the angles of the cavities, or the necks, or narrow passages, or canals which united two or more larger cavities; while the rarer fluid floated, as it were, on the other in deep cavities, or filled the body of shallower ones, with the exception of a circular vacuity, which diminished and disappeared with the slightest increase of temperature, or enlarged itself and disappeared in consequence of the fluid being converted into vapour.

The denser of these fluids does not appear to expand more than oil or water by the application of heat; but the other is *twenty-one* times more expansible than water. It evaporates at temperatures from 74° to 84°. The vacuity in it disappears by the heat of the mouth or of the hand; and it returns to its former state by a violent effervescence, producing a number of minute

* From the Transactions of the Royal Society of Edinburgh, vol. xxiii. part 1. Communicated by the Author.

† The American and French mineralogists have given the name of *Brewstoline* to the volatile, and *Cryptoline* to the dense fluid.

vacuities, which finally unite in one. The refractive power of the expansible fluid varies from 1.1311 to 1.2106, while that of the denser fluid is 1.2946, which is very much less than that of water. From the few experiments which I was able to make on these fluids when taken out of the cavities, it has been inferred that they are hydrocarbons.

The distribution of these cavities, in the specimens which contain them, is a subject of peculiar interest. They are often found singly, and of different sizes, at different depths in the mineral; but they most frequently occur in strata, and of such different magnitudes that the two fluids are distinctly seen in the largest, while the rest gradually diminish till they disappear in black points which the microscope can hardly descry. Three or four strata nearly parallel to one another, and with cavities of different sizes, rarely occur. In general the strata lie in planes frequently intersecting one another, and having no connexion with the primitive or secondary planes of the crystal. In some specimens the planes of the strata are curved, and in rare cases the sections of these planes are curves of contrary flexure.

In 1844 I was led to re-examine several hundred specimens of topaz with a more perfect microscope and a fine polarizing apparatus, with the view of ascertaining the nature and properties of certain crystalline deposits which I had noticed, and to which I had referred in my earliest observations*. In these new researches, the results of which were published in two papers in the 'Transactions' of this Society for 1845, I discovered two new classes of phenomena which had escaped the notice of preceding observers, and which threw much light on the formation of the minerals in which they were exhibited.

In many specimens of topaz from Brazil and New Holland, I discovered numerous cavities, filled with crystals of various primitive forms, and with different physical properties. These crystals are either fixed or moveable. Some of the fixed crystals are beautifully crystallized, and have their axes of double refraction coincident with those of the specimen which contains them. In some cavities there is only *one* crystal, in many *two*, *three*, and *four*, and in a great number the crystals actually fill the cavities to such a degree that the circular vacuity in the fluid cannot take its natural shape, and can often be scarcely recognized among the jostling crystals.

Upon the application of heat to these crystals, some of them gradually lost their angles, and melted slowly, till not a trace of them was visible. Others melted with greater difficulty; and some resisted the most powerful heat I could apply. The

* See Edinburgh Transactions, vol. x. p. 21, note, and plate 1. fig. 10, plate 2. figs. 20, 21; p. 419, note, and plate 19. fig. 4.

crystals which melted easily were quickly reproduced, sometimes reappearing in a more perfect form, but frequently running into amorphous shapes or granular crystallizations. While some of the crystals were resuming a tabular form, their tints, under the polarizing microscope, gradually rose in the scale of colours as their thickness increased; and when there happened to be numerous crystals in the specimen, the whole field of the microscope was filled with brilliant portions of light which they polarized.

While making these observations, crystals of a different kind presented themselves to me when the specimens which contained them were exposed to polarized light. These crystals were imbedded in the topaz; and as their axes of double refraction were not coincident with those of the mineral, they were seen in the obscure field of the microscope, brilliant with all the colours of polarized light. They often polarize five or six orders of colours; and in general they have beautiful crystalline forms, which are visible in the microscope even in common light. In some specimens of Brazil topaz, the imbedded crystals occur in groups of singular beauty, consisting of prisms and hexagonal plates, connected apparently by filaments of opaque matter. In all these specimens the crystals had a distinct outline, whether they were examined in common or in polarized light; but I have met with topazes in which the imbedded crystals had no visible outline in common light, and which never could have been detected but by the polarizing microscope. In one of these an amorphous crystal, nearly spherical, lay in a crowded group of small fluid-cavities, none of which had entered it—a proof that the cavities had been formed in the topaz when soft, and when it imprisoned the previously indurated crystal.

The other class of phenomena to which I have referred is of a still more remarkable nature, and has a more direct bearing on geological theories. About thirty years ago I communicated to the Geological Society the singular fact that I had found in a diamond a small cavity, round which four luminous sectors were seen in polarized light—a phenomenon which clearly proved that the diamond, when in a soft state, had been compressed by an elastic force proceeding from the cavity. This inference countenances the opinion that the diamond was of vegetable origin; and as this gem was a sort of outlaw in the mineral world, the idea that it had once been in a plastic state, like amber and other gums, and susceptible of compression, did not startle the mineralogists who believed in the ordinary doctrine of crystallization. The insulated fact, therefore, and the probable inference from it, excited no notice; and it was not till the same phenomenon had been observed more frequently in the

diamond, and in other minerals supposed to be of aqueous formation, that its geological importance was likely to be acknowledged.

In the Koh-i-noor diamond, which the Prince Consort kindly permitted me to examine in 1852, I found three black specks, scarcely visible to the eye, but which the microscope showed to be irregular cavities, surrounded with sectors of polarized light. In the two smaller diamonds which accompanied the Koh-i-noor, there were also several cavities surrounded with luminous sectors, and the same polarizing structure, which indicated the operation of compressing and dilating forces*. In order to obtain more information on this subject, I examined nearly fifty diamonds lent me by Messrs Hunt and Roskill, and in almost all of them I found numbers of cavities, of the most singular forms, round which the substance of the stone had been compressed and altered in a remarkable manner. The shapes of the cavities sometimes resembled those of insects and lobsters, and the streaks and patches of colour in polarized light were of the most variegated kind. In examining a large number of diamonds which adorn some of the oriental objects in the East India Company's Museum, I found that all these stones contained large cavities, and were coarse or flawed diamonds, which could not be cut into brilliants or used in rings or other ornaments. It seems, indeed, to be a general truth that there are comparatively few diamonds without cavities and flaws, and that this mineral is a fouler stone than any other used in jewellery. Some diamonds, indeed, derive their black colour entirely from the number of cavities which they contain, and which will not permit any light to pass between them.

Having found in diamond so many *Pressure Cavities*, as we may call them, round which the substance of the stone is compressed, I had some expectation of finding them in other minerals; and upon re-examining the numerous plates of topaz in my possession, I succeeded in discovering several under such remarkable circumstances that I submitted a description and drawings of them to this Society in 1845†. In searching for this phenomenon with the polarizing microscope, we first observe four sectors of depolarized light; and if the magnifying power is sufficient, we shall find in the centre of the black cross that separates the sectors a small opaque speck, which is the cavity or seat of the compressing force. This cavity is frequently of a rhomboidal form, and often only the 3000th or 4000th of an inch in diameter. It is always opaque, as if the elastic substance which

* In 1820 I discovered similar cavities in amber, &c. See *Edinb. Phil. Journ.* vol. ii. p. 334.

† See *Edinb. Trans.* 1845, vol. viii. p. 157; or *Journal de Physique*, 1846, vol. lxxxii. p. 367.

it contained had collapsed into a black powder; and I have met with only one cavity in which there was a speck of light in its centre. The polarized tint in the luminous sectors varies from the faintest *blue* to the *white* of the first order. In most cases the elastic force has spent itself in the compression of the topaz, the cavity remaining entire, and without any apparent fissure by which a gas or a fluid could escape. I have discovered, however, other cavities, and these generally of a larger size, in which the sides have been rent by the elastic force, and fissures, from one to six in number, propagated to a small distance around them. These fissures have modified the doubly refracting structure produced by compression, but the gas or fluid which has escaped has left no solid matter on the faces of fracture.

Soon after the publication of these results, I discovered still more remarkable cavities in a specimen of beryl brought from India by the Marchioness of Tweeddale, who was so kind as to present it to me. In cutting the crystal, Mr. Sanderson found that one end of it was foul, and produced a luminous ring round a candle. This ring, similar to the rings seen in certain specimens of Iceland spar, was produced by long and irregularly tubular cavities parallel to the sides of the hexagonal prism. As the tubes had been cut across by the lapidary, their contents had escaped; but whatever the contents were, whether fluid or gaseous, they had compressed the beryl, and produced the four luminous sectors around each cavity. This aggregation of luminous sectors produced a mass of depolarized light, which completely effaced the black cross of the uniaxal system of rings exhibited by the mineral. Different degrees of compression were produced by cavities of different sizes; but the resulting tint was generally a *white* of the first order, rising in some cases to a *yellow* of the same order.

Such is a brief notice of the fluid- and pressure cavities which exist in minerals, and which have a very obvious bearing on geological theories. Some of these facts have been upwards of forty years before the public*, and, along with others more recently discovered, have been widely circulated in British and foreign journals; and yet none of our geologists have made the slightest reference to them, either as difficulties to be explained, or arguments to be advanced in support of their own views.

In 1822 Sir H. Davy, when he was acquainted only with the existence in minerals of water, petroleum, and gas, did not hesitate to regard such facts as "seeming to afford a decisive argument in favour of the igneous theory of crystalline rocks"†; and in my paper of 1826 I was driven to the conclusion "that

* Edinburgh Philosophical Journal, vol. ii. p. 334, 1820.

† Philosophical Transactions, 1822, p. 367.

the cavities containing the two new fluids were formed by highly elastic substances, when the mineral itself has been either in a state of fusion or rendered soft by heat." At this time I was acquainted only with the two new fluids, and some of their chemical and physical properties; but when I had studied their arrangement in strata, this opinion acquired additional weight. Had these cavities been arranged in planes parallel to the primitive or secondary faces of the crystal, some argument might be urged in favour of their aqueous formation; but when it was found that the strata of cavities traversed the crystal in all possible directions, that they were bent also into curves of contrary flexure, and that even individual cavities had a curvilinear shape, it was impossible to resist the conclusion that the cavities were formed, and thus capriciously distributed, when the substance of the crystal was in a soft or plastic state. This conclusion derives additional strength from the fact that the water-cavities in crystals deposited from an aqueous solution are never thus arranged.

The discovery of pressure cavities in topaz and diamond may be considered as completing the evidence for the igneous origin of these minerals, and of the rocks which contain them. We know that gas in a state of compression exists in minerals. In the pressure cavities we have not only the seat of an elastic force, but its direct action upon the substance of the crystal. Though of equal density throughout, as is proved by the equality of its polarized tints, the crystal has its density increased round the pressure cavity, the density being a maximum close to the cavity. Such a structure is impossible in crystals formed by aqueous deposition; and hence there is not a single example of a pressure cavity in any of them. They exist, however, in amber and in glass, substances that have once been in a plastic state; and I have produced them artificially by compressing a solution of gum-arabic between two plates of glass so as to include some bubbles of air. The air in these cavities, being exposed to changes of temperature, compresses the circumjacent gum, and gives it that variation of density which produces four luminous sectors in polarized light, exactly of the same character as those which are found in topaz and diamond.

The existence of crystals of different physical properties in the cavities of minerals, and of imbedded crystals either shooting through their mass, or occurring in groups, or lying singly with their optical axes in every direction, admit of no other explanation than that which is afforded by supposing the surrounding mineral to have been in a state of fusion, and to have either contained the elements of the imbedded crystals, or to have surrounded them when previously formed.

Although, as I have already stated, no British geologist has seen the importance of the preceding facts, and their direct bearing on geological theories, yet they have been recently referred to*, and their value fully appreciated, by French geologists. In a discussion with M. Elie de Beaumont on the formation of mineral veins, M. Fournet†, the distinguished Professor of Geology at Lyons, has given a full and interesting account of this class of phenomena, and has adduced them to prove that mineral veins are formed by the injection of mineral matters in the state of fusion. In opposition to this argument, M. Elie de Beaumont makes the following observations:—"It is difficult," says he, "to admit that crystals of quartz containing two oily fluids, one of which is volatile at the temperature of 81° Fahr., have crystallized in a bath of quartz in fusion. But quartz forms part of the gangues of the greater number of veins, and quartz with fluid-cavities is far from being a rarity"‡. M. Fournet§ has, we think, removed this difficulty; but, without entering into the question as one of geology, we may safely assert that difficulties attaching to any theory are not arguments against it, especially if there are only two theories, and if equal difficulties attach to them both. We are so utterly unacquainted with the conditions under which the primitive rocks were formed, with the temperatures which prevailed at their formation, and with the pressures to which they must have been subject, that we are not entitled to charge any theory with difficulties which have their origin in our own ignorance, or in the very nature of the subject. We may never understand how the cavities in topaz have such singular and complex forms as those which I have described and delineated,—how these cavities should contain in one specimen two immiscible fluids, the one dense and the other volatile, and in another specimen various crystals of different primitive forms and physical properties. We may never understand how a series of these cavities could have arranged themselves in lines now straight and parallel, now curved and concentric, and now radiating from a centre; or how strata of these cavities could traverse the topaz in all directions with surfaces of single or double curvature. We may not be able to explain the special difficulty started by M. Elie de Beaumont; and yet it is absolutely certain that an elastic force, emanating from a pressure cavity, could not have compressed the topaz which surrounded it, unless the mineral had been in a soft and plastic state, or in the state of fusion.

* Daubrée, *Etudes sur le Métamorphisme*, 1860, p. 36.

† *Comptes Rendus*, &c., vol. li. p. 42; vol. liii. pp. 83, 610. And Fournet, *Géologie Lyonnaise*. Lyons, 1861, pp. 533, 715.

‡ *Comptes Rendus*, &c., July 15, 1861, vol. liii. p. 83, note.

§ *Géologie Lyonnaise*, p. 536.

XXIII. *Note on a Theorem relating to a Triangle, Line, and Conic.*

By A. CAYLEY, *Esq.**

I FIND, among my papers headed "Generalization of a Theorem of Steiner's," an investigation leading to the following theorem, viz. :—

Consider a triangle, a line, and a conic; with each vertex of the triangle join the point of intersection of the line with the polar of the same vertex in regard to the conic; in order that the three joining lines may meet in a point, the line must be a tangent to a curve of the third class; if, however, the conic break up into a pair of lines, or in a certain other case, the curve of the third class will break up into a point, and a conic inscribed in the triangle.

Let the equations of the sides of the triangle be

$$x=0, \quad y=0, \quad z=0,$$

the equation of the conic

$$(a, b, c, f, g, h)(x, y, z)^2=0,$$

and that of the line

$$\lambda x + \mu y + \nu z = 0;$$

then the polar of the vertex ($y=0, z=0$) has for its equation

$$ax + hy + gz = 0;$$

it therefore meets the line $\lambda x + \mu y + \nu z = 0$ in the point

$$x : y : z = h\nu - g\mu : g\lambda - a\nu : a\mu - h\lambda,$$

and the equation of the line joining this point with the vertex ($y=0, z=0$) is $(a\mu - h\lambda)y = (g\lambda - a\nu)z$. And the equations of the three joining lines therefore are

$$(a\mu - h\lambda)y = (g\lambda - a\nu)z,$$

$$(b\nu - f\mu)z = (h\mu - b\lambda)x,$$

$$(c\lambda - g\nu)x = (f\nu - c\mu)y,$$

lines which will meet in a point if

$$(a\mu - h\lambda)(b\nu - f\mu)(c\lambda - g\nu) - (g\lambda - a\nu)(h\mu - b\lambda)(f\nu - c\mu) = 0$$

or, multiplying out and putting as usual

$$K = abc - af^2 - bg^2 - ch^2 + 2fgh,$$

$$A = bc - f^2 \text{ \&c.},$$

if

$$\left. \begin{aligned} &2(abc - fgh)\lambda\mu\nu \\ &+ a\mathfrak{C}\mu\nu^2 + a\mathfrak{H}\mu^2\nu \\ &+ b\mathfrak{H}\nu\lambda^2 + b\mathfrak{F}\nu^2\lambda \\ &+ c\mathfrak{F}\lambda\mu^2 + c\mathfrak{C}\lambda^2\mu \end{aligned} \right\} = 0,$$

that is, the line must touch a curve of the third class.

* Communicated by the Author.

If this equation break up into factors, the form must be

$$(\alpha\lambda + \beta\mu + \gamma\nu)(A\mu\nu + B\nu\lambda + C\lambda\mu) = 0;$$

that is, we must have

$$A\alpha + B\beta + C\gamma = 2(abc - fgh),$$

$$B\alpha = b\mathfrak{H}, \quad C\alpha = c\mathfrak{G},$$

$$C\beta = c\mathfrak{F}, \quad A\beta = a\mathfrak{H},$$

$$A\gamma = a\mathfrak{G}, \quad B\gamma = b\mathfrak{F};$$

and the last six equations give without difficulty

$$A = \frac{ka}{\mathfrak{F}}, \quad \alpha = \frac{1}{k} \mathfrak{G}\mathfrak{H},$$

$$B = \frac{kb}{\mathfrak{G}}, \quad \beta = \frac{1}{k} \mathfrak{H}\mathfrak{F},$$

$$C = \frac{kc}{\mathfrak{H}}, \quad \gamma = \frac{1}{k} \mathfrak{F}\mathfrak{G},$$

where k is arbitrary; the first equation then gives

$$\frac{a\mathfrak{G}\mathfrak{H}}{\mathfrak{F}} + \frac{b\mathfrak{H}\mathfrak{F}}{\mathfrak{G}} + \frac{c\mathfrak{F}\mathfrak{G}}{\mathfrak{H}} = 2(abc - fgh);$$

or, reducing by the equations $\mathfrak{G}\mathfrak{H} = \mathfrak{A}\mathfrak{F} + ak$ &c., this is

$$\mathfrak{A}a = \mathfrak{B}b + \mathfrak{C}c - 2abc + 2fgh + \left(\frac{a^2}{\mathfrak{F}} + \frac{b^2}{\mathfrak{G}} + \frac{c^2}{\mathfrak{H}}\right)K = 0;$$

which, substituting for \mathfrak{A} , \mathfrak{B} , \mathfrak{C} their values, becomes

$$K \left(1 + \frac{a^2}{\mathfrak{F}} + \frac{b^2}{\mathfrak{G}} + \frac{c^2}{\mathfrak{H}}\right) = 0.$$

Hence if $K=0$, that is, if the conic break up into a pair of lines, or if

$$1 + \frac{a^2}{\mathfrak{F}} + \frac{b^2}{\mathfrak{G}} + \frac{c^2}{\mathfrak{H}} = 0,$$

in either case the equation of the curve of the third class becomes

$$\left(\frac{\lambda}{\mathfrak{F}} + \frac{\mu}{\mathfrak{G}} + \frac{\nu}{\mathfrak{H}}\right)\left(\frac{a}{\mathfrak{F}}\mu\nu + \frac{b}{\mathfrak{G}}\nu\lambda + \frac{c}{\mathfrak{H}}\lambda\mu\right) = 0;$$

that is, the curve breaks up into a point, and a conic inscribed in the triangle.

In the case where the conic breaks up into a pair of lines, then we have

$$(a, b, c, f, g, h)(x, y, z)^2 = 2(px + qy + rz)(p'x + q'y + r'z),$$

and thence

$$\begin{aligned} (\mathfrak{A}, \mathfrak{B}, \mathfrak{C}, \mathfrak{F}, \mathfrak{G}, \mathfrak{H})(x, y, z)^2 = \\ - \{(qr' - q'r)x + (rp' - r'p)y + (pq' - p'q)z\}^2; \end{aligned}$$

so that the equation in (λ, μ, ν) is

$$\{(qr' - q'r)\lambda + (rp' - r'p)\mu + (pq' - p'q)\nu\} \\ \{pp'(qr' - q'r)\mu\nu + qq'(rp' - r'p)\nu\lambda + rr'(pq' - p'q)\lambda\mu\} = 0;$$

where the point represented by the equation

$$(qr' - q'r)\lambda + (rp' - r'p)\mu + (pq' - p'q)\nu = 0$$

is, of course, the intersection of the two lines.

XXIV. *Supplement to a Theory of the Zodiacal Light.*

By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

AT the end of my communication on the Zodiacal Light in the February Number, reference is made to another phenomenon of like character, respecting which I expressed the opinion that it is probably related to the zodiacal light, but I did not attempt to give a theoretical explanation of it. Subsequent consideration of the observations to which I there referred has led me to the conclusion that it is so closely connected with the ordinarily observed zodiacal light, that the same theory ought to embrace both phenomena. The theory which I proposed should accordingly be capable of this extension; and it is my present object to show in what manner this demand upon it may be satisfied. But first I must state the precise nature of the phenomenon proposed to be explained, so far as that may be gathered from the accounts of Mr. Jones and Prof. Brorsen, which are the only ones that I am acquainted with.

The description of the luminous appearance by Mr. Jones, whose observations of the zodiacal light I referred to in my former communication, is contained in a letter to the editor of the 'Astronomical Journal' (vol. v. p. 28), dated Quito, November 18, 1856. After describing the advantages of his position in regard both to situation on the earth's surface relative to the ecliptic, and the rarity and transparency of the atmosphere, he makes a statement of the phenomena which had better be given in his own words. "My observations," he says, "are made on the summit of the hill Ychinbía, immediately adjoining Quito on the east, a place where I have horizons sufficiently open and clear. It is not my present object to give details of observations, but simply to notice one result, in hope of now drawing attention to the subject, and perhaps of inducing other observers to give it their particular notice. *I see here every night, and all through the night, a luminous arch from east to west, quite across the sky.* This arch, 20° wide, is visible at all hours, when the sky is clear, but is brightest and most striking when the ecliptic

* Communicated by the Author.

is vertical, at which times it looks almost like another Milky Way. It is very evidently the zodiacal light. This luminous arch, I know, is not a new discovery; for Baron Humboldt saw it in the seas off Mexico, and Professor Brorsen has noticed it more fully in Germany. I also had glimpses of it in my late cruise in the eastern seas. Here, however, it is developed with a remarkable degree of distinctness, and I am giving it particular attention." At the time of his writing, his observations at Quito had been of eleven weeks' continuance. He further states that the light of the luminous arch has little reference to change of position as regards the ecliptic, and that "while the zodiacal light over the horizon tapers off in intensity as it ascends upwardly, the luminous arch, after this other ceases, is uniform quite across the sky."

In a first letter, contained in vol. xlii. of the *Astronomische Nachrichten* (col. 219), and dated from Senftenberg, 1855, Nov. 13, Prof. Brorsen states that in that place, and also in the Isle of Alsen in the Duchy of Schleswig, he had observed during the two preceding years a luminosity, which he calls a reflexion (*Gegenschein*) of the zodiacal light; and he then gives, as follows, the result in substance of the two years' observations. "The reflexion is seen at the times of both equinoxes, but is more perceptible in spring than in autumn. The first faint trace may be remarked as early as February, after which, in March, April, and the beginning of May, it increases continually in brightness and extent. The much fainter and smaller autumn reflexion is apparent during the months of September, October, and November. Respecting both, it is a fact of which I have convinced myself by repeated observations, that the brightest part lies exactly opposite the sun's place, so that the estimated point of greatest intensity agrees within half a degree with the opposition-point of the sun. Further, the observations indicated that the spring reflexion towards the middle of April connects itself, by a very faint beginning, becoming by degrees brighter, as a scarcely perceptible light streak, with the westerly zodiacal light; while the autumn reflexion in the first half of November appears stretched along the ecliptic to the westerly horizon in a faint, scarcely recognizable girdle of light, which gradually, by increasing brightness and more extended base, passes over into the known appearance of the winter-evening zodiacal light. But from this time to the beginning of March, its point remains quite stationary about the position of τ_1 and τ_2 Arietis. Both reflexions can be distinctly seen here at the same time of the year on each evening that the sky is clear and the region of the heavens in which they are situated is moderately high, even after the commencement of moonlight."

In a subsequent communication, entitled "On the Ring-form Shape of the Zodiacal Light," and dated October 18, 1858, Prof. Brorsen gives observations made at Senftenberg from April 1854 to October 1858, of the positions of maximum intensity of "the faint extension of the ordinary zodiacal light from west and east along the ecliptic," which he had described in his former letter. The observations are all made either in February, March, April, and May, or in August, September, and October; and certainly the estimated positions of the maximums agree remarkably with the points of opposition to the sun. The second letter is contained in the *Astronomische Nachrichten*, vol. xlix. col. 219.

Allowance being made for the difference of positions of the observers, it can hardly be doubted that these two accounts refer to the same phenomenon. The chief point of discrepancy is that the American observer does not notice a position of maximum intensity, but states, on the contrary, that the luminous arch is uniform quite across the sky, while on this fact the other observer speaks very decidedly. On the other hand, it may be remarked that Prof. Brorsen's description of the phenomenon in the first half of November, as being an extremely faint *girdle* of light extending along the ecliptic to the horizon, agrees well enough with the result of the observations at Quito, the different estimations of the intensity of the light being readily accounted for by the difference of the localities.

Such being the account of the principal features of the phenomenon, I proceed now to offer a theoretical explanation of it, after recalling attention to the theory of the zodiacal light proposed in my last communication. The latter luminosity is there ascribed to gyrations of the æthereal medium produced by the rotation of the sun about its axis, and to disturbances which they undergo by reason of the motion of translation of the sun in space. It was considered that the effect of this motion would be the same as if the æther were impressed with an equal motion in the opposite direction. The sun would thus be at rest, and together with the steady gyrations there would be a uniform stream of the æther. It was proved by mathematical reasoning that such steady motions might coexist without mutual disturbance, so far as terms including the square of the velocity indicate, and that disturbances would arise out of motions expressed by terms of the *third* and higher orders relative to the velocity. Such disturbances, it was supposed, might become sensible in the form of light-undulations originating in the space common to the two motions. It is, however, to be said that in this way the zodiacal light may be accounted for, but no explanation is given of the related phenomenon of the luminous arch; and, moreover, it must be admitted that no reason is given in

this theory for concluding that motions expounded by terms of the third order would be adequate to produce, excepting in very small degree, the former phenomenon. I think also that the origins of both luminosities ought to be looked for in disturbances of *vibratory* motions, rather than in those of steady motions. If we supposed the luminosity to be due to a kind of reflexion of the solar rays from the parts of the æther subject to the gyratory motion, we should, it is true, have disturbances of vibratory motions expressed by terms of the order of the *square* of the velocity, and might thus account for the zodiacal light, but the phenomenon which is the immediate subject of this inquiry would still be left unexplained.

I come now to a consideration which, if I had proceeded consistently with the general theory of the physical forces which I expounded some time since in this Magazine, would have been introduced at first. It is the characteristic of a true physical theory that it fails only when its consequences are not rigidly and completely followed out, whereas a false or imperfect theory breaks down under this very process. It may be remembered that in that general theory the different physical forces are accounted for by the dynamical action of different orders of undulations of the æthereal medium, and that the force of gravity was ascribed to undulations emanating from masses, and of much larger dimensions than those which produce the phenomena of light. Now such large undulations emanating from the sun would be subject to disturbances expressed by terms of the *second* order in respect to velocity in their passage through those portions of the æther which are in gyratory motion; and disturbances of this kind might, as I have elsewhere argued, be made up of vibratory motions of different orders, a portion of which may be presumed to be of the light-producing order. But if we take into account the gravity-undulations emanating from the sun, it is evident that we must also take account of those emanating from the earth. Also it should be borne in mind that Mr. Jones's observations prove that the zodiacal light extends to a distance from the sun greater than the radius of the earth's orbit. It is therefore an obvious inference from these considerations, *to ascribe the zodiacal light to the gravity-undulations from the sun, and the luminous arch to those from the earth.*

The reason that the terrestrial undulations produce a sensible effect in regions where the effect of the solar undulations is too faint to be perceptible is, that they are really much the more powerful within a certain distance from the earth. The distance from the earth, along the prolongation of the earth's radius-vector, at which the effects would be equal, may be calculated

on the following principle. By knowing the law of gravity and the proportion of the attractions of the sun and earth at the unit of distance from their centres, we can find the point on the prolongation of the earth's radius-vector at which the sun's attraction is just equal to that of the earth. This point will be found to be at the distance of 152,500 miles nearly from the earth's centre. Now, without borrowing from theory more than that the gravitating effect of æthereal undulations, and their luminous effect now under consideration, each depend on terms of the order of the square of the velocity, we may, I think, presume that, since in each case there is no other difference of circumstances than that one set of undulations proceeds from the sun and the other from the earth, the effects of both kinds are in the *same* proportion. Accordingly, at distances from the earth within that above stated, the luminous effect of the terrestrial undulations will exceed that of the solar. It is therefore reasonable to infer that the total effect of the coexistence within that space of the terrestrial and solar undulations and the solar gyrations is the appearance of the luminous arch. It is evident that it may present the considerable breadth of 20° from its having its origin at points comparatively near the earth, and that it is at the same time limited in breadth on account of the limited transverse extent of the gyrations.

The above explanation seems to be open to the following objection. It may be urged that the earth's rotation about its axis must also produce gyratory motions of the æther, and that the coexistence of these with the terrestrial gravity-undulations should, according to the same principles, produce a luminosity disposed about the plane of the earth's equator, presenting the appearance of a luminous belt in that direction. To this I reply that, judging from the phenomena of the zodiacal light where it approaches the body of the sun, the luminosity would be spread over the whole of the earth's surface, and, without presenting a definite outline, would only be somewhat in excess as seen from the equatorial parts. In fact, in this way we may account, in great part, at least, for the luminousness of the sky which is observable every clear night at all places and all seasons of the year. This phenomenon has not escaped the attention of either theorists or observers. Mr. Jones has made the following interesting statement of what he noticed at Quito. "The thinness of the air on this plateau, by which, during the day, objects a great way off are seen with remarkable distinctness, and seem to be near by, might be expected to assist in atmospheric developments of several kinds, unusual at other places; and such is also the fact. A phenomenon which has sometimes drawn the attention of philosophers, namely, a general brightness of the

air at night, without any apparent cause for it, is in some nights very remarkable here. I find it has been noticed by others as well as myself. The brightness, without moon or any assignable reason, has been so great at times, that I have taken out printed papers or books to ascertain whether I could not see to read by it."

With respect to the variations of the visibility of the arch in different seasons of the year, noticed by Prof. Brorsen, two circumstances have to be considered. First, as in the case of the zodiacal light, the visibility of so faint a luminosity is dependent on the inclination of the ecliptic to the horizon at the times of observation. It seems that on this account the observations at Senftenberg were chiefly made near the times of the vernal and autumnal equinoxes. Again, the plane of symmetry of the zodiacal light being inclined at a small angle to the plane of the ecliptic, and the arch having its origin at the outlying portions of the gyrations, the degree of its brightness will depend, much more than that of the zodiacal light, on the earth's proximity to the nodes. There being reasons for concluding, as I showed in the former paper, that the earth passes the nodes about June 6 and December 6, it will be seen why, as stated by Prof. Brorsen, the brightness increases on advancing from the equinoxes towards those epochs, and why, regard being had to the positions of the ecliptic, the times most favourable for its visibility are not quite synchronous with those for the visibility of the zodiacal light.

Another fact distinctly attested by Prof. Brorsen, namely, the greater visibility of the arch in spring than in autumn, seems to point to some degree of eccentricity of the zodiacal light relative to the sun's position. An eccentricity of this kind might very reasonably be attributed to an effect of the disturbance of the æther caused by the motion of the body of the sun in space. Hitherto we have taken no account of this disturbance; but it is evident that the motion of translation produces, as well as the rotatory motion, a steady motion relative to the sun, and that the whole relative steady motion of the æther is really compounded of the effects of the two movements. The resolved part of the sun's motion perpendicular to the plane of symmetry of the zodiacal light would have no effect in producing the eccentricity; but the part resolved in that plane would, by the confluence of the relative streams which pass by or through the sun, produce an elongation of the disturbance, or a kind of *wake*, in the quarter from which the sun is moving. Thus the motions concerned in generating the luminous arch would not be the same in all directions from the sun. I shall not pursue this part of the subject further at present, because it will pro-

bably not be safe to theorize upon it till the facts relating to the degree of visibility of the arch in different seasons of the year have been ascertained by observations taken at the equator, in positions, such as that of Quito, where the influences of atmospheric changes are in a great degree eliminated. I will only add one theoretical remark relative to the position of maximum brightness, observed by Prof. Brorsen to be just opposite the sun's place. It appears to me that the explanation of this singular fact must be wholly distinct from that of the luminous arch. According to my theory of gravity-undulations, those from the sun are propagated without impediment or retardation through the body of the earth; but it cannot be affirmed that they undergo no modification whatever. It seems reasonable to suppose that they partly consist of, or in their transmission give rise to, a small amount of undulations that are subject in a slight degree to retardation and consequent refraction, and that to an effect of the convergence of these undulations the phenomenon in question may be attributed. In the "Theory of the Force of Gravity" (Phil. Mag. for December, 1859), I have suggested that a similar modification of terrestrial gravity-undulations might account for the observed excess of gravity in insular positions, and for deviations of the plumb-line noticed in India.

The concurrence and consentaneity of different parts of the general physical theory in the foregoing explanations, embracing both the zodiacal light and the associated phenomenon of the luminous arch, ought, I think, to be regarded as some evidence of the truth of the theory.

Cambridge, February 20, 1863.

XXV. *Observations on Cæsium and Rubidium.* By OSCAR D. ALLEN, Ph.B., Assistant in the Sheffield Laboratory, Yale College, U.S.*

THE discovery of the presence of the new elements rubidium and cæsium in several varieties of European lepidolite, made it a subject of interesting inquiry to ascertain whether American lepidolite would not also serve as a source for these rare metals.

A preliminary experiment made last autumn by Mr. John M. Blake and myself having shown that the lepidolite from Hebron in Maine contains these alkalies in comparative abundance, I was led to visit that locality, and there obtained the material which served for the following investigation.

* From Silliman's American Journal for November 1862.

Lepidolite occurs at Hebron in large quantity, in a coarsely crystalline granite, associated with red and green tourmaline and albite. It has a granular and at the same time foliated crystalline structure, a pale rose to violet colour, and very closely resembles the lepidolite of Penig in Saxony, and, like that, is also associated with the rare species amblygonite.

Preparation of the salts of Cæsium and Rubidium from the Hebron Lepidolite.—The process used for decomposing this mineral was based upon that employed by Prof. J. Lawrence Smith for the determination of alkalis in silicates. Ten parts of the pulverized lepidolite were first mixed with forty parts of coarsely powdered quicklime; a mixture of enough water to slake the quicklime, with hydrochloric acid sufficient to form from six to seven parts of chloride of calcium, was next made ready; the two mixtures were then united, and stirred vigorously during the process of slaking, thus intimately blending the mineral with suitable proportions of dry hydrate of lime and chloride of calcium.

It was found by experiment that practically as good results were obtained when the lepidolite was powdered sufficiently fine to pass a sieve of 20 holes to the linear inch as when it was more finely pulverized, the fact being that the foliated structure of the mineral exposes a large surface to the decomposing agency of the lime mixture.

The mixture was heated to redness for six to eight hours in Hessian crucibles. Care was taken to avoid a heat much above redness, as otherwise the alkaline chlorides volatilize in dense clouds, and, the mass fusing, is absorbed to a considerable extent into the crucible and lost. The long duration of the ignition was a matter of convenience, due to the character of the furnace employed, and probably not necessary to the decomposition of the mineral.

The agglomerated product obtained from the ignition of this mixture was detached from the crucibles and boiled with water till all but a trace of the chlorides was removed. The solution thus procured, containing chloride of calcium and the chlorides of the alkali-metals, was evaporated till crystals began to form; then sulphuric acid was added as long as sulphate of lime separated, taking care to avoid an excess, and the whole mass was evaporated to dryness, and strongly heated to expel free hydrochloric acid. The residue was treated with water, and the small quantity of sulphate of lime which passed into solution was precipitated by carbonate of ammonia; the filtered solution was again evaporated to dryness and ignited.

Ten and a half kilogrammes of lepidolite treated in this way afforded 2169 grammes of salts consisting of chlorides, with a

small admixture of sulphates, of sodium, lithium, potassium, rubidium, and cæsium. This quantity of salts, subjected to Bunsen's process of fractional precipitation with bichloride of platinum, furnished 132 grammes of the platinochlorides of cæsium and rubidium, in which no potassium could be detected with the spectroscope. The platinochlorides were very gently heated in a current of hydrogen gas until complete reduction of the platinum took place, and the chlorides were then extracted with water.

The percentages of cæsium and rubidium obtained from the mineral by this process were calculated from the amount of chlorine contained in these mixed chlorides.

0.5825 grm., dissolved in water and precipitated with nitrate of silver, gave 0.5835 grm. of chloride of silver, which represents 0.1439 grm. of chlorine.

These numbers furnish the following equations:—

$$\text{Rb} + \text{Cs} = 0.5825 - 0.1439, \quad (1)$$

$$\frac{\text{Rb}}{85.36^*} + \frac{\text{Cs}}{123.35^*} = \frac{0.1439}{35.5}, \quad (2)$$

which give $\text{Cs} = 0.3002$ and $\text{Rb} = 0.1384$. According to these proportions, the 132 grammes of platinochlorides contained 31.1969 grammes of cæsium, 14.3826 grammes of rubidium, which numbers respectively correspond to 0.3 per cent. and 0.14 per cent. of the mineral employed.

It appears, therefore, that it is practicable to extract almost one-half per cent. of the two metals from the Hebron lepidolite, even when operating on a large scale, and in a somewhat crude manner. In separating the platinochlorides of cæsium and rubidium from the platinochloride of potassium, a not inconsiderable amount of these metals went into solution with the potassium-salt, thus materially diminishing the quantity obtained. Much the larger proportion of this loss was rubidium, due to the greater solubility of its platinochloride. On comparing these results with Cooper's analyses † of the Rozena lepidolite, it appears that, although not quite so rich in rubidium, the Hebron mineral is remarkably rich in cæsium. The lepidolite from Rozena and Zinnwald contain, according to the published analyses, only an unweighable trace of cæsium, while that from Hebron contains more than three-tenths of 1 per cent.

Experiments in separating Cæsium and Rubidium.—The process described by Bunsen for separating the new alkalies appeared to be so troublesome, requiring for the preparation of pure rubi-

* Combining proportions of cæsium and rubidium determined by Bunsen (Poggendorff's *Annalen*, vol. cxiii. p. 339).

† *Journ. Prakt. Chem.* vol. lxxxv. p. 125.

dium-salts twenty to thirty extractions of the carbonates with boiling absolute alcohol (Phil. Mag. for July 1862, p. 50), that I have made various attempts to discover a simpler method.

In the first place, a trial was made with the picrates of the new metals. To a concentrated solution of their mixed chlorides an alcoholic solution of picric acid was added. The liquid immediately filled with fine acicular crystals. These were rinsed with water and successively recrystallized from fresh portions of water eleven times. Portions of the first, second, third, fourth, seventh, and eleventh crops of crystals were separately examined in the spectroscope, the picrates being converted into chlorides for this purpose by treatment with aqua regia. No difference being observable between the spectra of the various crops, no further experiments were made in this direction. It may be here remarked that the mixed picrates crystallize with great facility in needles an inch in length, and perfectly resemble the corresponding potassium-salt.

A second series of trials was made with the platinobromides of potassium, rubidium, and cæsium. The platinobromide of potassium is known to be readily soluble in water. The platinobromides of cæsium and rubidium readily separate from dilute solutions of these three metals, but carry down potassium with them. For the removal of the latter metal from the new alkalis, the platinobromides appear to have no advantage over the platinochlorides, while they are equally inadequate to the separation of cæsium and rubidium from each other. In external characters the three platinobromides closely resemble each other.

Finally, recourse was had to the bitartrates, and with satisfactory results. Carbonates of cæsium and rubidium were first prepared from the chlorides by converting them into sulphates, separating the sulphuric acid with caustic baryta, and removing the excess of baryta by carbonic acid. To the alkaline solution thus obtained, twice as much tartaric acid was added as was necessary to neutralize it. This solution was concentrated till it was nearly saturated at 100° C. The crystals which deposited on cooling, when examined by the spectroscope, showed the rubidium lines more intensely than did the original mixture, while the cæsium lines were much fainter. This product was dissolved and recrystallized from hot saturated solutions three times. The cæsium reaction in these successive crops diminished until in the fourth it disappeared, leaving the rubidium spectrum in entire purity.

In order to ascertain whether the more soluble bitartrate of cæsium could be purified from rubidium by fractional crystalli-

zation, the solution from which the first crystals had been removed was concentrated to nearly one-half its original volume, when, by cooling, a very small quantity of salts of the two alkalies was deposited. This operation was repeated three times, when a portion of the solution, evaporated to dryness and examined by the spectroscope, gave only the lines belonging to cæsium. The several intermediate products containing both alkalies were then united, and another portion of each salt separated from them in the same manner. By repeating this process of fractional crystallization four times with about 40 grammes of the mixed salts, 23·77 grammes of bitartrate of cæsium, and 12·511 grammes of bitartrate of rubidium were obtained, while 3·74 grammes remained unseparated. It was found that the cæsium-salt thus obtained, although exhibiting no impurity when tested by the spectroscope directly, *i. e.* after conversion by ignition into carbonate, was still mixed with a trace of rubidium, as on converting it into chloride a faint line characteristic of the latter metal was perceptible. The separation of two or three more small crops of crystals sufficed to render the residual solution perfectly free from any admixture that could be detected by a spectroscope of ordinary power*. The rubidium-salt was also more carefully tested in the same manner, but was found to be entirely pure.

The process above described thus furnishes a simple and easy method of separating in a perfectly pure state a large share (in these trials about 90 per cent.) of a mixture of the two alkalies. It requires no great expenditure of time, since the solutions employed can be concentrated at high temperatures, and, on cooling, immediately deposit well-formed crystals.

Composition and solubility of the Bitartrates of Cæsium and Rubidium.—Bitartrate of rubidium crystallizes from hot solutions in colourless, transparent, flattened prisms, which are often half an inch or more in length, even when formed rapidly from small quantities of solution. They remain unaltered in the air, and also are unchanged at a temperature of 100° C. The pulverized salt, dried at 100° C., was burned with chromate of lead in the usual manner.

I. 0·4681 grm. gave 0·0902 grm. water, and 0·354 grm. carbonic acid.

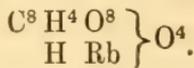
To determine the base, the salt was heated to a temperature a little below redness, the resulting carbonate extracted with water from a small residue of carbon which could not be burned away without volatilizing rubidium. The carbonate was then con-

* The instrument used was a modification of Bunsen and Kirchhoff's spectroscope, devised by Prof. J. P. Cooke, and manufactured by Messrs. Alvan Clark and Sons, of Cambridgeport.

verted into chloride, fused, and weighed without exposure to the air.

II. 1.3772 grm. gave 0.7149 grm. chloride of rubidium.

In the following statement these results are reduced to per cents., and compared with the calculated composition of bitartrate of rubidium as expressed by the formula



	Calculated.		Found.	
			I.	II.
C ⁸ . . .	48.00	20.48	20.62	
H ⁵ . . .	5.00	2.13	2.17	
O ¹¹ . . .	88.00	37.55	..	
RbO . . .	93.36	39.84	..	40.09
	<u>234.36</u>	<u>100.00</u>		

The solubility of this salt in hot and cold water was determined by evaporating on the water-bath solutions saturated at the given temperatures and weighing the residues.

I. 11.9254 grms. of solution, saturated at the boiling-point, gave a residue of 1.2555 grm.

One part of the salt accordingly requires 8.5 parts of boiling water for solution.

II. 17.535 grms. of solution, saturated at 25° C., gave a residue of 0.205 grm.

III. 16.094 grms. of solution, saturated at 25° C., gave a residue of 0.188 grm.

One part of the salt thus required respectively 84.53 and 84.6 parts of water at 25° C. for solution.

The *bitartrate of cæsium* forms crystals closely resembling the rubidium-salt, but in my experiments they were usually of smaller size.

The salt obtained by concentrating the solution from which all the rubidium had been separated was to all appearance pure. It was recrystallized, and after drying at 100° C., at which temperature it suffered no loss of weight, was analysed in the same manner as the bitartrate of rubidium.

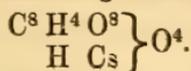
I. 0.4718 grm. gave 0.0786 grm. water, and 0.294 grm. carbonic acid.

II. 0.5966 grm. gave 0.101 grm. water, and 0.372 grm. carbonic acid.

III. 1.3086 grm. gave 0.7708 grm. chloride of cæsium.

Assuming the combining proportion of cæsium to be 123.35, as determined by Bunsen, the following statement exhibits the

composition of the salt, according to the formula



	Calculated.		Found.		
			I.	II.	III.
C ⁸	48.00	17.62	16.99	17.02	
H ⁵	5.00	1.83	1.85	1.88	
O ¹¹	88.00	32.31	
CsO	131.35	48.24	48.70
	<u>272.35</u>	<u>100.00</u>			

The discrepancy between the composition as calculated and found is perhaps due to a slight admixture of the neutral tartrate, which might possibly have been present owing to the use of insufficient tartaric acid.

The solubility of bitartrate of cæsium was determined for the same temperature and by the same methods as were employed in the case of the rubidium-salt.

I. 2.998 grms. of solution, saturated at the boiling-point, gave a residue of 1.483 grm.

One part of the salt requires 1.02 part of boiling water for solution.

II. 11.931 grms. of solution, saturated at 25° C., gave a residue of 1.054 grm.

III. 8.7625 grms. of solution, saturated at 25° C., gave a residue of 0.7727 grm.

One part of the salt accordingly requires 10.32 parts of water at 25° C. for solution.

The fact that bitartrate of rubidium requires about eight times as much water for solution as bitartrate of cæsium, explains the facility with which these salts can be separated from each other by crystallization.

In these experiments I have received the advice and assistance of Professors Johnson and Brush, for which I here take pleasure in expressing my most grateful acknowledgments*.

New Haven, August 12, 1862.

* Since the above was written I have recovered, from the mother-liquors and washings of the 132 grammes platinochlorides of cæsium and rubidium first obtained, an additional quantity of potash-free platinochlorides (chiefly of rubidium) amounting to 40 grammes, making the total yield 172 grammes from 10½ kilogrammes of lepidolite. Most of this remained in solution from the use of insufficient bichloride of platinum in some of the precipitations. The amount of rubidium in the Hebron lepidolite thus appears to be not less than in that from Rozena.

XXVI. *On the Equivalent and Spectrum of Cæsium.*

By S. W. JOHNSON and O. D. ALLEN*.

IN the preceding article a method is described of separating cæsium from rubidium by fractional crystallization of the bitartrates of these metals. The analyses of the bitartrate of cæsium there given, while perfectly according with each other as regards carbon, disagree with the numbers deduced from Bunsen's equivalent to such an extent that we have undertaken to ascertain whether the salt was impure or Bunsen's equivalent incorrect.

From the great care used in preparing the bitartrate, and especially from the fact that its spectrum remained unaltered though the salt was repeatedly recrystallized, we were inclined to suppose that Bunsen had not operated with a pure substance.

This might easily happen, on account of the small quantity of material at his disposal, without at all detracting from the merit of this distinguished chemist.

A quantity of bitartrate of cæsium, purified by concentrating its solution and recrystallization, as described in the paper referred to, and containing no foreign matters recognizable by the spectroscope, except an inevitable trace of sodium and (to judge from a certain red line) perhaps some lithium, was treated directly with bichloride of platinum in quantity sufficient for complete precipitation. This platinochloride of cæsium, after thorough washing, was reduced in hydrogen, the chloride of cæsium dissolved from the platinum and evaporated to dryness with addition of a little hydrochloric acid.

We thus obtained an amorphous mass of a pure white colour, which, unlike Bunsen's chloride, was *not perceptibly deliquescent* even in a very moist atmosphere. The spectrum of the chloride thus prepared was identical with that of the original bitartrate. Both salts gave a red line nearly coincident with the α line of lithium. In order to determine whether this line was due to a trace of lithium, or belongs to the spectrum of cæsium, a portion of chloride was again precipitated with a relatively small quantity of bichloride of platinum, the precipitate was most thoroughly washed, and from it a new sample of chloride of cæsium was prepared. The red line was no less apparent in this than in the former preparations. The same process of partial precipitation was repeated a third time without altering the spectrum.

Again, from a hot dilute solution of 15 grms. of chloride of cæsium, about 1 grm. of cæsium was thrown down as platinochloride; the product thus procured gave a spectrum identical with that from the original bitartrate.

* Communicated by the Authors: from Silliman's American Journal for January 1863.

We concluded from these experiments that our chloride of cæsium was free from lithium, and as pure as it is well possible to obtain any substance without the most extraordinary and for most purposes unreasonable precautions.

As to the properties of the chloride of cæsium, we observed that not only is it not deliquescent, but it is hardly hygroscopic. The unfused and porous salt may be weighed in moist air with as much accuracy as chloride of sodium. After it has been fused it does not alter in weight during twenty-four hours' exposure to the air in cold dry weather. It may be fused in a platinum capsule over the gas-flame when the air is dry without acquiring an alkaline reaction. In a damp atmosphere it is apt to lose chlorine during fusion. The residue, after the reduction of platinochloride of cæsium by hydrogen at a gentle heat, is alkaline. It is hardly possible to fuse chloride of cæsium without loss by volatilization. Hence our first estimations of cæsium in this form were too low by 4- to 7-tenths of 1 per cent.

For determining the equivalent of cæsium we have made four chlorine-estimations. Two of these (I. and II.) were made in the chloride obtained as already described. Their details and results are given below. The filtrates from these analyses, containing nitrates of cæsium and silver, after the latter had been removed, were added to a solution of several grammes of the original chloride, and the whole was partially precipitated with bichloride of platinum, and a second portion of chloride of cæsium procured, on which determination III. was made. Finally, the nitrate of cæsium from this analysis was mingled with repeatedly purified chloride obtained in the previous study of the spectrum; about half the cæsium was again thrown down as platinochloride, and with this product another estimation of chlorine (IV.) was made.

The determination of chlorine was conducted in the usual manner, by precipitation with nitrate of silver and filtration. Washed Swedish filters were employed, which gave each an ash weighing four-tenths of a milligramme. The weighings were taken on a balance by Becker and Sons, of Brooklyn, New York, which with an ordinary load indicates one-twentieth of a milligramme with great decision and perfect constancy.

The data of our determinations are as follows:—

I.	1·8371 grms. Cs Cl	gave 1·5634 Ag Cl	=·386598 Cl,	and 1·4505 Cs.
II.	2·1295	„ „	1·8111 „	=·447848 „ 1·68165 „
III.	2·7018	„ „	2·2992 „	=·56853 „ 2·13327 „
IV.	1·56165	„ „	1·3302 „	=·32893 „ 1·23272 „

The percentage composition of chloride of cæsium, and the equivalents deduced from the above figures, are as follows, silver being considered = 107·94, and chlorine = 35·46, Stas:—

	Per cent. of		Equiv. of Cs.	
	Cl.	Cs.		
1.	21·044	78·956	133·050	Allen.
2.	21·031	78·969	133·150	Johnson.
3.	21·043	78·957	133·054	Johnson.
4.	21·063	78·937	132·892	Allen.
Average	21·045	78·955	133·036	

We may accordingly assume the round number (133) as the equivalent of caesium.

Calculated by this equivalent, the formula of bitartrate of caesium corresponds well with the results of experiment. As mentioned in the paper referred to, the analyses of this salt furnished the following data:—

I. 0·4718 grm. gave 0·0786 grm. water, and 0·294 grm. carbonic acid.

II. 0·5966 grm. gave 0·101 grm. water, and 0·372 grm. carbonic acid.

III. 1·3086 grm. gave 0·7708 grm. chloride of caesium.

In two other estimations since made,

IV. 2·0347 grms. gave 1·206 grm. chloride of caesium.

V. 1·8271 grm. gave 1·0857 grm. chloride of caesium.

	Calculated.				Found.					
	Cs=123·35.		Cs=133.		I.	II.	III.	IV.	V.	
C ^s	48·00	17·62	C ^s	48	17·02	16·99	17·02
H ^s	5·00	1·83	H ^s	5	1·77	1·85	1·88
O ¹¹	88·00	32·31	O ¹¹	88	31·21
CsO	131·35	48·24	CsO	141	50·00	49·30	49·61	49·73
	272·35	100·00		282·	100·00					

The equivalent number (133) brings caesium into a triad with rubidium and potassium. We have then two alkali triads, viz. lithium (eq. 7), sodium (eq. 23), and potassium (eq. 39·1),

$$\frac{7+39}{2} = 23;$$

and potassium, rubidium (eq. 85·36), and caesium,

$$\frac{39+133}{2} = 86.$$

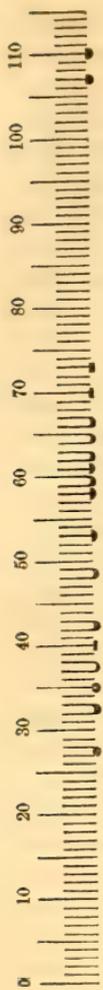
The correction of the equivalent of caesium implies a revision of its spectrum, since the data given by Kirchhoff and Bunsen with reference to both were obtained from the same impure material.

The cæsium spectrum, as we have procured it, is perhaps, from the number, colour, and definition of its lines, the most beautiful to be observed among all the alkali and earthy metals. Kirchhoff and Bunsen, in the figure given by them (*Philosophical Magazine*, vol. xxii. 1861), represent eleven lines. We find without difficulty seven more lines, and observe further that some of those figured by Kirchhoff and Bunsen are not mapped in their correct positions. To enable other chemists to compare their cæsium preparations with ours, we will attempt to describe the cæsium spectrum as seen in our instrument, which has a single flint-glass prism.

Beginning at the left or red extremity of the spectrum, we will indicate the lines in the order of their occurrence by Roman numerals: I. is a red line of medium brightness nearly equidistant between the Fraunhofer lines *a* and *B*; II. is a bright line partly coincident with, but slightly to the left of and narrower than the α line of lithium; III. is a faint line nearly approaching Fraunhofer's line *C*; IV. is the faintest of the red lines; V. is a faint line midway between the α and β lines of lithium; VI. is a bright red line midway between the sodium line and α lithium; VII. is an orange-red line of medium intensity directly to the right of the α strontium line; VIII. is a fine yellow line just to the right of and close upon the sodium line. The position of the green lines it is difficult to describe. First comes a group of three, IX. X. and XI., which are separated by very narrow spaces, and which are represented well in the spectrum plate of Kirchhoff and Bunsen, though placed a trifle too far to the right. Then, after an interval scarcely wider than the lines themselves, come XII. and XIII., which are very near each other. After another space as broad as these lines we encounter XIV. Midway between XIV. and XVI. is XV. The latter (XV.) coincides with the dark line *E*. Finally, the two pale blue lines XVII. and XVIII. complete the list.

For the convenience of those who may use spectroscopes of the same construction as ours, we will mention the degrees on the scale of our instrument which correspond to the cæsium lines. In our observations we have brought the degree 100 (10 on the scale) into the sodium line. Then the blue of strontium is at 156° , the violet of potassium 257° , the red of potassium at $65\text{--}66^\circ$, the red of lithium at $80\text{--}81^\circ$. With this adjustment the cæsium lines are as follows, beginning with the red: I. 75° , II. 80° , III. $82\text{--}3^\circ$, IV. 85° , V. $87\text{--}8^\circ$, VI. 91° , VII. $97\text{--}8^\circ$, VIII. 101° , IX. 106° , X. $107\text{--}8^\circ$, XI. 109° , XII. 111° , XIII. $112\text{--}13^\circ$, XIV. $114\text{--}15^\circ$, XV. 118° , XVI. 121° , XVII. $157\text{--}58^\circ$, XVIII. 160° .

The position of the cæsium lines on the scale figured at the



top of the spectrum-plate in Fresenius's *Zeitschrift*, is approximately given in the accompanying diagram, by help of which our results may be directly compared with those of Kirchhoff and Bunsen.

The order of brilliancy in the lines of what we suppose to be the spectrum of pure cæsium, with but the minutest trace of sodium, is for the red lines as follows: VI. II. VII. I. V. III. IV. The line IV. is only made out under the most favourable conditions. II., nearly coincident with α lithium of Kirchhoff and Bunsen, and not figured by them, is as bright as their γ cæsium, our VI. (?) Among the yellow and green lines to the right of the sodium line, the order of brilliancy is the following: VIII. IX. XI. XII. XIV. XIII. XV. X. The yellow line VIII. is hardly less characteristic of the spectrum of *pure* cæsium than the two blue lines. It also is nearly as distinct as any of the green lines when sodium is not present in too large quantity, and is much more readily made out than the extreme red line δ of rubidium.

To sum up, we find four red lines to the left of those given by Kirchhoff and Bunsen, one of which is as bright as any of the red lines in the cæsium spectrum. Further, the red lines of Kirchhoff and Bunsen are not figured in their true positions, being too near each other and too far to the right. Finally, we observe a fine yellow line and two unimportant green lines not mapped by them. The lines which we have supplemented to those of Kirchhoff and Bunsen are not characteristic except in the absence of foreign matters. For this very reason, however, they become important to those who are engaged in the study of the new elements.

New Haven, Conn., December 24, 1862.

XXVII. *On Radiation through the Earth's Atmosphere.* By JOHN TYNDALL, F.R.S., Professor of Natural Philosophy, Royal Institution*.

NOBODY ever obtained the idea of a line from Euclid's definition that it is length without breadth. The idea is obtained from a real physical line drawn by a pen or pencil, and therefore possessing width,—the idea being afterwards brought, by a process of abstraction, more nearly into accordance with the conditions of the definition. So also with regard to physical

* From the Proceedings of the Royal Institution for Jan. 23, 1863.

phenomena; we must help ourselves to a conception of the invisible by means of proper images derived from the visible, afterwards purifying our conceptions to the needful extent. Definiteness of conceptions, even though at some expense to delicacy, is of the greatest utility in dealing with physical phenomena. Indeed it may be questioned whether a mind trained in physical research can at all enjoy peace without having made clear to itself some possible way of conceiving of those operations which lie beyond the boundaries of sense, and in which sensible phenomena originate.

When we speak of radiation through the atmosphere, we ought to be able to affix definite physical ideas, both to the term atmosphere and the term radiation. It is well known that our atmosphere is mainly composed of the two elements oxygen and nitrogen. These elementary atoms may be figured as small spheres scattered thickly in the space which immediately surrounds the earth. They constitute about $99\frac{1}{2}$ per cent. of the atmosphere. Mixed with these atoms we have others of a totally different character; we have the molecules, or atomic groups, of carbonic acid, of ammonia, and of aqueous vapour. In these substances diverse atoms have coalesced to form little systems of atoms. The molecule of aqueous vapour, for example, consists of two atoms of hydrogen united to one of oxygen; and they mingle as little triads among the monads of oxygen and nitrogen which constitute the great mass of the atmosphere.

These atoms and molecules are separate; but in what sense? They are separate from each other in the sense in which the individual fishes of a shoal are separate. The shoal of fish is embraced by a common medium, which connects the different members of the shoal, and renders intercommunication between them possible. A medium also embraces our atoms; within our atmosphere exists a second, and a finer atmosphere, in which the atoms of oxygen and nitrogen hang like suspended grains. This finer atmosphere unites not only atom with atom, but star with star; and the light of all suns and of all stars is in reality a kind of music propagated through this interstellar air. This image must be clearly seized; and then we have to advance a step. We must not only figure our atoms suspended in this medium, but we must figure them vibrating in it. In this motion of the atoms consists what we call their heat. "What is heat to us," as Locke has perfectly expressed it, "is in the body heated nothing but motion." Well, we must figure this motion communicated to the medium in which the atoms swing, and sent in ripples through it with inconceivable velocity to the bounds of space. Motion in this form, unconnected with ordinary matter, but speeding through the interstellar medium,

receives the name of Radiant Heat ; and if competent to excite the nerves of vision, we call it Light.

Aqueous vapour was defined to be an invisible gas. Vapour was permitted to issue horizontally with considerable force from a tube connected with a small boiler. The track of the cloud of condensed steam was vividly illuminated by the electric light. What was seen, however, was not vapour, but vapour condensed to water. Beyond the visible end of the jet the cloud resolved itself into true vapour. A lamp was placed under the jet at various points ; the cloud was cut sharply off at that point, and when the flame was placed near the efflux orifice the cloud entirely disappeared. The heat of the lamp completely prevented precipitation. This same vapour was condensed and congealed on the surface of a vessel containing a freezing-mixture, from which it was scraped in quantities sufficient to form a small snowball. The beam of the electric lamp, moreover, was sent through a large receiver placed on an air-pump. A single stroke of the pump caused the precipitation of the aqueous vapour within, which became beautifully illuminated by the beam ; while, upon a screen behind, a richly coloured halo, due to diffraction by the little cloud within the receiver, flashed forth.

The waves of heat speed from our earth through our atmosphere towards space. These waves dash in their passage against the atoms of oxygen and nitrogen, and against the molecules of aqueous vapour. Thinly scattered as these latter are, we might naturally think meanly of them as barriers to the waves of heat. We might imagine that the wide spaces between the vapour-molecules would be an open door for the passage of the undulations, and that, if those waves were at all intercepted, it would be by the substances which form $99\frac{1}{2}$ per cent. of the whole atmosphere. Three or four years ago, however, it was found by the speaker that this small modicum of aqueous vapour intercepted fifteen times the quantity of heat stopped by the whole of the air in which it was diffused. It was afterwards found that the dry air then experimented with was not perfectly pure, and that the purer the air became the more it approached the character of a vacuum, and the greater, by comparison, became the action of the aqueous vapour. The vapour was found to act with 30, 40, 50, 60, 70 times the energy of the air in which it was diffused ; and no doubt was entertained that the aqueous vapour of the air which filled the Royal Institution theatre during the delivery of the discourse, absorbed 90 or 100 times the quantity of radiant heat which was absorbed by the main body of the air of the room.

Looking at the single atoms, for every 200 of oxygen and nitrogen there is about 1 of aqueous vapour. This 1, then, is

80 times more powerful than the 200; and hence, comparing a single atom of oxygen or nitrogen with a single atom of aqueous vapour, we may infer that the action of the latter is 16,000 times that of the former. This was a very astonishing result, and it naturally excited opposition, based on the philosophic reluctance to accept a result so grave in consequences before testing it to the uttermost. From such opposition a discovery, if it be worth the name, emerges with its fibre strengthened, as the human character gathers force from the healthy antagonisms of active life. It was urged that the result was on the face of it improbable; that there were, moreover, many ways of accounting for it, without ascribing so enormous a comparative action to aqueous vapour. For example, the cylinder which contained the air in which these experiments were made, was stopped at its ends by plates of rock-salt, on account of their transparency to radiant heat. Rock-salt is hygroscopic; it attracts the moisture of the atmosphere. Thus a layer of brine readily forms on the surface of a plate of rock-salt; and it is well known that brine is very impervious to the rays of heat. Illuminating a polished plate of salt by the electric lamp, and casting, by means of a lens, a magnified image of the plate upon a screen, the speaker breathed through a tube for a moment on the salt; brilliant colours of thin plates (soap-bubble colours) flashed forth immediately upon the screen—these being caused by the film of moisture which overspread the salt. Such a film, it was contended, is formed when undried air is sent into the cylinder; it was therefore the absorption of a layer of brine which was measured, instead of the absorption of aqueous vapour.

This objection was met in two ways. First, by showing that the plates of salt when subjected to the strictest examination show no trace of a film of moisture. Secondly, by abolishing the plates of salt altogether, and obtaining the same results in a cylinder open at both ends.

It was next surmised that the effect was due to the impurity of the London air; and the suspended carbon-particles were pointed to as the cause of the opacity to radiant heat. This objection was met by bringing air from Hyde Park, Hampstead Heath, Primrose Hill, Epsom Downs, a field near Newport in the Isle of Wight, St. Catharine's Down, and the sea-beach near Black Gang Chine. The aqueous vapour of the air from these localities intercepted at least seventy times the amount of radiant heat absorbed by the air in which the vapour was diffused. Experiments made with smoky air proved that the suspended smoke of the atmosphere of West London, even when an east wind pours over it the smoke of the city, exerts only a fraction of the destruc-

tive powers exercised by the transparent and impalpable aqueous vapour diffused in the air.

The cylinder which contained the air through which the calorific rays passed was polished within, and the rays which struck the interior surface were reflected from it to the thermo-electric pile which measured the radiation. The following objection was raised:—You permit moist air to enter your cylinder; a portion of this moisture is condensed as a liquid film upon the interior surface of your tube; its reflective power is thereby diminished; less heat therefore reaches the pile, and you incorrectly ascribe to the absorption of aqueous vapour an effect which is really due to diminished reflexion of the interior surface of your cylinder.

But why should the aqueous vapour so condense? The tube within is warmer than the air without, and against its inner surface the rays of heat are impinging. There can be no tendency to condensation under such circumstances. Further, let five inches of undried air be sent into the tube, that is, one-sixth of the amount which it can contain. These five inches produce their proportionate absorption. The driest day, on the driest portion of the earth's surface, would make no approach to the dryness of our cylinder when it contains only five inches of air. Make it 10, 15, 20, 25, 30 inches: you obtain an absorption exactly proportional to the quantity of vapour present. It is next to a physical impossibility that this could be the case if the effect were due to condensation. But lest a doubt should linger in the mind, not only were the plates of rock-salt abolished, but the cylinder itself was dispensed with. Humid air was displaced by dry, and dry air by humid in the free atmosphere; the absorption of the aqueous vapour was here manifest, as in all the other cases.

No doubt, therefore, can exist of the extraordinary opacity of this substance to the rays of obscure heat; and particularly such rays as are emitted by the earth after it has been warmed by the sun. It is perfectly certain that more than ten per cent. of the terrestrial radiation from the soil of England is stopped within ten feet of the surface of the soil. This one fact is sufficient to show the immense influence which this newly-discovered property of aqueous vapours must exert on the phenomena of meteorology.

This aqueous vapour is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer-night the aqueous vapour from the air which overspreads this country, and you would assuredly destroy every plant capable of being destroyed by a freezing temperature. The warmth of our fields and gardens would pour itself unre-

quited into space, and the sun would rise upon an island held fast in the iron grip of frost. The aqueous vapour constitutes a local dam, by which the temperature at the earth's surface is deepened: the dam, however, finally overflows, and we give to space all that we receive from the sun.

The sun raises the vapours of the equatorial ocean; they rise, but for a time a vapour screen spreads above and around them. But the higher they rise, the more they come into the presence of pure space, and when, by their levity, they have penetrated the vapour screen, which lies close to the earth's surface, what must occur?

It has been said that, compared atom for atom, the absorption of an atom of aqueous vapour is 16,000 times that of air. Now the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapour will therefore radiate with 16,000 times the energy of an atom of air. Imagine then this powerful radiant in the presence of space, and with no screen above it to check its radiation. Into space it pours its heat, chills itself, condenses, and the tropical torrents are the consequence. The expansion of the air, no doubt, also refrigerates it; but in accounting for those deluges, the chilling of the vapour by its own radiation must play a most important part. The rain quits the ocean as vapour; it returns to it as water. How are the vast stores of heat, set free by the change from the vaporous to the liquid condition, disposed of? Doubtless in great part they are wasted by radiation into space. Similar remarks apply to the cumuli of our latitudes. The warmed air, charged with vapour, rises in columns, so as to penetrate the vapour screen which hugs the earth; in the presence of space, the head of each pillar wastes its heat by radiation, condenses to a cumulus, which constitutes the visible capital of an invisible column of saturated air.

Numberless other meteorological phenomena receive their solution by reference to the radiant and absorbent properties of aqueous vapour. It is the absence of this screen, and the consequent copious waste of heat, that causes mountains to be so much chilled when the sun is withdrawn. Its absence in Central Asia renders the winter there almost unendurable; in Sahara the dryness of the air is sometimes such that, though during the day "the soil is fire and the wind is flame," the chill at night is painful to bear. In Australia, also, the thermometric range is enormous, on account of the absence of this qualifying agent. A clear day, and a dry day, moreover, are very different things. The atmosphere may possess great visual clearness, while it is charged with aqueous vapour; and on such occasions great chilling cannot occur by terrestrial radiation. Sir

John Leslie and others have been perplexed by the varying indications of their instruments on days equally bright; but all these anomalies are completely accounted for by reference to this newly-discovered property of transparent aqueous vapour. Its presence would check the earth's loss; its absence, without sensibly altering the transparency of the air, would open wide a door for the escape of the earth's heat into infinitude.

XXVIII. *Theorems relating to the Canonic Roots of a Binary Quintic of an Odd Order.* By A. CAYLEY, Esq.*

I CALL to mind Professor Sylvester's theory of the canonical form of a binary quintic of an odd order; viz., the quintic of the order $2n+1$ may be expressed as a sum of $\overline{n+1}$ $(2n+1)$ th powers, the roots of which, or say the *canonic roots* of the quintic, are to constant multipliers *près* the factors of a certain covariant derivative of the order $(n+1)$, called the *Canonizant*. If, to fix the ideas, we take a quintic function, then we may write

$(a, b, c, d, e, f \curvearrowright x, y)^5 = A(lx + my)^5 + A'(l'x + m'y)^5 + A''(l''x + m''y)^5$
 (it would be allowable to put the coefficients A each equal to unity; but there is a convenience in retaining them, and in considering that a canonic root $lx + my$ is only given as regards the ratio $l : m$, but that the coefficients l, m remain indeterminate); and then the canonic roots $(lx + my)$, &c. are the factors of the Canonizant

$$\left| \begin{array}{cccc} y^3, & -y^2x, & yx^2, & -x^3 \\ a, & b, & c, & d \\ b, & c, & d, & e \\ c, & d, & e, & f \end{array} \right|$$

It is to be observed that this reduction of the quintic to its canonical form, *i. e.* to a sum of $\overline{n+1}$ $(2n+1)$ th powers, is a *unique* one, and that the quintic cannot be in any other manner a sum of $\overline{n+1}$ $(2n+1)$ th powers.

Prof. Sylvester communicated to me, under a slightly less general form, and has permitted me to publish the following theorems:—

1. If the second emanant $(X\partial_x + Y\partial_y)^2 U$ has in common with the quintic U a single canonic root, then all the canonic roots of the emanant are canonic roots of the quintic; and, moreover, if the remaining canonic root of the quintic be $rx + sy$, then (X, Y) , the facients of emanation, are $= (s, -r)$, or, what is the same thing, they are given by the equation

$$\text{canont. } U(X, Y \text{ in place of } x, y) = 0.$$

* Communicated by the Author.

In fact, considering, as before, the quintic $U = (a, b, c, d, e, f) \chi(x, y)^5$, we have

$$U = A(lx + my)^5 + A'(l'x + m'y)^5 + A''(l''x + m''y)^5,$$

and thence

$$(X\partial_x + Y\partial_y)^2 U = B(lx + my)^3 + B'(l'x + m'y)^3 + B''(l''x + m''y)^3,$$

if for shortness $B = 6.5(lX + mY)^2 A$, &c.

Suppose $(X\partial_x + Y\partial_y)^2 U$ has in common with U the canonic root $lx + my$, then

$$(X\partial_x + Y\partial_y)^2 U = C(lx + my)^3 + C'(px + qy)^3,$$

and thence

$$B'(l'x + m'y)^3 + B''(l''x + m''y)^3 = (C - B)(lx + my)^3 + C'(px + qy)^3,$$

which must be an identity; for otherwise we should have the same cubic function expressed in two different canonical forms.

And we may write

$$B' = C', \quad l'x + m'y = px + qy, \quad B'' = 0, \quad C = B,$$

and then we have

$$(X\partial_x + Y\partial_y)^2 U = B(lx + my)^3 + B'(l'x + m'y)^3;$$

so that all the canonic roots of the emanant are canonic roots of the quantic. Moreover, the condition $B'' = 0$ gives $l''X + m''Y = 0$, that is, $X : Y = m'' : -l''$, or writing $rx + sy$ instead of $l''x + m''y$, $X : Y = s : -r$; and the system is

$$U = A(lx + my)^5 + A'(l'x + m'y)^5 + A(rx + sy)^5,$$

$$(s\partial_x - r\partial_y)^2 U = B(lx + my)^3 + B'(l'x + m'y)^3,$$

which proves the theorem.

2. The two functions, canont. U , canont. $(X\partial_x + Y\partial_y)^2 U$, have for their resultant {canont. $U(X, Y$ in place of $x, y)$ }²ⁿ, if $2n + 1$ be the order of U .

In fact, in order that the equations

$$\text{canont. } U = 0, \quad \text{canont. } (X\partial_x + Y\partial_y)^2 U = 0,$$

may coexist, their resultant must vanish; and conversely, when the resultant vanishes, the equations will have a common root. Now if the equation canont. $(X\partial_x + Y\partial_y)^2 U = 0$ has a common root with the equation canont. $U = 0$, all its roots are roots of canont. $U = 0$; and, moreover, if $rx + sy = 0$ be the remaining root of canont. $U = 0$, then $X : Y = s : -r$, that is, we have

$$\text{canont. } U(X, Y \text{ in place of } x, y) = 0;$$

or the resultant in question can only vanish if the last-mentioned equation is satisfied. It follows that the resultant must be a

power of the *nilfactum* of the equation; and observing that canont. U is of the form $(a, \dots)^{n+1}(x, y)^{n+1}$, *i. e.* that it is of the degree $n+1$ as well in regard to the coefficients as in regard to the variables (x, y) , it is easy to see that the resultant is of the degree $2n(n+1)$ as well in regard to the coefficients as in regard to (X, Y) ; that is, we have $2n$ as the index of the power in question.

3. In particular, if $Y=0$, the theorem is that the resultant of the functions canont. U, canont. $\partial_x^2 U$ is equal to the $2n$ th power of the first coefficient of canont. U.

Thus for $n=1$, that is, for the cubic function $(a, b, c, d)(x, y)^3$, we have

$$\begin{aligned} \text{canont. } U &= \begin{vmatrix} y^2, & -xy, & x^2 \\ a, & b, & c \\ b, & c, & d \end{vmatrix} \\ &= (ac-b^2, \quad ad-bc, \quad bd-c^2)(x, y)^2, \\ \text{canont. } \partial_x^2 U &= \begin{vmatrix} y, & -x \\ a, & b \end{vmatrix} = ax + by. \end{aligned}$$

And the resultant of the two functions is

$$\begin{aligned} &= (ac-b^2, \quad ad-bc, \quad bd-c^2)(b, -a)^2 \\ &= -(ac-b^2)^2, \end{aligned}$$

which verifies the theorem.

The theorems were, in fact, given to me in relation to the quantic U and the second differential coefficient $\partial_x^2 U$; but the introduction instead thereof of the second emanant $(X\partial_x + Y\partial_y)^2 U$ presented no difficulty.

2 Stone Buildings, W.C.,
February 16, 1863.

XXIX. *Experiments on Ozone.* By M. SORET.
In a Letter to Professor TYNDALL.*

YOU have perhaps remarked that I have made a note of one or two observations relative to *ozone*. This leads me to say a few words to you on some experiments which I have since then made on this subject in M. Bunsen's laboratory. I had formerly ascertained (*Archive*, 1854, vol. xxv. p. 263) that the quantity of ozone is greatly increased when the voltameter is cooled, as you have also found. I had made some analyses by arsenious acid, but the method was not perfect. I resumed experiments of the same kind, using M. Bunsen's method with sulphurous acid and a standard solution of iodide of potassium.

* Communicated by Professor Tyndall.

My first idea was to try and ascertain whether less or more ozone was produced in light than in darkness. I found no appreciable difference, but I found that, with the voltameter which I used, I produced even at ordinary temperatures very considerable proportions of ozone. I worked with a voltameter of 500 to 700 cubic centims. capacity, filled with acidulated water (one volume of $\text{SO}^3 \text{HO}$ to five volumes of water), with very fine wires of platinum-iridium for electrodes; the negative electrode was surrounded by a porous cell, and the gases were consequently not mixed. I obtained at ordinary temperatures, much higher than that of 0° , a quantity of ozone varying with the circumstances, but amounting almost to 1 per cent. for the whole of the oxygen disengaged. The gas appeared to be capable of being dried by passing through sulphuric acid without appreciable loss of ozone. By surrounding the voltameter with a mixture of ice and salt, and allowing the oxygen to pass directly into iodide of potassium, I obtained a quantity of iodine corresponding to 20 milligrammes of oxygen; the oxygen collected, after passing through the iodide, filled a flask of 720 cubic centims. capacity. If, then, we assume that the 20 milligrammes of oxygen absorbed represent ozone, we obtain a proportion of $\frac{2}{105}$.

By collecting the gas under water, and absorbing with iodide of potassium after all the gas has been disengaged, less oxygen is found, because water dissolves a considerable proportion (about one-fifth of the ozone liberated in an experiment in which the water was analysed).

These proportions are then very appreciable; and the essential conditions for obtaining them appear to be—

1. The use of large voltameters, to avoid the heating by the passage of the current, and perhaps a perturbing action of the oxygenized water.

2. The separation of the gases.

3. The use of electrodes of platinum-iridium exercises possibly also an influence.

4. The cooling of the voltameter.

5. The use of a sufficiently concentrated solution of sulphuric acid.

I availed myself of the facility of the production of ozone to repeat Baumert's fundamental experiment, in which dried electrolytic oxygen, passed into a glass tube coated with a slight layer of anhydrous phosphoric acid, dissolves this deposit if the tube has been heated at one point—an experiment which, according to Baumert, proves that ozone contains hydrogen. In operating with a voltameter in which the two gases were disengaged (they were separated by a porous cell), I found in fact

Phil. Mag. S. 4. Vol. 25. No. 167. *March* 1863. P

that water was formed by heating the disengaged oxygen. But by effecting the decomposition in a large vessel containing acidulated water in which the positive electrode was placed, and a large porous cell filled with sulphate of copper in which the negative electrode was placed, so that there was no disengagement of hydrogen, there was not the smallest trace of water in the oxygen after its passage through the heated tube. I prove, then, that in Baumert's experiment, as M. Marignac had supposed, it is an imperfect separation of the gases which has led him to a conclusion different from that usually admitted.

I continue to work at this subject, but I have thought that these first results might possess some interest for you, and hence I have taken the liberty of communicating them to you before my investigation is terminated and ready for publication.

XXX. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from vol. xxiv. p. 531.]

LAMY* has communicated the following additional observations on thallium. The metal has as little tenacity as malleability; its density is 11·862, and its equivalent 204; its specific heat, according to Regnault†, is 0·03355. It is diamagnetic, and a bad conductor of heat and electricity. The aqueous solutions of its salts are precipitated neither by alkalis nor alkaline carbonates, nor by dilute solution of red or yellow prussiate of potash. Hydrochloric acid produces a white precipitate of difficultly soluble chloride; iodide of potassium and chloride of platinum yield a yellow iodide, and a double chloride still more insoluble.

Sulphuretted hydrogen produces no precipitate in acid solutions, and only a partial one in neutral solutions; it precipitates the metal as black sulphide from alkaline solutions. Zinc precipitates the metal from its various solutions, in brilliant laminae.

Thallium forms with oxygen two oxides. The *protoxide* is soluble in water, which it renders alkaline and caustic; it absorbs carbonic acid from the air, forming a carbonate insoluble in alcohol. In the solid state the oxide is yellow or black, according as it is hydrated or not. Its colourless solution, evaporated *in vacuo*, deposits long bundles of yellowish prismatic needles, which blacken as evaporation proceeds, so that at a certain stage of desiccation there is a curious mixture of yellow and black crystals. The protoxide melts above 300° to a brown volatile liquid which attacks porcelain, taking from it a portion of silica.

* *Comptes Rendus*, December 8, 1862.

† *Ibid.* December 16, 1862.

When the dry protoxide is heated with absolute alcohol it is dissolved, and a *thallic alcohol* formed analogous to potassic alcohol. It is a limpid oil of the specific gravity 3.50, and scarcely less refrangible than bisulphide of carbon. It is little soluble in alcohol, and is decomposed by water, forming pure protoxide of thallium.

When thallium is completely burned in oxygen, it forms an insoluble black oxide of the composition ThO^3 . This melts at a red heat and disengages oxygen. It forms salts with hydrochloric, sulphuric, and nitric acids, which are not very stable.

When sesquichloride of thallium is treated with potash, a brown oxide is precipitated, and protochloride of thallium remains in solution. This oxide unites with acids more readily than the black oxide, from which it differs by containing an equivalent of water.

The *carbonate of thallium*, ThO CO^2 , dissolves to the extent of about 5 per cent. in water at the ordinary temperature, and to about 22 in water at 100°C . It crystallizes in long, flattened, prismatic needles, readily fusible to a grey mass whose density is 7.06.

The sulphate, ThO SO^3 , crystallizes in beautiful oblique rhombic prisms, somewhat less soluble than the carbonate.

The *nitrate*, ThO NO^5 , is the most soluble of the salts of thallium. 100 grms. of water at 18° dissolve 9.75 grms., and 580 grms. at a temperature of 107° .

Thallium appears to form three chlorides. The most stable is the *protochloride*, ThCl , which has great analogy with chloride of silver, except in the matter of solubility. The *sesquichloride of thallium*, $\text{Th}^2 \text{Cl}^3$, forms beautiful yellow hexagonal plates, soluble without decomposition in acidulated water to the extent of about 5 per cent. It melts at 400° to a brown volatile liquid, and solidifies to a yellowish-brown mass, the density of which is 5.90.

Both these chlorides appear capable of absorbing 1 to 1.5 equivalent of chlorine to form unstable perchlorides, which heat alone decomposes.

Although the metal thallium has, by means of the spectro-scope, been found to exist in several specimens of pyrites from different sources, yet Böttger, who had tried to detect its presence in a great many samples of the deposits in sulphuric acid works in which pyrites is burned, was not able to do so; Kuhlmann, in the deposits from whose works Lamy extracted thallium, describes* their exceptional construction, which permitted the accumulation of large quantities of this substance.

* *Comptes Rendus*, January 26, 1863.

The sulphuric acid from the combustion of pyrites contains arsenic. To get rid of this, Kuhlmann placed a supplementary chamber before the ordinary chambers in which the sulphurous acid is converted into sulphuric acid, which was so large that the gases becoming cooled deposited not only substances mechanically carried away, but also volatile substances readily condensed, and especially arsenious acid. After some months of a combustion of about 3000 kilogrammes of pyrites per diem, considerable masses of arsenic and selenium are deposited, along with mercury and thallium, amounting in some parts of the deposit to $\frac{1}{2}$ per cent.

In works where this plan is not adopted, any thallium which might be contained in pyrites would mix with the sulphate of lead covering the floor of the first chamber, and would dissolve in the sulphuric acid as fast as it was formed, so that the deposits of sulphate of lead would contain traces of this substance so small as to be inappreciable even to the spectroscope.

The following observations on the preparation of rubidium have been communicated by Prof. Bunsen in a letter to Prof. Dumas*.

The material for the purpose was extracted from the lepidolite residues left in the preparation of lithium. In separating caesium from rubidium, use was made of the difference in solubility of the neutral tartrate of caesium (which is deliquescent), and of the bitartrate of rubidium, which is very little soluble †.

The reduction of carbonate of rubidium by charcoal is more difficult than that of sodium, and less so than that of potassium. To effect it, the following mixture was heated in a potassium-furnace: $89\frac{1}{2}$ parts of bitartrate of rubidium, $8\frac{1}{2}$ parts of tartrate of lime, and 2 parts of lampblack from turpentine. The metal was received in a vessel containing naphtha; 5 grammes of metal were obtained from 75 grammes of bitartrate.

The metal melts at $38^{\circ}5$, and its specific gravity is 1.516. According to recent determinations at Heidelberg, sodium melts at $95^{\circ}6$, potassium at $62^{\circ}5$, and lithium at 180° .

Like potassium, rubidium burns on water with a rotatory motion, and in its other properties has the greatest analogy with that element.

M. Dietzenbacher ‡ has observed that the presence of a small quantity of chlorine, bromine, or iodine modifies the properties of sulphur to a remarkable extent. When a mixture of 400 parts of sulphur and 1 part of iodine is heated to about 180° ,

* *Comptes Rendus*, January 26, 1863.

† See also Mr. Allen's paper on the subject at p. 189 of the present Number.

‡ *Ibid.* January 5, 1863.

on cooling, a sulphur is produced which remains for a long time elastic, and which, when poured on a glass or porcelain plate, is obtained in flexible sheets. The same change is produced by iodide of potassium, or by even a smaller proportion of iodine. The sulphur thus prepared is insoluble in bisulphide of carbon.

The action of 1 per cent. of bromine at a temperature of 200° is similar; but the sulphur, instead of being black and having a metallic lustre, has the colour of yellow wax, and is much softer than the foregoing variety: about 75 to 80 per cent. are insoluble in bisulphide.

Chlorine passed over sulphur at 240° changes it into a kind of soft sulphur, which can be readily drawn out and the parts stuck together. It contains a rather larger proportion of sulphur soluble in bisulphide than that treated by bromine. After having been worked up for an hour or two it suddenly hardens, and then becomes quite insoluble in bisulphide of carbon.

M. Kämmerer* has made the following observations on the isolation of fluorine. In a perfectly dry tube, iodine was introduced along with a small tube closed with a stopper and filled with fluoride of silver in excess. The tube was sealed after expelling all air by the vapour of iodine, and then, having broken the small tube in the interior, the apparatus was heated to 70° or 80°. At the expiration of twenty-four hours all the iodine had disappeared, the tube was perfectly transparent, and its contents colourless. The tube was opened under mercury, and the gas transferred to a eudiometer, in which it was rapidly absorbed by a fragment of potash. After absorbing the gas, no trace of silica or iodine could be found in the liquid. The oxygen displaced by the fluorine had combined with potash or water, to form peroxide either of potassium or hydrogen. The tube was not at all attacked. The author believes he has isolated fluorine, and he mentions Davy's statement that fluorine does not attack glass, and could be transferred over mercury. He proposes to repeat the experiment with bromine.

Marignac† has communicated the abstract of a long series of researches on the tungstates, fluotungstates, and silicotungstates. He agrees with Scheibler‡ in considering that there are only two varieties of tungstic acid—one insoluble, forming ordinary tungstates precipitable by acids, and the other soluble, forming soluble metatungstates not precipitable by acids.

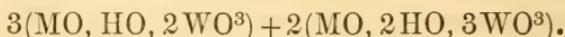
The constitution of the metatungstates is simple, and agrees with the formula $MO_4WO + xAq$. The neutral tungstates are

* *Journal für praktische Chemie*, vol. lxxxv. p. 452. *Répertoire de Chimie*, January 1863.

† *Comptes Rendus*, December 15, 1862. ‡ *Phil. Mag.*, vol. xx. p. 374.

represented by the formula $MOWO^3$; but there are numerous acid salts with various and complex constitutions, although all containing $2\frac{1}{3}$ to $2\frac{1}{2}$ equivalents of acid for one of base. These are probably complex compounds of bitungstates and tritungstates.

Among these various salts, which are decomposed by frequent solutions and recrystallizations, there is one class distinguished by their great stability, and which Laurent called the *paratungstates*. He believed the ratio of the base to acid was as 5 : 12. More recently Lotz was led to propose the ratio 3 : 7, which view Scheibler adopted. Marignac's analyses have led him to support Laurent's views, or rather to consider them as double salts, and, allowing for the water in their composition, formulate them thus :



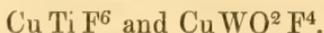
Marignac, like Berzelius, has not been able to obtain true fluotungstates: the salts obtained by treating tungstates with hydrofluoric acid contain oxygen, and may be considered as compounds of tungstates and fluotungstates, and represented by the general formula



In the study of the crystalline form of the fluoxytungstates, one curious result has been noted, namely, that the fluoxytungstate of copper is isomorphous with the group containing the fluosilicate, fluostannate, and fluotitanate of the same metal. With the ordinary formulæ,



this isomorphism appears at first sight inexplicable: But this anomaly disappears when it is considered that isomorphism only obtains in formulæ when these represent the atomic composition of a body, and not their equivalent constitution. Now fluorine being a biatomic element, it must be doubled, and thus the formulæ of these compounds become



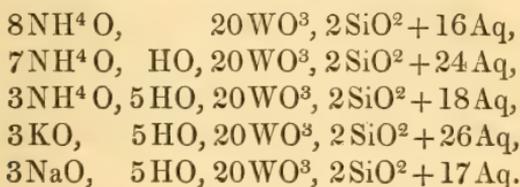
This curious isomorphism justifies to a certain point the conclusions, that fluorine can in certain cases replace oxygen atom for atom, spite of the non-equivalence of their atoms; and also that Berzelius's mode of regarding the fluoxytungstates, or salts of tungstates and fluotungstates, cannot be retained, for the isomorphism of these salts with the fluotitanates cannot be thereby explained.

The silicotungstates are a new class of salts, but must have been obtained by most chemists who have worked at the tung-

states, for they are contained in the mother-liquor from the preparation of the latter. They are easily obtained by boiling the solutions of the acid tungstates with gelatinous silica. The potash-, soda-, and ammonia-salts are all as yet studied. They are soluble, and crystallize well. In many respects their solutions comport themselves like those of the metatungstates. They are very stable, and are not decomposed by treatment with hydrochloric acid, and are only converted into acid salts. By fusion with alkalis they are converted into a mixture of tungstates and silicates. They all appear to contain ten equivalents of tungstic to one of silicic acid, and the neutral salts contain four equivalents of base. These ratios it is convenient to double, and represent the neutral silicotungstates by the general formula



so as to include in this formula the acid and double salts. The following are some of those which Marignac has already analysed:—



M. Persoz, jun.*, has found that silk rapidly dissolves in a hot concentrated solution of chloride of zinc, and more slowly in the cold. The solution used marked 60° in the areometer, and was boiled with oxide of zinc, so that it was basic; it became cloudy on the addition of water from the precipitation of a basic chloride. The solution thus prepared dissolves silk without attacking either wool or vegetable fibres. In any mixture of the three the silk may be dissolved by chloride, and the wool destroyed by soda, so as to preserve only the vegetable fibres. It thus affords a means of discriminating the complex nature of certain tissues.

Chloride of zinc can dissolve such a quantity of silk as to be entirely viscous, and capable of being drawn out like a thick syrup, resembling a concentrated solution of gum-arabic. Ammonia produces, in a solution of the silk diluted with water, a precipitate readily soluble in an excess of the reagent.

Persoz submitted this solution, diluted with water acidulated by hydrochloric acid, to the action of the dialyser. The chloride of zinc passed in large quantity; the liquid became more viscous and then increased in volume, forming an opaline jelly like starch paste. It still contained a small quantity of zinc, from which it

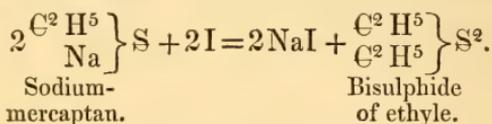
* *Comptes Rendus*, December 22, 1862.

could not be quite freed. In this condition it was quite soluble in acetic acid; but when dried, it could be broken into vitreous fragments, and was no longer soluble in that reagent.

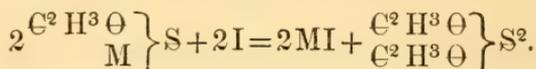
By employing a more dilute solution of the silk and the application of heat, the solution was obtained quite clear; all the chloride of zinc could then be separated by the dialyser, and an insipid colourless liquid was finally obtained, which on evaporation left a golden brittle varnish.

Kekulé and Linnemann* have investigated the action of iodine on some organic sulphur-compounds.

When iodine is added to a solution of sodium-mercaptan, a brisk action takes place, the colour of the iodine disappears, and an oily layer separates on the surface which has all the properties of bisulphide of ethylene. The reaction may be thus expressed:—



The action of iodine on the thiocetates of sodium, potassium, and barium was also investigated. The reaction is in all cases the same; an iodide of the metal is formed and *bisulphide of acetylene*, thus



The raw product from this reaction contains some free sulphur, resulting from an action of water on the bisulphide. It is purified by washing with water, drying over CaCl, and filtering. A product is thus obtained which, in winter, gradually solidifies to a crystalline mass. This substance still contains free sulphur; it is accordingly exposed to a temperature of 15°, upon which it partially crystallizes; the liquid is poured off from the crystals, which are then melted and again allowed to crystallize, and the process is repeated several times. Finally, the crystals are dissolved in a small quantity of bisulphide of carbon, the solution cooled, and a crystal of bisulphide of acetylene added, on which large colourless transparent crystals gradually form.

Pure bisulphide of acetylene melts at 20°; it has a peculiar, very slightly hepatic odour. It is insoluble in water, but readily so in alcohol, ether, and bisulphide of carbon. It is decomposed by water, with formation of thiocetic acid and separation of free sulphur. It is decomposed with violence by strong nitric acid. It is also decomposed by heat. When distilled, thiocetic acid passes over at 93°, the temperature gradually rises to 160°; the

* Liebig's *Annalen*, September 1862.

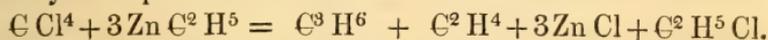
distillate is continually of a deeper colour, and in the retort a mixture of sulphur and charcoal remains.

Bisulphide of acetylene was treated with mercury in the anticipation of removing sulphur and forming free acetylene; a metallic sulphide was formed, but an organic product free from sulphur could not be obtained.

The facility with which zincethyle is prepared by the method described by Rieth and Beilstein*, has led these chemists† to try several reactions with it. In the expectation of forming an allyle from an ethyle compound, they investigated the action of chloride of carbon on zincethyle, which might take place in the following manner:—

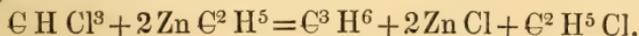


This anticipation, however, was not realized; ethylene and propylene gases were obtained. The action is very energetic, and only small quantities of the chloride must be added at once; by carefully regulating the temperature, a continuous disengagement of gas is obtained. To free the gas from chloride of ethyle, it is passed through sulphide of potassium and potash; it is then absorbed by bromine. When the action is complete, the bromine is treated by caustic potash and then washed with water, upon which a mixture in equivalent quantities of bromide of ethylene and bromide of propylene is obtained. In reference to this, the authors were enabled to confirm an observation which had been already made by Bauer, namely, that it is impossible to separate the two by fractional distillation. The boiling-point rose at once to 135°, and between this temperature and 139° the whole liquid passed over. The analysis of the product gave numbers which are exactly intermediate between those required by bromide of ethylene and bromide of propylene. The reaction may be expressed as follows:—

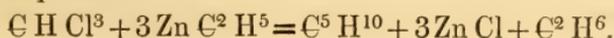


Chloride of carbon. Zincethyle. Propylene. Ethylene.
of carbon.

The action of chloroform on zincethyle was tried, in the expectation that it might take place as follows, and produce propylene gas:—



Some propylene was indeed formed, but mixed with ethylene, from which it could not be freed. But the principal product of the reaction consisted of *amylenes*, the formation of which may be thus expressed:—



* Phil. Mag. vol. xxiv. p. 306. † Liebig's *Annalen*, November 1862.

The action is much less energetic than that of chloride of carbon, and requires to be aided by heat. The amylene was absorbed by bromine, so as to convert it into bromide; and the authors confirm the observations made by other chemists as to the difficulty of obtaining this body pure, inasmuch as it is mixed with other bromides, and is partially decomposed by distillation.

Sesquichloride of carbon, C^2Cl^6 , acts very energetically on zincethyle, and is hereby reduced to the protochloride of carbon, C^2Cl^4 . This latter body appears to be without action on zincethyle.

According to Fittig*, when one equivalent of benzole is heated with 2 of bromine, a slight elevation of temperature ensues, and a brown liquid is formed which continually gives out hydrobromic acid. When the liquid is allowed to stand for about eight days, then treated with potash, washed, and rectified along with some unaltered benzole and a small quantity of crystalline dibromobenzole, monobromobenzole passes over amounting to about three-fourths of the benzole taken, and with a constant boiling-point between 152° and 154° .

Monobromobenzole is a very indifferent body; it can be boiled for days with caustic potash without alteration. It is also without action upon cyanide of potassium or acetate of silver.

It is, however, acted upon by sodium: when to an ethereal solution of monobromobenzole sodium is added, a bluish-black coating is rapidly formed upon it, which becomes heavy and sinks, and at the same time the ether becomes heated to ebullition. When the action is finished, the mass is treated with ether so long as anything is extracted, the ethereal solution filtered, and the ether distilled off, upon which a yellowish coloured oil is left, which at a higher temperature passes over colourless and solidifies in the receiver. This body is the radical phenyle, $\text{C}^{12}\text{H}^{10}$, or $\left. \begin{array}{l} \text{C}^6\text{H}^5 \\ \text{C}^6\text{H}^5 \end{array} \right\}$, and it stands to the alcohol, $\text{C}^6\text{H}^6\Theta$, homologous with benzoic alcohol, in the same relation as ethyle, $\left. \begin{array}{l} \text{C}^2\text{H}^5 \\ \text{C}^2\text{H}^5 \end{array} \right\}$, to ethylic alcohol.

Phenyle crystallizes from its alcoholic solution in large colourless transparent, brilliant laminae, which are endowed with a brilliant lustre. It melts at $70^\circ.5$, and boils at about 245° .

In a subsequent investigation †, Fittig has examined some of the derivatives of this body. When strong nitric acid is poured over small quantities of phenyle, this body becomes transiently black, and then dissolves in the acid with violent action. On

* Liebig's *Annalen*, vol. xxi. p. 362.

† *Ibid.* December 1862.

cooling, the mass solidifies to a thick crystalline magma, which is filtered through gun-cotton, and, when purified, is obtained in long colourless needles melting at 213° . This is *dinitrophenyle*, $\left. \begin{array}{l} \text{C}^6 \text{H}^4 \text{N}\Theta^2 \\ \text{C}^6 \text{H}^4 \text{N}\Theta^2 \end{array} \right\}$. When an alcoholic solution of this substance is treated with sulphuretted hydrogen, a red liquid is obtained containing two bases, *Amidonitrophenyle* and *Diamidophenyle*, the quantity of which depends on the extent to which the action of sulphuretted hydrogen has been carried: if this has been continued for a long time, the latter compound is almost exclusively obtained; but if sulphuretted hydrogen has only been passed into the cold liquid until it has dissolved up clear, the former compound is obtained. They may be separated by using their different solubility in water, as diamidophenyle only is soluble in hot water; or it may be precipitated from the solution as sulphate by the addition of dilute sulphuric acid.

Amidonitrophenyle, $\left. \begin{array}{l} \text{C}^6 \text{H}^4 (\text{N H}^2) \\ \text{C}^6 \text{H}^4 (\text{N}\Theta^2) \end{array} \right\}$, when crystallized from alcohol, forms small red acicular crystals which melt at 160° , and at a higher temperature volatilize under partial decomposition. It is almost entirely insoluble both in hot and cold water, but readily so in alcohol. It does not combine with acids, but its hydrochloric solution yields, when precipitated by bichloride of platinum, a very unstable platinum-salt.

Diamidophenyle, $\left. \begin{array}{l} \text{C}^6 \text{H}^4 (\text{N H}^2) \\ \text{C}^6 \text{H}^4 (\text{N H}^2) \end{array} \right\}$, is a base, and has the same composition as Zinin's benzidine, $\text{C}^{12} \text{H}^{12} \text{N}^2$; and a comparison of the properties of the free base, as well as of its well-defined sulphate and oxalate, left no doubt as to its identity with that substance.

If, in the above preparation of dinitrophenyle, the acid solution filtered off from the crystalline magma is treated with water, a semisolid mass is separated, which by repeated crystallization from alcohol may be separated into a fluid mass, which is nitrobenzole, and into a crystalline substance. When purified by repeated crystallization from alcohol, this is obtained in brilliant, hard, brittle prisms an inch in length, which have not the least resemblance with dinitrophenyle, from which the body also differs by melting at $93^{\circ}5$; that is, more than 100 degrees lower. Yet this compound has the same composition as dinitrophenyle, and the two bodies are therefore isomeric; Fittig names it *isodinitrophenyle*. The two bodies appear to be always produced at the same time; but the conditions of their formation have not been ascertained. They are probably derivatives of two isomeric hydrocarbons, which by a transposition of their atoms readily pass into one another.

XXXI. *Remarks on an Article entitled "Energy" in 'Good Words.'* By JOHN TYNDALL, F.R.S.*

THE time which I am able to devote to unscientific reading is so stinted as to leave me in almost utter ignorance of the general periodical literature of this country. Hence it is that until the 20th of February I was not aware of the existence of an article in the October Number of a journal called 'Good Words,' from the combined pens of the Professors of Natural Philosophy in Glasgow and in Edinburgh—Professor William Thomson and Professor Tait—in which, though not mentioned by name, I am referred to in a manner which it might be expected would have come to my knowledge long ago. When, however, it is known that the other articles in the number to which I refer, bear such titles as "The Childhood of Jesus," "The Trial Sermon," "The Bands of Love," "At Home in the Scriptures," &c., I think I may be excused if an article on Energy, in the scientific sense of the term, imbedded in such matter as those titles indicate, escaped my attention.

The article referred to contains the following paragraph:—

"Curiously enough, although similar coincidences are common, while Joule was pursuing and publishing his investigations, there appeared in Germany a paper by Mayer, of Heilbronn. Its title is 'Bemerkungen über die Kräfte der unbelebten Natur,' and its date 1842. In this paper the results obtained by previous naturalists are stated with precision (among them the fundamental one of Davy), new experiments are suggested, and a method for finding the mechanical equivalent of heat is propounded †. On the strength of this publication an attempt has

* Communicated by the Author.

† To this portion of their paragraph Messrs. Thomson and Tait have appended the following note, which I leave to the consideration of those who are acquainted with Mayer's publications. I purposely abstain from making any scientific comment on either the article or the note; *moral* considerations alone now interest me. "Mayer's method," writes the northern philosophers, "is founded on the supposition that diminution of the volume of a body implies an evolution or generation of heat; and it involves essentially a false analogy between the natural fall of a body to the earth, and the condensation produced in an elastic fluid by the application of external force. The hypothesis on which he thus grounds a definite numerical estimate of the relation between the agencies here involved, is that the heat evolved when an elastic fluid is compressed and kept cool, is simply the dynamical equivalent of the work employed in compressing it. The experimental investigations of subsequent naturalists have shown that this hypothesis is altogether false for the generality of fluids, especially liquids, and is at best only *approximately* true for air; whereas Mayer's statements imply its indiscriminate application to all bodies in nature, whether gaseous, liquid, or solid, and show no reason for choosing air for the application of the supposed principle to calculation, but that at the time he wrote air was the only body for which the requisite numerical data were known with any approximation to accuracy."

been made to claim for Mayer the credit of being the first to establish in all its generality the principle of the Conservation of Energy. It is true that '*la science n'a pas de patrie,*' and it is highly creditable to British philosophers that they have so liberally acted according to this maxim. But it is not to be imagined that on this account there should be no scientific patriotism, or that, in our desire to do all justice to a foreigner, we should depreciate or suppress the claims of our own countrymen. And it especially startles us, that the recent attempts to place Mayer in a position which he never claimed, and which had long before been taken by another, should have found support within the very walls wherein Davy propounded his transcendent discoveries."

This paragraph—at all events the latter part of it—refers to a Lecture on Force given by me at the Royal Institution on the evening of Friday the 6th of June, 1862. An abstract of the lecture is printed in the 'Proceedings' of the Institution; it is reprinted in the Philosophical Magazine for last July, and it also forms a portion of the Appendix to the 12th Lecture in my book on Heat, which is announced by the Messrs. Longman for the 4th of March.

Many will agree with me in thinking that it would hardly conduce to the interests or the dignity of science, if the habit were to become general of taking difficult and disputed points, which apparently involve imputations on individual character, into such a court as that chosen by Professor Thomson and Professor Tait. It is very laudable and very desirable that men in their high positions should *instruct* the readers of 'Good Words;' but these respectable persons are placed in a false position when they are virtually called upon to decide between the rival claims of Joule and Mayer, and to form an opinion as to the scientific morality of myself. With such an audience authority is, of course, decisive; and hence the practical wisdom of combining two pens in performing the normal work of one. There is, however, another court, in which mere authority is less influential, and in the decisions of which I shall always cheerfully acquiesce. To this court, that of instructed men of science, I now beg to transfer the case opened by the two gentlemen referred to in 'Good Words.'

The precise meaning of the above paragraph, in all its parts, is difficult to define; but its effect is to leave upon the reader's mind a very unpleasant impression regarding the part which I have acted with reference to the claims of Dr. Mayer and Dr. Joule*. Possibly its distinguished authors did not mean to pro-

* The journal in which this impression is conveyed enjoys a monthly circulation of 70,000.

duce this impression. If not, they will perhaps have the kindness to say so in the proper place. If they did, then I trust they will not deem me unreasonable if I ask them to be more explicit. I hereby openly invite them to point out the particulars in which I have erred in judgment or misrepresented facts. I challenge them to do so, not in the spirit of polemical bravado, but because I know my own readiness to make prompt atonement for any wrong that I may have done. I can scarcely allow myself to suppose that the charge implied in the phrase "depreciate or suppress the claims of our own countrymen," is meant to apply to me. But it is at least doubtfully employed, placed as it is in such close proximity to a most pointed reference to myself. The grammatical meaning of the sentence which follows seems to be that "attempts (made by others) to place Mayer in a position which he never claimed," were "supported" by me. As a matter of fact, this is incorrect.

The assertion of Messrs. Thomson and Tait, that Mayer had never claimed the position which I assigned to him, is also incorrect. It is true, indeed, that the "claims" of Mayer have been few and far between; and in this respect his example might, in many cases, be followed with advantage. He recognizes the great merits of Mr. Joule; he says it cannot be denied that he made the independent discovery of the convertibility and equivalence of heat and motion. He dwells with pleasure upon Joule's beautiful researches, and states that the law of equivalence, and its numerical expression, were published almost simultaneously in Germany and in England. As far as I can judge, he desires nothing more than permission to stand beside his more fortunate fellow-labourer in the memory of men. But he will not bear, nor do I think the scientific world will call upon him to bear, removal from the position which he has so fairly won. At page 53 of his last pamphlet, entitled *Bemerkungen über das mechanische Equivalent der Wärme* (Hielbronn, 1851), he writes thus:—"The new subject (the mechanical theory of heat) soon began to excite the attention of learned men; but inasmuch as both at home and abroad the subject has been exclusively treated as a foreign discovery, I find myself compelled to make the claims to which priority entitles me; for, although the few investigations which I have given to the public, and which have almost disappeared in the flood of communications which every day sends forth, without leaving a trace behind, prove, by the very form of their publication, that I am not one who hankers after effect, it is not therefore to be assumed that I am willing to be deprived of intellectual property, which documentary evidence proves to be mine."

In my morning lectures, which extended over three months of

the spring of last year, the name and merits of Mr. Joule were constantly before my audience; and throughout the entire course I made but a single passing allusion to Mayer. In fact, I was then as unacquainted with the real merits of Mayer as Professors Thomson and Tait appear to be now*. But even after I had made myself acquainted with all that Mayer had done, I did not bate a jot in my admiration of Mr. Joule. For a whole month before the above words regarding depreciation and suppression received public utterance, the following passage, with reference to the mechanical theory of heat, stood in the largest type of the *Philosophical Magazine*:—"It is to Mr. Joule, of Manchester, that we are almost wholly indebted for the experimental treatment of this subject. With his mind firmly fixed upon a principle, and undismayed by the coolness with which his first labours appear to have been received, he persisted for years in his attempts to prove the invariability of the relation between heat and ordinary mechanical force. He placed water in a suitable vessel, agitated it by paddles moved by measurable forces, and determined the elevation of temperature; he did the same with mercury and sperm-oil. He also caused discs of cast iron to rotate against each other, and measured the heat produced by their friction. He urged water through capillary tubes, and measured the heat thus generated. The results of his experiments leave no doubt upon the mind, that under all circumstances the absolute amount of heat produced by the expenditure of a definite amount of mechanical force is fixed and invariable."

In my 'Lectures on Heat, considered as a Mode of Motion,' now on the point of publication, I have drawn the following parallel between Joule and Mayer:—

"Do I refer to these things in order to exalt Mayer at the expense of Joule? It is far from my intention to do so. The man who through long years, without encouragement, and in the face of difficulties which might well be deemed insurmountable, could work with such unswerving steadfastness of purpose to so triumphant an issue, is safe from depreciation. And it is not the experiments alone, but the spirit which they incorporate, and the applications which their author made of them, which entitle Mr. Joule to a place in the foremost rank of physical philosophers. Mayer's labours have, in some measure, the stamp of a profound intuition, which rose, however, to the energy of undoubting conviction in the author's mind. Joule's labours, on the contrary, are an experimental demonstration. True to the speculative instinct of his country, Mayer drew large and weighty conclusions from slender premises, while the

* I here, of course, refer to the time when they wrote their article.

Englishman aimed, above all things, at the firm establishment of facts. And he did establish them. The future historian of science will not, I think, place these men in antagonism. To each belongs a reputation which will not quickly fade, for the share he has had, not only in establishing the dynamical theory of heat, but also in leading the way towards a right appreciation of the general energies of the universe."

If this recognition of Mr. Joule will not satisfy my critics, I cannot help it. It is simply a difference of estimate between them and me—a difference which may exist without the least infraction of good faith on either side. There is nothing here to "startle" brave men, or to give the slightest colouring of truth to insinuations regarding "depreciation" and "suppression." However "the walls wherein Davy propounded his transcendent discoveries" may feel the want of his great presence, I trust they still contain men not less anxious than he was to act honourably by their fellows, and who would consider the highest rewards of science too dear if purchased by any deviation from this course of action.

Royal Institution,
Feb. 24, 1863.

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 154.]

May 22, 1862.—Major-General Sabine, President, in the Chair.

THE following communications were read :—
"On the Theory of the Motion of Glaciers." By William Hopkins, Esq., F.R.S.

Almost all the numerous discussions which have taken place during the last twenty years respecting our theories of glacial motion have had for their object the assertion of some particular view, rather than the establishment of a complete and sufficient theory founded on well-defined hypotheses and unequivocal definitions, together with a careful comparison of the results of accurate theoretical investigation with those of direct observation. Each of these views has been regarded, improperly, in the author's opinion, as a *Theory of Glacial Motion*. The Expansion Theory ignored the Sliding Theory, though they were capable of being combined; the latter theory was equally ignored by the Viscous Theory, in which, moreover, instead of the definitions of terms being clear and determinate, no definition of *viscosity* was ever given, though that term designated the fundamental property on which the views advocated by this theory depended. Again, the Regelation Theory is not properly a theory of the motion of glaciers, but a beautiful demonstration of a property

of ice, entirely new to us, on which certain peculiarities of the motions of glaciers depend.

When we shall have obtained a *Theory of the Motion of Glaciers* which shall command the general assent of philosophers, no qualifying epithet will be required for the word *theory*; it would indeed be inappropriate, as seeming to indicate the continued recognition of some rival theory. If, for instance, it should be hereafter admitted that the sliding of a glacier over its bed and the property of regelation in ice are equally necessary, and, when combined, perfectly sufficient to account for the phenomena of glacial motion, there would be a manifest impropriety, not to say injustice, in selecting either of the terms *sliding* or *regelation* by which to designate this combined theory. The author makes these remarks because he believes that the preservation of the partial epithets above mentioned has a tendency to prevent our regarding the whole subject in that more general and collective aspect under which it is one of the principal objects of this paper to present it.

This object must necessarily give to the present paper something of the character of a *résumé* of what has hitherto been done, whether it be our purpose to adopt or reject the conclusions of others. There are periods in the history of almost every science when its sound and healthy progress may almost as much demand the refutation of that which is erroneous as the establishment of that which is true. It is not intended, however, to enter into any review of the past labours of glacialists with respect to exploded theories, but only to notice those more recent researches and speculations which appear either to demand refutation as erroneous, or to be admitted into any well-founded theory as correct.

With a view, in the first place, to remove the ambiguities which have beset this subject from the want of explicit definitions, the author enters into the following discussion and explanation of terms employed to express properties of ice on which our theories of glacial motion must essentially depend.

1. The external forms of all bodies in nature may be changed in a greater or less degree, and without producing discontinuity in their mass or destruction of their internal structure, by the action of any external forces, the original or undisturbed form from which the change of form is to be estimated being that which the body would assume if acted on by no external forces whatever. This change of form necessarily implies a change in the relative positions of the component particles of the mass, or a certain greater or smaller amount of *molecular mobility*, or power in the particles of moving *inter se*. We may speak either of the general change of the form of the whole body, or of that which takes place in each of its small elementary portions; it is, in fact, in this latter sense that we are obliged to regard it in any accurate investigations, because the change of form for different elements will usually be different. Change of form in an element may or may not be accompanied by a change of its volume. In the first case it leads to *cubical* extension or compression; in the latter, merely to extension or compression of the

surface and not the volume of the element. It may be called *superficial* extension or compression. These changes of volume and form in any element must be produced by the forces acting on it. Thus we may conceive linear extension alone produced at any interior point of the mass by two equal and opposite tensions acting on two elementary component particles there in the direction of the line joining their centres of gravity, while compression alone would result if those tensions were changed into pressures. In such cases extension or compression would be the result of forces which may be called *direct* or *normal* forces. In the case above mentioned, in which the volume and density of every element of the mass remain unaltered, there can be no such direct normal action as that just mentioned. It must be perpendicular to the normal action, and therefore a *transversal* or *tangential* action. There would be no tendency to make the contiguous particles approach to or recede from each other, but to cause the one to *slide* tangentially past the other.

If the body have a structure like that of any hard, vitreous or crystalline mass, pressure at any point will tend to break or crush the body, and thus to destroy the continuity of its structure. This tendency will be opposed by the *resisting power* of the substance. The tendency of the direct or normal tension is to separate the contiguous particles, and thus produce a finite fissure, or a discontinuity in the mass. It is resisted by the *normal cohesive power*; and in like manner the transverse or tangential action is resisted by the *tangential cohesion*, or that which prevents the component particles from sliding past each other. Again, when the component particles at any point of a body are relatively displaced, they have always a certain tendency to regain their originally undisturbed position, and the force thus excited, considered with reference to the force of displacement at that point, affords a measure of what is called the *elasticity* of the body. Since the force of restitution may vary from zero to the corresponding force of displacement, the elasticity, when measured by their ratio, may vary from zero to unity.

2. We may now define such terms as *solid*, *plastic*, *viscous*, and the like, with all the accuracy which their definitions admit of. We may call a body emphatically a *solid* body when it possesses the following properties:—(1) small extensibility and compressibility, (2) great power of resistance and great cohesive power, both normal and tangential, and (3) great elasticity. It will thus require a comparatively great force to produce any sensible relative displacement among the constituent molecules of the body; if we conceive the force required to become infinitely great, we arrive at absolute *rigidity* as the limit of solidity. Again, we shall best, perhaps, define *plasticity* or *viscosity*, if we suppose the forces of displacement to be such as to produce only a small transverse or tangential displacement of the constituent particles, *i. e.* a superficial, not a cubical, extension or compression. Then if the force of restitution bear only an inappreciable ratio to the corresponding force of displacement, *i. e.* if the *tangential elasticity* be not of sensible magnitude, the mass may be emphatically said to be *plastic*. This is the essential condition of

what may with strict propriety be termed *plasticity*; it might also be added that, as bodies are constituted in nature, the force required to produce the original displacement in plastic bodies will be small as compared with that required in solid bodies. Viscosity and semi-fluidity are terms which only express similar properties of bodies, but usually indicating that still smaller forces only are required to produce a given displacement in such bodies than in plastic ones. The limiting case is that of perfect fluidity, in which both the forces of original displacement and those of restitution are indefinitely small. In these latter cases the tangential cohesion is necessarily small, and such also (as bodies are usually constituted) will be the normal cohesion. At the same time the power of resisting compression of volume may be very great, as in fact it is in nearly all masses not technically designated as *elastic masses*. In other words, the *normal* elasticity, with reference to pressure, may be of any magnitude, while the *tangential* elasticity equals zero.

It will be observed that a body is here spoken of as held in a state of constraint by internal forces, but without any kind of dislocation which should destroy its continuity or injure its structure. If, however, the external forces should be sufficiently increased, the structure of a vitreous or crystalline mass, or that of any mass possessing hardness and brittleness, will be destroyed by a pressure greater than its power of resistance can withstand; or the continuity of its mass will be destroyed by any normal tension greater than the normal cohesion; or, again, by any tangential tension greater than the tangential cohesion. The normal tension would thus produce an open fissure; and the tangential tension would cause one particle of the mass to slide past another, but without producing any open discontinuity. On the contrary, in a properly plastic or viscous mass there is no definite structure for excessive pressure to destroy; there is no question as to the formation of open fissures; and the characteristic absence of *tangential* elasticity allows of any amount of change in the relative positions of the constituent particles of the mass without breach of its continuity.

It would of course be impossible to draw an exact and determinate line of demarcation between solidity and plasticity, but it is not therefore the less certain that there are bodies which do unequivocally possess the property of solidity, and others which do as unequivocally possess the property of plasticity, according to the definitions here given of these terms. Solidity and plasticity with respect to numerous cases in nature thus become determinate properties of those aggregates of material particles which we call bodies. Ice, a vitreous or crystalline and brittle mass, which will neither bear any but the smallest extension without breaking, nor more than the smallest compression without being crushed, must be solid, and cannot be plastic, if we are to use those terms as significant of determinate properties of bodies.

3. The advocates of the viscous theory would not probably admit the necessity of the above rigorous definition of the term viscous in its application to glacier ice. But the defect of that theory has

always been in the entire want of any accurate definition of that term. When such definition was demanded, it was said that glacier ice must be viscous, because a glacier adapted itself to the inequalities of its valley as a viscous mass would do. This was equivalent to saying that the mass was viscous because it moved in a particular manner, instead of asserting that the mass moved in that particular manner because it was viscous. Now this kind of inversion of the direct enunciation of the proposition is only admissible when there is no other physical cause than the one assigned, to which it is conceivable that the observed phenomena should be ascribed. Thus we may assert with perfect conviction, that gravity exists as a property of matter and acts according to a certain law, because the bodies of the solar system move as if such were the case; but the conclusiveness of this inductive proof of the proposition—that “gravity is a property of matter”—rests entirely on our conviction that matter has no other property by which we could equally account for the phenomena of the celestial motions. And so with regard to glaciers. If viscosity were the only conceivable property of ice by which we could possibly account for the observed motion of glaciers, then would the observed phenomena of that motion perfectly convince us of the existence of the property in question. But here the two cases entirely differ, inasmuch as there was no general conviction, nor even a decided probability at the time I allude to, that no physical property of ice could exist besides viscosity which might account for the observed phenomena of a glacier’s motion; and at the present time it is proved that there *is* another property of ice by which those phenomena are perfectly accounted for, and the inductive proof becomes altogether valueless. Moreover, in the case of universal gravitation, the inductive proof is the only possible one, whereas in glacier motion we are concerned with a property which, in whatever sense the definition of it may be regarded, must be as capable of being rendered patent by experiment in ice, if it exist, as in any other substance.

The answer, then, that was given to the question—what is viscosity?—comprised no definition at all of that term. The viscous theory ignored the possibility of the molecular mobility of a glacial mass united with the preservation of its continuity, being attributable to any other property than that which was designated as *viscosity*, but without giving any exact definition of the term. If it was meant to define by it the property which is here defined by the same terms, the theory had a legitimate claim to be considered a *physical* theory, because it assigned a determinate physical property as the cause of certain observed phenomena. In this sense, however, the author conceives that it would now be admitted to be entirely disproved by Professor Tyndall’s experiments, in which the ice exhibits so clearly the property of solidity, and the absence of all indication of plasticity. It may be presumed that the hypothesis of viscosity could only have been adopted in the first instance from the apparent absence of any other property of ice which might account equally well for the molecular mobility of the glacial mass.

4. But if the determinate property of viscosity, as here defined, be

not recognized in ice, what, it will be asked, is really the idea which has been attached to the term plastic or viscous? The question, as already observed, is difficult to answer. Perhaps the best way of doing so is to refer to the *Prefatory Note* to Principal Forbes's 'Occasional Papers' (p. xvi). He there intimates that the expressions "bruising and re-attachment," and "incipient fissures re-united by time and cohesion," used by him in 1846, are to be regarded as having the same meaning as the expression "fracture and regelation," first introduced into the subject in 1857. Now there is no ambiguity whatever in this latter expression. "Fracture" means the breaking and splitting of the ice regarded as a brittle and crystalline *solid*, and could never be intended to have the slightest reference to viscosity. In fact the expression is altogether inapplicable to any body which can be called viscous without a violation of scientific language. Still this, it may be said, may be only a want of strict accuracy of expression, rather than of accuracy of conception. But if a notion of cracking and breaking, so foreign to any idea of plasticity, should be admitted, it could not be said that a glacier moved as it is observed to move, because it was plastic, but merely that it moved *as if* it were plastic. The true inference from the motion would have been that glacier ice possessed not necessarily real plasticity, a definite property of bodies, but a *quasi-plasticity*, which expresses no determinate property at all, but may consist with many different properties. It merely expresses, in fact, the power of the component elements of the mass of changing to a certain extent their relative positions. But this is not the peculiar property of ice; it is common, indeed, to all bodies exposed to disruptive forces which, as in the case of ice, the cohesive power is unable to withstand. The mass of any other substance, as well as that of a glacier, will then be broken into fragments sufficiently small to allow it to follow the impulses of the external forces acting on it. To say, therefore, that a glacier moves *as if* it were plastic is not to assign to ice any property peculiar to itself, and therefore does not properly constitute a *physical* theory of glacier motion at all.

5. But if we pass over the difference between true plasticity and that which, as we have pointed out, is merely apparent, there still remained the great difficulty, which was only removed by the experiments of Mr. Faraday and Dr. Tyndall. Every one who believed ice to be a solid body, believed as a matter of necessity that a glacier must, on account of the external conditions to which it is subjected, be excessively broken and dislocated in the course of its motion. The author was himself one of those who fell into the error of attributing too much influence to the larger and more visible disruptions of the mass; but the great difficulty was in the perfect subsequent reunion of portions which had thus been separated, whether by larger or smaller dislocations. And here it will necessarily be asked whether, in the expressions above quoted, "*re-attachment*" and the "*reunion by time and cohesion*" of separated portions when again brought into contact, really mean the same thing as *regelation*? This question the author thinks can be answered only by saying that, whatever might be the intended meaning of those expressions, they failed to

convey to the minds of others the most remote idea of regelation, as a property of ice at a particular temperature. No better proof can be given of this than the general conviction which appeared to flash across the mind of every glacialist when he first heard of Professor Tyndall's experiment, that the recognition of the property of instantaneous regelation was a well-marked and important discovery, which had at once completely removed a great stumbling-block in glacial theory. In fact, the viscous theory assigns no physical cause for the reunion in question. All we could do, before the publication of those experiments, was to infer from the observed facts that ice did possess some property which facilitated the reunion of separate pieces in contact; but this was like the attempt to define viscosity by an appeal to the phenomena which that property was intended to explain. Regelation has, in fact, no connexion with viscosity, but stands in direct antagonism to it.

An *imperfect plasticity* in ice has sometimes been spoken of. The fact is, all solid bodies may be said to have an imperfect plasticity, if we chose to admit this vagueness in scientific language, since all are capable of greater or less extension or compression. As to the apparent plasticity inferred from the motion of glacial masses, and arising from the crevicing of the ice as already explained, it has no relation whatever to real plasticity. Such crevices are the necessary consequences of the external forces acting on the glacier, and are as essential to the theory of regelation as they are unconnected with any property of plasticity.

The author then briefly describes the experiment, by which it is shown that ice will slide down an inclined plane at an inclination to the horizon less than that of any known glacier, provided its lower surface be in that state of disintegration in which it will necessarily be when its temperature = zero (C.). The motion is then slow and uniform. That glaciers do slide over their beds, has been established as clearly as it can be by the comparatively few observations which have been made on the subject; and every existing glacial valley, and every valley which is believed to have been such at former geological periods, testify to the truth of that conclusion. The author also explains that both theory and observation agree in the result that the temperature of the lower surface of a glacier of any considerable depth in the latitude of the Alps must necessarily be = zero (C.). He regards this sliding motion as far too important a part of the whole motion of a glacier to be neglected in any complete theory of that motion.

The author then proceeds to investigate certain properties of the internal tensions and pressures at any point (P) in the interior of a mass held in a state of constraint by external forces. He shows that at every point (P) there are three determinate directions, at right angles to each other, in which the direct tension is such that in one of them it is a maximum, in another a minimum, and in the third neither a complete maximum nor a complete minimum; it is convenient to call this the *mean axis*. The tensions or pressures in these directions are called *principal tensions* or *pressures*; there are

also two other directions through P characterized by a peculiar property. If we take two adjoining particles, P and P', in the line of maximum tension, that tension will exert a greater effort than there will be in any other direction to separate those particles; or if the internal force be the maximum pressure, those points will be more compressed together than in any other direction. In the two directions (now to be defined) the forces on P and P', acting perpendicularly to the line joining those particles, will exert a greater tendency than is exerted in any other direction, to separate them by making one *slide tangentially* past the other, and then to twist and contort any internal elementary portion of the mass. These two directions are perpendicular to each other, and bisect the angles between the directions of maximum tension and maximum pressure. This problem is treated entirely mathematically; it is the *typical* problem of this part of the subject. The results are applied to a real glacier by the analogy which it bears to the typical one.

For the application of these analytical results, the author then considers the nature of the forces called into action by the two primary characteristics of the motion of a glacier—that its central move faster than its marginal portions, and the portions near the upper faster than those near the lower surface of the mass. He also takes account of the modifications to which these forces may be subjected by changes of form and inclination in the containing valley. He likewise explains the different modes in which the mass may be fractured when the forces become such as to overpower its powers of cohesion or resistance. If the cohesion give way to the maximum tension, an open fissure must be formed in a direction perpendicular to that tension. If the *resisting power* of the ice give way to the maximum pressure at any point, the mass will be *crushed* at that point, but its continuity will be immediately restored by regelation, the internal constraint will be momentarily removed, and the mass will move on. By a repetition of this process the glacier is enabled to move forward, preserving at once the continuity of its motion, of its mass, and of its structure.

The *veined structure* of glacial ice is then examined, and it is shown that, so far as Professor Tyndall's *pressure theory* of that structure involves the condition of the structural surfaces being perpendicular at each point to the maximum pressure there, it is perfectly accordant with the theoretical results of this paper. Whether the structure be marginal, longitudinal and central, or transversal, this is equally true, assuming always that the structure in each locality is the direct and immediate consequence of the forces acting there and tending to produce it. Probably, however, the veined structure in one locality may have originated in another from which it has been *transmitted* by the motion of the glacier. Supposing this to be so entirely, the author examines how this motion of transmission would modify the forms of the transmitted structure. Practically, and within the limits to which observation has yet extended, these modifications would produce forms sensibly coincident with those which would result, as in the previous case considered, from the immediate action

of the forces, independently of transmission. The respective effectiveness of these two causes, therefore, in producing the veined structure in any particular locality is not at present determined. Its determination would require more accurate and detailed observations than have yet been made on this subject.

The *differential theory* of the veined structure is then considered; but here the author dissents entirely from all Professor Forbes's mechanical reasoning, by which he professes to determine the positions of the surfaces of maximum differential motion, which, according to this theory, are coincident with the structural veins. Mr. Hopkins contends that the actual differential motion of two contiguous particles must necessarily take place in the common direction of their motions. He cannot understand the effectiveness of such motion in any other sense, in producing the phenomena in question. He has investigated for this case the forms of the veined surfaces, but finds them altogether different from the observed forms; and with respect to Prof. Forbes's investigation he cannot possibly admit it, as he at present understands it.

The author then examines the *intensity* of the *dislocating forces* acting on the glacier. He demonstrates Prof. Forbes's error in supposing that it is much augmented by an enormous hydrostatic pressure within the mass, tending to push it onward in the direction in which it may be most free to move. It is proved that, under the existing conditions of a glacier, the hydrostatic pressure from the water contained in the pores of the mass can but little exceed the atmospheric pressure on its surface. But Mr. Hopkins shows that there must in many localities be a very large increase in the intensity of the internal tensions and pressures arising from the free sliding motion of the whole glacier. Where the motion of a particular part of the mass is retarded by local circumstances, there will probably be an enormous pressure upon it *à tergo*, from the mass behind; or there may, in other cases, be a great additional tension, arising from the freer motion of the mass in front. Hence the dislocating forces must often be greatly increased, the dislocation is ensured, and the operation of regelation brought into action; and the continued motion of the glaciers is preserved when it might otherwise be arrested.

“Experiments on Food; its Destination and Uses.” By William S. Savory, Esq., F.R.S.

“On a New Series of Compounds containing Boron.” By Dr. Edward Frankland, F.R.S.

June 19.—Major-General Sabine, President, in the Chair.

The following communications were read:—

“Dissections of the Ganglia and Nerves of the Oesophagus, Stomach, and Lungs.” By Robert Lee, M.D., F.R.S.

“Further Observations on the Distribution of Nerves to the Elementary Fibres of Striped Muscle.” By Lionel S. Beale, M.B., F.R.S.

“Researches on the Development of the Spinal Cord in Man, Mammalia, and Birds.” By Jacob Lockhart Clarke, Esq., F.R.S.

“Observations made on the Movements of the Larynx when viewed by means of the Laryngoscope.” By John Bishop, Esq., F.R.S.

“Anatomy and Physiology of the Spongiadæ.” By J. Scott Bowerbank, LL.D., F.R.S.

“On the Spectrum of Carbon,” By John Attfield, Esq., F.C.S., Demonstrator of Chemistry at St. Bartholomew’s Hospital.

The author has prismatically examined various flames containing carbon. He finds that certain rays of light are common to ignited oxycarbons, hydrocarbons, nitrocarbons, and sulphocarbons, and concludes that these common rays are those emanating from ignited carbon vapour. By special manipulation he obtains the carbon spectrum with olefiant gas, cyanogen, carbonic oxide, and bisulphide of carbon. Observed by the naked eye, the prevailing colour of ignited carbon is light blue.

GEOLOGICAL SOCIETY.

[Continued from vol. xxiv. p. 492.]

November 19, 1862.—Prof. A. C. Ramsay, President, in the Chair.

The following communication was read:—

“On the Cambrian and Huronian Formations, with remarks on the Laurentian.” By J. J. Bigsby, M.D., F.G.S.

This paper is divided into two parts, the first treating of the Cambrian Formation, and the second of the Huronian. The author observed that the Cambrian is very local in its distribution, the Silurian in many cases lying directly upon Metamorphic Rocks; he made some remarks upon the mineralogical and stratigraphical characters of the first-named formation, the scarcity of its fossils, its conformable upward passage into the Silurian, and its absence in America and Northern Europe.

In the second part were described the Huronian of Canada, the Azoic Rocks of the southern shores of Lake Huron and Lake Superior, and the Second Azoic Group of Norway, all of which are considered by the author to belong to the same period. It was then stated that the Huronian Formation and its equivalents agree in being unconformable to the Silurian and conformable to the Laurentian, in containing many beds of Limestone and a large quantity of copper-ores, and in the total absence of fossils; in all of which respects they differ from the Cambrian. The author, therefore, came to the conclusion that the Cambrian and the Huronian are distinct formations, and that the latter is very much the older.

December 3, 1862.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. “Description of the Remains of a new Enaliosaurian (*Eosaurus Acadianus*), from the Coal-formation of Nova Scotia.” By O. C. Marsh, Esq., M.A. Communicated by Sir C. Lyell, V.P.G.S.

The specimen which formed the subject of this communication

consists of two biconcave vertebral centra. The strata wherein they were discovered were described, and the reptilian remains hitherto found in them briefly referred to. The form and structure of these vertebræ were then described in detail, and contrasted with those of the vertebræ of other Enaliosaurians and of Plagiostomous fishes; and it was stated that, with the exception of these, no Enaliosaurian remains have as yet been found in strata older than the Trias.

2. "Description of *Anthracosaurus*, a new genus of Carboniferous Labyrinthodonts." By Professor T. H. Huxley, F.R.S., F.G.S.

Anthracosaurus is distinguished from all other known Labyrinthodonts by the quadrate form and oblique position of the orbits, by the existence of elongated supratemporal foramina, and by the comparatively small number and large size of the teeth. The skull exhibited had an extreme length of 15 inches, and an extreme width of 12 inches. There are about thirty maxillary, two vomerine, and ten palatine teeth, which are ridged and become flattened and two-edged towards their apices. The vomerine, palatine, and some of the anterior maxillary teeth are between 2 and 3 inches long, and from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch in diameter at the base. The species exhibited was named *A. Russellii*, after its discoverer. Probably its entire skeleton had a length of not less than 6 feet.

3. "On the Thickness of the Pampean Formation near Buenos Ayres." By Charles Darwin, Esq., M.A., F.R.S., F.G.S.

Some sections of Artesian wells sunk in this formation showed its entire thickness near Buenos Ayres to be about 210 feet. It was stated to rest upon various marine beds upwards of 100 feet thick, containing *Ostrea Patagonica*, *Ostrea Alvarezii*, *Pecten Paranensis*, &c. These reposed upon red calcareous clay, which was bored through to a depth of 213 feet more, contained no fossils, and is of unknown age.

4. "Geological Notes on the locality in Siberia where Fossil Fishes and *Estheria* were found by Dr. Middendorf." By C. E. Austin, Esq., C.E., F.G.S.

These fossils were obtained from a bed of shale in a low cliff about 200 versts south of Nertschinsk, forming the west bank of the Tourga, a small tributary of the River Onon. The author gave a detailed section of the cliff, and noticed the volcanic and sedimentary rocks of the district, and more especially referred to the far-famed, gem-containing granite of Odon-Tchalon.

5. "Note on *Estheria Middendorfi*." By Professor T. Rupert Jones, F.G.S.

The locality whence this *Estheria* was obtained was described in the former paper. The carapace was stated to approach in character to that of *E. Dahalacensis*, which lives in the freshwater marshes of Dahalac and in the Tigris. It was concluded that the deposit in which this fossil occurs is of freshwater formation, and probably of Tertiary date.

December 17, 1862.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "On the Skiddaw Slate Series." By Professor R. Harkness, F.R.S., F.G.S.; with a Note on the Graptolites, by J. W. Salter, Esq., F.G.S.

In this paper some general sections through the Skiddaw Slates were described in detail, and the localities in which fossils had been previously found by Professor Sedgwick were especially noticed. The author then stated that he had discovered several species of Graptolites new to the Skiddaw Slates in certain flaggy beds almost devoid of cleavage, which occur at intervals in the lower portion of the series, in several localities. Professor Harkness showed that these rocks were much more fossiliferous than had hitherto been supposed, and that the evidence of the fossils, as interpreted by Mr. Salter, clearly proved them to be of the same age as the Lower Llandeilo rocks of Wales and the Quebec Group of Canada. The thickness of the Skiddaw Slates was estimated at 7000 feet, and the total thickness from the base of the Skiddaw Slates to the Coniston limestone at 14,000 feet.

Besides several species of well-known Graptolites that are also found in the Lower Llandeilo rocks and in the Quebec Group (Faonic System), Mr. Salter has been enabled to identify *Phyllograpsus angustifolium*, Hall, *Tetragrapsus bryonoides*, Hall, and another species of that genus, *Dichograpsus Sedgwicki*, n. sp., *Didymograpsus caduceus*, and some others. He has given the name of *Caryocaris Wrightii* to a Crustacean discovered in these rocks by Mr. Wright. Mr. Salter considers the Skiddaw Slates to be of the same age as the Quebec Group, the graptoliferous rocks of Melbourne, and the Tremadoc Slates of Wales.

2. "On Fossil *Estheria*, and their Distribution." By Professor T. Rupert Jones, F.G.S.

Referring to the Monograph of Fossil *Estheria*, now in course of publication by the Palæontographical Society, for descriptions of the species and for general remarks on the genus, the author in this paper pointed out the chief characters of the fourteen species of *Estheria* that he had obtained, by the help of friends at home and abroad, from several of the geological formations; and pointed out that they belong mainly to the passage-groups, and, he believed, chiefly to fresh and brackish waters. He also compared the distribution of the twenty-two recent species with that of the fossil *Estheria*.

3. "On the Flora of the Devonian Period in North-Eastern America. Appendix." By Dr. J. W. Dawson, LL.D., F.R.S., F.G.S.

Dr. Dawson enumerated in this Appendix some additional species of plants lately obtained from Perry, by Mr. Brown of that place. He also stated that recent observations have shown that the beds spoken of in his paper as belonging to the Catskill Group of New York, really represent the Chemung Group of that State, according to Professor J. Hall.

January 7, 1863.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. “On the Lower Carboniferous *Brachiopoda* of Nova Scotia.”
By T. Davidson, Esq., F.R.S., F.G.S.

The age of these beds was stated to have been first clearly determined by Sir C. Lyell; and the author, having mentioned his concurrence in the views of that geologist on this question, proceeded to point out the affinities of the entire Carboniferous formation to the Permian, and observed that many species, especially of *Brachiopoda*, are common to both formations. He combated the idea of a universal extinction of species at the close of the Palæozoic epoch, on the ground that some Palæozoic species pass upwards into Mesozoic strata; and then, after remarking upon the vagueness of the term ‘species,’ proceeded to show that science was not yet in a condition to enable us to test satisfactorily, by observation, the value of Mr. Darwin’s theory of descent with modification. Mr. Davidson then remarked that the Lower Carboniferous *Brachiopoda* of Nova Scotia were smaller than the same or representative species occurring in contemporaneous strata in other parts of the world; and he concluded by giving diagnoses of the species determined by him, and comparing his list of species with that given by Sir C. Lyell in his ‘Travels in North America.’

2. “On the Gravels and other superficial Deposits of Ludlow, Hereford, and Skipton.” By T. Curley, Esq., C.E., F.G.S.

In describing some plans and sections taken during the progress of drainage-works in Ludlow, Hereford, and Skipton, the author pointed out the relations of the older rocks to the contiguous gravel-deposits; he mentioned the existence, near Ludlow, of two kinds of gravel, having a difference of level of about 100 feet, described three terraces of a like nature, about 30 or 40 feet apart vertically, in the vicinity of Hereford, and then noticed the similar deposits near Skipton. Mr. Curley considers the majority of these gravel-beds to be of lacustrine origin.

January 21, 1863.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. “On a Northerly Extension of the Upper Silurian ‘Passage-beds’ to Linley, Salop.” By George E. Roberts, Esq., and John Randall, Esq. Communicated by the President.

Sections obtained by the authors along the course of Linley Brook, near Bridgenorth, Salop, were shown to exhibit an ascending series of deposits from Aymestry shales, through Upper Ludlow rock, Downton sandstones (with bone-bed), grey shales and grits (with bone-bed), and plant-bearing shales, to Old Red clays. The lower bone-bed was stated to be chiefly composed of scales of *Thelodus* and broken *Lingula*, and the higher one to contain a more than usual abundance of fish-spines; and it was remarked that crustacean remains were altogether absent, but *Lingula cornea* had a range upwards to the Old Red clay. The authors considered the physical conditions of the period to be those indicated by the

remarks of Sir Roderick Murchison upon the change in the character of the sediments which closed the Silurian epoch. The occurrence at Trimpley, and elsewhere, of a cornstone-band in the plant-bearing shales was noted as giving a more defined basis for the Old Red Sandstone.

2. "On some Crustacean-tracks from the Old Red Sandstone near Ludlow." By George E. Roberts, Esq.

Tracks of a crustacean found by Mr. Alfred Marston on a thin sandstone layer, lying between two bands of cornstone at Bouldon, 7 miles N. of Ludlow, were exhibited by the author, and doubtfully referred to *Hymenocaris*. The sandstone in question was stated to be rich in crustacean and annelidan tracks and trails. The lower cornstone in the section exhibited at Bouldon was referred to the horizon of the plant-bearing shales of Linley.

3. "On the Parallel Roads of Glen Roy, and their place in the History of the Glacial Period." By T. F. Jamieson, Esq., F.G.S., Professor of Agriculture in the University of Aberdeen.

After describing the general appearance of the Roads, the author referred to the different theories that have been framed to account for them, giving his reasons for considering both the marine hypotheses untenable, and pointing out the evidences in favour of Agassiz's theory of a dam of glacier-ice having supported a fresh-water lake. He especially dwelt upon the coincidence between the height of each of the Parallel Roads and that of a neighbouring watershed, but also remarked upon the objections to a glacial barrier, explaining how it might have shrunk at three successive periods, so as to allow of the formation of the three Roads. He then showed that the period of the formation of these roads must either have been posterior to that of the chief submergence of the Drift-period, or that the sea did not reach them during the submergence; also, that it was prior to the formation of the 40 feet raised beach of Argyleshire.

Professor Jamieson concluded by stating that his examination of Lochaber had led him to infer that the Parallel Roads are the beaches of ancient freshwater lakes, which arose from glaciers damming the mouths of the valleys and reversing their drainage, at a date subsequent to that of the great land-glaciation of Scotland, owing to a re-extension of the glaciers after the chief submergence of the Drift-period.

February 4, 1863.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "On a Hyæna-den at Wookey Hole, near Wells."—No. II. By W. Boyd Dawkins, Esq., B.A., F.G.S., of the Geological Survey of Great Britain.

The former but partial exploration of this cave by the author convinced him of the desirability of a more rigorous examination, the details of which were given in this paper, with a Table of the species of Mammalia whose remains were met with (showing the distribution of the teeth and bones in the several parts of the cave),

and also a statement of the general results arrived at. A consideration of the distribution of the remains in the cavern and their close juxtaposition to the roof, coupled with the fact that the flint and chert implements discovered were found in much lower positions, led Mr. Dawkins to infer that the bones had been dragged in by hyænas, and that the cave had been subjected to periodical inundations of waters laden with red mud, whereby the bones had been elevated by degrees until they occupied their present position. After a detailed description of the bones, the author concluded by some general remarks upon the bearing of this cave-fauna upon the ancient physical geography of the district, and the antiquity of the associated implements of human manufacture.

2. "On the discovery of *Paradoxides* in Britain." By J. W. Salter, Esq., F.G.S., of the Geological Survey of Great Britain.

A short sojourn in the neighbourhood of St. David's enabled Mr. Salter to discover, at Porth-rhaw, near Whitchurch, on the St. David's road, a gigantic Trilobite belonging to a genus which has been long sought for in the British Isles. The author gave a short description of the geological features of the locality, and a section showing the succession of beds belonging to the Primordial Zone in Wales, as well as a diagnosis of the newly discovered Trilobite, which he named *Paradoxides Davidi*.

3. "On the fossil *Echinidæ* of Malta." By Thomas Wright, M.D., F.G.S. With Notes on the Miocene Beds of the Island, by A. Leith Adams, A.M., M.B. (22nd Regiment).

The Echinoderms described in this paper by Dr. Wright were discovered by Dr. Leith Adams during a careful examination of the strata and geological features of Malta. A description of the Miocene beds was given by the latter gentleman, in which he stated his reasons for not accepting entirely the classification of them proposed by Captain Spratt, and followed by Earl Ducie in his Geological Map of the Maltese Islands. He divided the Miocene strata into the following subdivisions:—1. The Upper Limestone; 2. The Sand Bed; 3. The Marl; 4. The Calcareous Sandstone; 5. The Lower Limestone; and again subdivided the Upper Limestone into three parts. Dr. Wright gave a diagnosis and detailed description of forty species of *Echinidæ*, eighteen of which are new; and Dr. Adams added a Table showing their stratigraphical distribution.

XXXIII. Intelligence and Miscellaneous Articles.

DETERMINATION OF THE WAVE-LENGTH OF THE RAY A.

BY M. MASCART.

I HAVE the honour to lay before the Academy the results of some experiments which I have made on the application of coloured flames to the investigation of the lengths of undulations. By comparing the wave-lengths of the rays corresponding to the different lines of the solar spectrum with the deviations which the same rays undergo in a refracting prism, it is seen that these two quantities vary in opposite directions, and that the ratio of the increase of the wave-length

to the corresponding diminution of deviation increases rapidly in the less refrangible part of the spectrum. But the ray A of the extreme red is difficult to work with, on account of the feeble lustre of the solar light in this part of the spectrum; its wave-length is not known, and it is not mentioned in most of the tables of refraction hitherto published.

It appeared to me interesting to determine this wave-length by the aid of a grating; and I used for this purpose, not the solar light, but the least deflected bright ray of potash-salts, which, as the recent experiments of M. Kirchhoff have shown, coincides exactly with the ray A.

I used a Babinet's goniometer showing 10 seconds, and a grating of about 4 square centimetres superficies divided into fortieths of a millimetre; I compared the deviation of the line A with that of the bright soda line. To obtain the greatest intensity possible in the luminous source, I used several methods, especially the combustion of hydrogen charged with the vapours of potassium (as MM. Wolf and Diacon have done on the suggestion of M. Foucault), and the volatilization of chloride of potassium in an oxyhydrogen blowpipe. The latter method, devised by M. Debray, always succeeded best. Spite of these precautions I could only observe the first spectrum, which gave little value to isolated observations; but the mean of a great number of concordant measures was about 768 millionths of a millimetre.

For the wave-length of the line A the number 750 millionths of a millimetre is generally received—a number deduced from the theoretical law of dispersion found by M. Cauchy, or from an interpolation formula. When the season is more favourable to this kind of experiments, I propose to determine the refractive indices of the ray A in different substances, and to see if Cauchy's law agrees sufficiently with experiment.

This research has led me to another observation. As I needed a very intense luminous source, I thought of the volatilization of potassium between the two poles of a powerful voltaic battery; but the result did not correspond to my anticipation. I obtained a magnificent spectrum, more complicated than those which have been hitherto indicated for potassium; the red line corresponding to the solar line B was very intense and quite double: but I sought in vain for the line A; and examining with care, I saw a feeble red illumination on both sides of an obscure space in the region of the luminous line; I could even distinguish a brilliant line between two black lines; that is to say, the extreme double line of potassium was inverted. This partial reversal of the potash lines does not seem to me to be in disaccord with M. Kirchhoff's theory; for it is to be noted that the line which is reversed is that which is produced at a lower temperature. The same phenomenon takes place with sodium as in M. Fizeau's experiment; the double line D is the only one which is reversed; but this reversal has a peculiarity which every one has been able to observe: it is that the reversed black line can in certain circumstances increase to a considerable extent, retaining always well-defined limits. The preceding method is advantageous for deter-

mining the wave-length of certain very brilliant lines like those of lime, strontia, and thallium. It has given me the opportunity of observing that at a high temperature thallium is not monochromatic as had been thought. This fact is not surprising when we consider the great number of lines which soda gives at a high temperature.

These results are part of a research which I have been pursuing for more than a year in the laboratories of the Ecole Normale. I have already published, in the *Revue des Sociétés Savants*, a Note on the chemical spectra of the alkaline metals, in order to reserve to myself the facility of continuing these researches at pleasure.—*Comptes Rendus*, January 19, 1863.

ON A NEW FORM OF SPECTROSCOPE. BY DR. WOLCOTT GIBBS.

Messrs. J. and W. Grunow, the well-known opticians of New York, have just completed, at my suggestion, a spectroscope involving a new principle, or rather one for the first time applied to instruments of this kind. In this instrument the prism of flint glass has a refracting angle of only 37° ; the rays which diverge from the slit are rendered parallel in the usual manner, by an achromatic lens having the slit in its principal focus. The bundle of rays then falls upon the first surface of the prism at a perpendicular incidence, and of course makes an angle of 37° with the second surface. Under these circumstances the refraction takes place at an angle so near the limiting angle that the refracted rays emerge nearly parallel to the second surface of the prism. The amount of dispersion produced in this manner is very great, while the loss of light, occasioned by reflexion at the first surface in the prisms of 60° placed in the position of least deviation, is avoided. The spectrum thus produced possesses remarkable intensity, and the dark lines are seen in countless numbers and with great distinctness. The instrument in this form is sufficient for all *chemical* purposes; but it is so constructed as to permit the use of a second prism, by which the length of the spectrum is of course greatly increased. Though the telescopes are only 6 inches in length, with a magnifying power of about 6, the spectrum compares very advantageously with that of a large apparatus with telescopes of 18 inches focal length and $1\frac{1}{2}$ inch aperture, and a prism of 60° . I may mention that the centre of the second surface of the prism lies in the vertical axis of the instrument, and also that in a prism of this kind the refracted rays diverge as if from a single radiant point (which is not the case with prisms of the ordinary construction), the angular dispersion being at the same time much greater. So far as I have been able to find, this form of prism was first employed by Matthiessen. In a lithographed copy of Regnault's 'Lectures on Optics' at the Collège de France in 1848, prisms on this principle, of various forms, are figured and described, together with the spectra produced. These last exhibit an extraordinary extension of the violet end of the spectrum. A Matthiessen prism of flint glass, in which the first surface is concave so as to admit the addition of a double convex lens of crown glass, appears to be preferable for the spectroscope, in consequence of the saving of light.—Silliman's *American Journal*, January 1863.

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[FOURTH SERIES.]

APRIL 1863.

XXXIV. *On Celestial Dynamics.* By DR. J. R. MAYER*.

I. *Introduction.*

EVERY incandescent and luminous body diminishes in temperature and luminosity in the same degree as it radiates light and heat, and at last, provided its loss be not repaired from some other source of these agencies, becomes cold and non-luminous.

For light, like sound, consists of vibrations which are communicated by the luminous or sounding body to a surrounding medium. It is perfectly clear that a body can only excite such vibrations in another substance when its own particles undergo a similar movement; for there is no cause for undulatory motion when a body is in a state of rest, or in a state of equilibrium with the medium by which it is surrounded. If a bell or a string is to be sounded, an external force must be applied; and this is the cause of the sound.

If the vibratory motion of a string could take place without any resistance, it would vibrate for all time; but in this case no sound could be produced, because sound is essentially the propagation of motion; and in the same degree as the string communicates its vibrations to the surrounding and resisting medium its own motion becomes weaker and weaker, until it at last sinks into a state of rest.

The sun has often and appropriately been compared to an incessantly sounding bell. But by what means is the power of

* *Beiträge zur Dynamik des Himmels, in populärer Darstellung*, von Dr. J. R. Mayer, Stadtarzt in Heilbronn. Heilbronn, 1848. Translated by Dr. H. Debus, F.R.S.

this body kept up in undiminished force so as to enable him to send forth his rays into the universe in such a grand and magnificent manner? What are the causes which counteract or prevent his exhaustion, and thus save the planetary system from darkness and deadly cold?

Some endeavoured to approach "the grand secret," as Sir Wm. Herschel calls this question, by the assumption that the rays of the sun, being themselves perfectly cold, merely cause the "substance" of heat, supposed to be contained in bodies, to pass from a state of rest into a state of motion, and that in order to send forth such cold rays the sun need not be a hot body, so that, in spite of the infinite development of light, the cooling of the sun was a matter not to be thought of.

It is plain that nothing is gained by such an explanation; for, not to speak of the hypothetical "substance" of heat, assumed to be at one time at rest and at another time in motion, now cold and then hot, it is a well-founded fact that the sun does not radiate a cold phosphorescent light, but a light capable of warming bodies intensely; and to ascribe such rays to a cold body is at once at variance with reason and experience.

Of course such and similar hypotheses could not satisfy the demands of exact science, and I will therefore try to explain in a more satisfactory manner than has been done up to this time the connexion between the sun's radiation and its effects. In doing so, I have to claim the indulgence of scientific men, who are acquainted with the difficulties of my task.

II. *Sources of Heat.*

Before we turn our attention to the special subject of this paper, it will be necessary to consider the means by which light and heat are produced. Heat may be obtained from very different sources. Combustion, fermentation, putrefaction, slaking of lime, the decomposition of chloride of nitrogen and of gun-cotton, &c. &c., are all of them sources of heat. The electric spark, the voltaic current, friction, percussion, and the vital processes are also accompanied by the evolution of this agent.

A general law of nature, which knows of no exception, is the following:—In order to obtain heat, something must be expended; this something, however different it may be in other respects, can always be referred to one of two categories: either it consists of some material expended in a chemical process, or of some sort of mechanical work.

When substances endowed with considerable chemical affinity for each other combine chemically, much heat is developed during the process. We shall estimate the quantity of heat thus set free by the number of kilogrammes of water which it

would heat 1° C. The quantity of heat necessary to raise one kilogramme of water one degree is called a unit of heat*.

It has been established by numerous experiments that the combustion of one kilogramme of dry charcoal in oxygen, so as to form carbonic acid, yields 7200 units of heat, which fact may be briefly expressed by saying that charcoal furnishes 7200° of heat.

Superior coal yields 6000° , perfectly dry wood from 3300° to 3900° , sulphur 2700° , and hydrogen $34,600^{\circ}$ of heat.

According to experience, the number of units of heat only depends on the quantity of matter which is consumed, and not on the conditions under which the burning takes place. The same amount of heat is given out whether the combustion proceeds slowly or quickly, in atmospheric air or in pure oxygen gas. If in one case a metal be burnt in air and the amount of heat directly measured, and in another instance the same quantity of metal be oxidized in a galvanic battery, the heat being developed in some other place—say, the wire which conducts the current,—in both of these experiments the same quantity of heat will be observed.

The same law also holds good for the production of heat by mechanical means. The amount of heat obtained is only dependent on the quantity of power consumed, and is quite independent of the manner in which this power has been expended. If, therefore, the amount of heat which is produced by certain mechanical work is known, the quantity which will be obtained by any other amount of mechanical work can easily be found by calculation. It is of no consequence whether this work consists in the compression, percussion, or friction of bodies.

The amount of mechanical work done by a force may be expressed by a weight, and the height to which this weight would be raised by the same force. The mathematical expression for "work done," that is to say, a measure for this work, is obtained by multiplying the height expressed in feet or other units by the number of pounds or kilogrammes lifted to this height.

We shall take one kilogramme as the unit of weight, and one metre as the unit of height, and we thus obtain the weight of one kilogramme raised to the height of one metre as a unit measure of mechanical work performed. This measure we shall call a kilogrammetre, and adopt for it the symbol $Km\ddagger$.

Mechanical work may likewise be measured by the velocity

* The heat requisite to raise 1 kilogramme of water 1° C. will heat 1 lb. av. of water 3.9681° F.

[\ddagger If one metre = 3.2808 English feet, and one kilogramme = 2.2045 lbs. av., it follows that one $Km = 7.2325$ foot-pounds.—TR.]

obtained by a given weight in passing from a state of rest into that of motion. The work done is then expressed by the product obtained by the multiplication of the weight by the square of its velocity. The first method, however, because it is the more convenient, is the one usually adopted; and the numbers obtained therefrom may easily be expressed in other units.

The product resulting from the multiplication of the number of units of weight and measures of height, or, as it is called, the product of mass and height, as well as the product of the mass and the square of its velocity, are called "*vis viva* of motion," "mechanical effect," "dynamical effect," "work done," "*quantité de travail*," &c. &c.

The amount of mechanical work necessary for the heating of 1 kilogramme of water 1° C. has been determined by experiment to be = 367 Km; therefore $\text{Km} = 0.00273$ units of heat*.

A mass which has fallen through a height of 367 metres possesses a velocity of 84.8 metres in one second; a mass, therefore, moving with this velocity originates 1° C. of heat when its motion is lost by percussion, friction, &c. If the velocity be two or three times as great, 4° or 9° of heat will be developed. Generally speaking, when the velocity is c metres, the corresponding development of heat will be expressed by the formula

$$0.000139^{\circ} \times c^2.$$

III. *On the Measure of the Sun's Heat.*

The actinometer is an instrument invented by Sir John Herschel for the purpose of measuring the heating effect produced by the sun's rays. It is essentially a thermometer with a large cylindrical bulb filled with a blue liquid, which is acted upon by the sun's rays, and the expansion of which is measured by a graduated scale.

From observations made with this instrument, Sir John Herschel calculates the amount of heat received from the sun to be sufficient to melt annually at the surface of the globe a crust of ice 29.2 metres in thickness.

Pouillet has recently shown by some careful experiments with the lens pyrheliometer, an instrument invented by himself, that every square centimetre of the surface of our globe receives, on an average, in one minute an amount of solar heat which would

* How this important result is obtained has been explained in my paper "Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel."

[This essay was published in 1845. At that time de la Roche and Berard's determination of the specific heat of air was generally accepted. If the physical constants used by Mayer be corrected according to the results of more recent investigation, the mechanical equivalent of heat is found to be 771.4 foot-pounds. Mr. Joule finds it = 772 foot-pounds.—Tr.]

raise the temperature of one gramme of water $0\cdot4408^{\circ}$. Not much more than one-half of this quantity of heat, however, reaches the solid surface of our globe, since a considerable portion of it is absorbed by our atmosphere. The layer of ice which, according to Pouillet, could be melted by the solar heat which yearly reaches our globe would have a thickness of $30\cdot89$ metres.

A square metre of our earth's surface receives, therefore, according to Pouillet's results, which we shall adopt in the following pages, on an average in one minute $4\cdot408$ units of heat. The whole surface of the earth is $=9,260,500$ geographical square miles*; consequently the earth receives in one minute 2247 billions of units of heat from the sun.

In order to obtain smaller numbers, we shall call the quantity of heat necessary to raise a cubic mile of water 1° C. in temperature, a cubic mile of heat. Since one cubic mile of water weighs $408\cdot54$ billions of kilogrammes, a cubic mile of heat contains $408\cdot54$ billions of units of heat. The effect produced by the rays of the sun on the surface of the earth in one minute is therefore $5\cdot5$ cubic miles of heat.

Let us imagine the sun to be surrounded by a hollow sphere whose radius is equal to the mean distance of the earth from the sun, or $20,589,000$ geographical miles; the surface of this sphere would be equal to 5326 billions of square miles. The surface obtained by the intersection of this hollow sphere and our globe, or the base of the cone of solar light which reaches our earth, stands to the whole surface of this hollow sphere as $\frac{9,260,500}{4} : 5326$ billions, or as 1 to 2300 millions. This is the ratio of the heat received by our globe to the whole amount of heat sent forth from the sun, which latter in one minute amounts to $12,650$ millions of cubic miles of heat.

This amazing radiation ought, unless the loss is by some means made good, to cool considerably even a body of the magnitude of the sun.

If we assume the sun to be endowed with the same capacity for heat as a mass of water of the same volume, and its loss of heat by radiation to affect uniformly its whole mass, the temperature of the sun ought to decrease $1\cdot8$ C. yearly, and for the historic time of 5000 years this loss would consequently amount to 9000° C.

A uniform cooling of the whole of the sun's huge mass cannot, however, take place; on the contrary, if the radiation were to occur at the expense of a given store of heat or radiant power, the sun

* The geographical mile $=7420$ metres, and one English mile $=1608$ metres.

would become covered in a short space of time with a cold crust, whereby radiation would be brought to an end. Considering the continued activity of the sun through countless centuries, we may assume with mathematical certainty the existence of some compensating influence to make good its enormous loss.

Is this restoring agency a chemical process?

If such were the case, the most favourable assumption would be to suppose the whole mass of the sun to be one lump of coal, the combustion of every kilogramme of which produces 6000 units of heat. Then the sun would only be able to sustain for forty-six centuries its present expenditure of light and heat, not to mention the oxygen necessary to keep up such an immense combustion, and other unfavourable circumstances.

The revolution of the sun on his axis has been suggested as the cause of his radiating energy. A closer examination proves this hypothesis also to be untenable.

Rapid rotation, without friction or resistance, cannot in itself alone be regarded as a cause of light and heat, especially as the sun is in no way to be distinguished from the other bodies of our system by velocity of axial rotation. The sun turns on his axis in about twenty-five days, and his diameter is nearly 112 times as great as that of the earth, from which it follows that a point on the solar equator travels but a little more than four times as quickly as a point on the earth's equator. The largest planet of the solar system, whose diameter is about $\frac{1}{10}$ th that of the sun, turns on its axis in less than ten hours; a point on its equator revolves about six times quicker than one on the solar equator. The outer ring of Saturn exceeds the sun's equator more than ten times in velocity of rotation. Nevertheless no generation of light or heat is observed on our globe, on Jupiter, or on the ring of Saturn.

It might be thought that friction, though undeveloped in the case of the other celestial bodies, might be engendered by the sun's rotation, and that such friction might generate enormous quantities of heat. But for the production of friction two bodies, at least, are always necessary which are in immediate contact with one another, and which move with different velocities or in different directions. Friction, moreover, has a tendency to produce equal motion of the two rubbing bodies; and when this is attained, the generation of heat ceases. If now the sun be the one moving body, where is the other? and if the second body exist, what power prevents it from assuming the same rotatory motion as the sun?

But could even these difficulties be disregarded, a weightier and more formidable obstacle opposes this hypothesis. The known volume and mass of the sun allow us to calculate the

vis viva which he possesses in consequence of his rotation. Assuming his density to be uniform throughout his mass, and his period of rotation twenty-five days, it is equal to 182,300 quintillions of kilogrammetres (Km). But for one unit of heat generated, 367 Km are consumed; consequently the whole rotation-effect of the sun could only cover the expenditure of heat for the space of 183 years.

IV. *The Organization of the Planetary System contains the Cause of the Sun's Heat.*

The space of our solar system is filled with a great number of ponderable objects, which have a tendency to move towards the centre of gravity of the sun; and in so doing, their rate of motion is more and more accelerated.

A mass, without motion, placed within the sphere of the sun's attraction, will obey this attraction, and, if there be no disturbing influences, will fall in a straight line into the sun. In reality, however, such a rectilinear path can scarcely occur, as may be shown by experiment.

Let a weight be suspended by a string so that it can only touch the floor in one point. Lift the weight up to a certain height, and at the same time stretch the string out to its full length; if the weight be now allowed to fall, it will be observed, almost in every case, not to reach at once the point on the floor towards which it tends to move, but to move round this point for some time in a curved line.

The reason of this phenomenon is that the slightest deviation of the weight from its shortest route towards the point on the floor, caused by some disturbing influence such as the resistance of the air against a not perfectly uniform surface, will maintain itself as long as motion lasts. It is nevertheless possible for the weight to move at once to the point; the probability of its doing so, however, becomes the less as the height from which it is allowed to drop increases or the string, by means of which it is suspended, is lengthened.

Similar laws influence the movements of bodies in the space of the solar system. The height of the fall is here represented by the original distance from the sun at which the body begins to move; the length of the string by the sun's attraction, which increases when the distance decreases; and the small surface of contact on the floor by the area of the section of the sun's sphere. If now a cosmical mass within the physical limits of the sun's sphere of attraction begins its fall towards that heavenly body, it will be disturbed in its long path for many centuries, at first by the nearest fixed stars, and afterwards by the bodies of the solar system. Motion of such a mass in a

straight line, or its perpendicular fall into the sun, would therefore, under such conditions, be impossible. The observed movement of all planetary bodies in closed curves agrees with this.

We shall now return to the example of the weight suspended by a string and oscillating round a point towards which it is attracted. The diameters of the orbits described by this weight are observed to be nearly equal; continued observation, however, shows that these diameters gradually diminish in length, so that the weight will by degrees approach the point in which it can touch the floor. The weight, however, touches the floor not in a mathematical point, but in a small surface; as soon, therefore, as the diameter of the curve in which the weight moves is equal to the diameter of this surface, the weight will touch the floor. This final contact is no accidental or improbable event, but a necessary phenomenon caused by the resistance which the oscillating mass constantly suffers from the air and friction. If all resistance could be annihilated, the motion of the weight would of course continue in equal oscillations.

The same law holds good for celestial bodies.

[To be continued.]

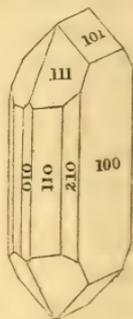
XXXV. *On the Crystalline Form and Optical Properties of Sulphate of Thallium.* By VIKTOR VON LANG*.

THE crystals of sulphate of thallium (Th SO_4), on which I made the following crystallographic and optical observations, were prepared by M. Lamy: their investigation was of special interest to me, as, from measurements of other combinations of thallium recently published by M. de la Provostaye, it seemed to follow that this new element is in its combinations isomorphous with potassium. Now the salts that are isomorphous with sulphate of potassium are remarkable on account of the difference they exhibit in their optical properties, and it was therefore interesting to me to get, if possible, a new salt of this series for investigation. That the crystals of sulphate of thallium are really isomorphous with the corresponding potassium-salt, will be seen by the following crystallographic description of them.

The crystals are combinations of the forms

100, 010, 110, 210, 101, 111;

they are elongated parallel to the axis c , the plane (100) being generally very largely developed, as is represented in the accompanying figure. The para-



* Communicated by the Author.

meters are

$$a : b : c = 1 : 0.7319 : 0.5539 ;$$

and the calculated and observed angles compared with the corresponding values for sulphate of potassium are:—

	Calc.	Obs.	KSO ⁴ .
110.100 =	° — ′	53° 48′	53° 16′
110.010 =	36 12	36 21	36 44
210.100 =	34 20	—	33 49
210.010 =	55 40	—	56 11
110.210 =	19 28	19 30	19 27
101.100 =	—	61 1	60 12
101.1̄01 =	57 58	—	59 36
111.100 =	66 10	66 11	65 34
111.010 =	56 29	56 29	56 20
111.110 =	46 50	46 50	46 15
111.101 =	33 31	33 40	33 40
111.11̄1 =	67 2	67 14	67 19
111.1̄11 =	47 38	47 48	48 52

The faces are very brilliant, and have an adamantine lustre, probably due to the greater predominance (from its high chemical equivalent) of the element thallium in the compound: thallium may thus be supposed to stand in the same relation to potassium that lead does to the isomorphous barium.

With regard to the optical properties of these crystals, I found, on laying one of them on the large plane (100) in the polarizing apparatus, that, from the absence of interference-curves and black hyperbolas, this plane is probably parallel to the plane of the optic axes, and that therefore the axis *a* is parallel to the axis of mean optical elasticity. Trying the compensating action of a section of quartz, interference-curves were produced when the quartz was turned on an axis parallel to the crystallographic axis *c*; the interference-fringes thus produced were parallel with the same direction. We may conclude from this experiment that the smallest axis *c* coincides with the axis of least optical elasticity, and that the mean crystallographic axis is parallel to the greatest axis of elasticity. The optical orientation of these crystals is therefore given by the symbol

bac.

I succeeded afterwards in splitting out of one of the crystals a section across the direction in which the crystals are prolonged. This section showed directly, by the appearance of coloured curves, that the plane of the optic axes is parallel with

the plane (100). It seemed, moreover, that this section is perpendicular to the first mean line, although the points of the optic axes were not in the field of view. In consequence the crystals would be positive. The double refraction is weak.

The following Table contains the optical orientation of the salts isomorphous with the sulphate of potassium which I have hitherto had the opportunity of examining:—

	SO ⁴	SeO ⁴	TeO ⁴	CrO ⁴
Am	bac +	bac -		
K	acb +		acb +	acb -
Th	bac +			

It will be seen from this Table that, in its optical properties, the sulphate of thallium accords with the ammonium, and not with the potassium salt. It would not only be interesting to fill up some of the blanks of the above Table, but also of importance to investigate at least the sulphates of the new elements cæsium and rubidium, which belong to the same isomorphous group. Another desirable object of investigation would be the influence exercised on the optical orientation by the substitution of alcohol radicals in the sulphate of ammonium, provided these new combinations are isomorphous with their original type, which, as far as I am aware, has hardly been proved by measurements even in the case of the simplest of these bodies.

XXXVI. *Contributions towards the History of Spectrum Analysis and of the Analysis of the Solar Atmosphere.* By G. KIRCHHOFF*.

IN my 'Researches on the Solar Spectrum and the Spectra of the Chemical Elements' †, I made a few short historical remarks concerning earlier investigations upon the same subject. In these remarks I have passed over certain publications in silence—in some cases because I was unacquainted with them, in others because they appeared to me to possess no special interest in relation to the history of the discoveries in question. Having become aware of the existence of the *former* class, and seeing that more weight has been considered to attach to the

* Communicated by Professor Roscoe.

† Published by MacMillan and Co., Cambridge and London, 1862.

latter class of publications by others than by myself, I will now endeavour to complete the historical survey.

1. Amongst those who have devoted themselves to the observation of the spectra of coloured flames, I must, in the first place, mention Herschel and Talbot. Their names need special notice, as they pointed out with distinctness the service which this mode of observation is capable of rendering to the chemist. For a knowledge of their researches I am mainly indebted to Prof. W. Allen Miller, who gave an extract from them in a lecture republished in the number of the 'Chemical News' for 19th April, 1862. It is there stated that in the volume of the Transactions of the Royal Society of Edinburgh for 1822, at p. 455, Herschel shortly describes the spectra of chloride of strontium, chloride of potassium, chloride of copper, nitrate of copper, and boracic acid: The same observer says, in his article on Light in the *Encyclopædia Metropolitana*, 1827, p. 438,—“Salts of soda give a copious and purely homogeneous yellow; of potash, a beautiful pale violet.” He then describes the colours given by the salts of lime, strontia, lithia, baryta, copper, and iron, and continues—“Of all salts the muriates succeed best, from their volatility. The same colours are exhibited also when any of the salts in question are put (in powder) into the wick of a spirit-lamp. The colours thus communicated by the different bases to flame afford in many cases a ready and neat way of detecting extremely minute quantities of them. The pure earths, when violently heated, as has recently been practised by Lieut. Drummond, by directing on small spheres of them the flames of several spirit-lamps, urged by oxygen gas, yield from their surfaces lights of extraordinary splendour, which, when examined by prismatic analysis, are found to possess the peculiar definite rays in excess which characterize the tints of flames coloured by them; so that there can be no doubt that these tints arise from the molecules of the colouring matter, reduced to vapour and held in a state of violent ignition.”

Talbot says*, “The flame of sulphur and nitre contains a red ray which appears to me of a remarkable nature. This red ray appears to possess a definite refrangibility, and to be characteristic of the salts of potash, as the yellow ray is of the salts of soda, although, from its feeble illuminating power, it is only to be detected with a prism. If this should be admitted, I would further suggest that whenever the prism shows a *homogeneous* ray of any colour to exist in a flame, this ray indicates the formation or the presence of a *definite chemical compound*.” Somewhat further on, in speaking of the spectrum

* Brewster's Journal of Science, v., 1826. Chemical News, April 27, 1861.

of red-fire and of the frequent occurrence of the yellow line, he says, "The other lines may be attributed to the antimony, strontia, &c. which enter into this composition. For instance, the orange ray may be the effect of the strontia, since Mr. Herschel found in the flame of muriate of strontia a ray of that colour. If this opinion should be correct, and applicable to the other definite rays, a glance at the prismatic spectrum of a flame may show it to contain substances which it would otherwise require a laborious chemical analysis to detect." In a subsequent communication*, the same physicist, after a striking description of the spectra of lithium and strontium, continues—"Hence I hesitate not to say that optical analysis can distinguish the minutest portions of these two substances from each other with as much certainty, if not more, than any other known method."

In these expressions the idea of "chemical analysis by spectrum-observations" is most clearly put forward. Other statements, however, of the same observers, occurring in the same memoirs from which the foregoing quotations are taken (but not mentioned by Prof. Miller in his abstract), flatly contradict the above conclusions, and place the foundations of this mode of analysis on most uncertain ground.

Herschel, in page 438 of his article on Light, almost immediately before the words quoted above, says—"In certain cases when the combustion is violent, as in the case of an oil-lamp urged by a blowpipe (according to Fraunhofer), or in the upper part of the flame of a spirit-lamp, or when sulphur is thrown into a white-hot crucible, a very large quantity of a definite and purely homogeneous yellow light is produced; and in the latter case forms nearly the whole of the light. Dr. Brewster has also found the same yellow light to be produced when spirit of wine, diluted with water and heated, is set on fire."

Talbot states †—"Hence the yellow rays may indicate the presence of soda, but they nevertheless frequently appear where no soda can be supposed to be present." He then mentions that the yellow light of burning sulphur, discovered by Herschel, is identical with the light of the flame of a spirit-lamp with a salted wick, and states that he was inclined to believe that the yellow light which occurred when salt was strewed upon a platinum foil placed in a flame "was owing to the water of crystallization rather than to the soda; but then," he continues, "it is not easy to explain why the salts of potash, &c. should not produce it likewise. Wood, ivory, paper, &c., when placed in the gas-flame, give off, besides their bright flame,

* Phil. Mag. 1834, vol. iv. p. 114. Chemical News, April 27, 1861.

† Brewster's Journal, v., 1826.

more or less of this yellow light, which I have always found the same in its characters. The only principle which these various bodies have in common with the salts of soda is *water*; yet I think that the formation or presence of water cannot be the origin of this yellow light, because ignited sulphur produces the *very same*, a substance with which water is supposed to have no analogy." "It may be worth remark," he adds in a note, "though probably accidental, that the specific gravity of sulphur is 1.99, or almost *exactly twice* that of water." "It is also remarkable," he continues in the text, "that alcohol burnt in an open vessel, or in a lamp with a metallic wick, gives but little of the yellow light; while if the wick be of cotton it gives a considerable quantity, and that for an unlimited time. (I have found other instances of a change of colour in flames, owing to *the mere presence* of the substance, which *suffers no diminution in consequence*. Thus a particle of muriate of lime on the wick of a spirit-lamp will produce a quantity of red and green rays for a whole evening without being itself sensibly diminished"*)

In a later portion of the memoir he attributes the yellow line in one place to the presence of soda-salts, at another to that of sulphur. Thus, in the above-mentioned statement concerning the spectrum of red-fire, he says, "The bright line in the yellow is caused, without doubt, by the combustion of the sulphur" †.

Hence we must admit that the conclusion that the aforesaid yellow line can be taken as a positive proof of the presence of sodium-compounds in the flame can in no way be deduced from Herschel and Talbot's researches. On the contrary, the numerous modes in which the line is produced would rather point to the conclusion that it is dependent upon no chemical constituent of the flame, but arises by a process whose nature is unknown, which may occur, sometimes more easily, sometimes with difficulty, with the most different chemical elements. If we accept such an explanation concerning this yellow line, we must form a similar opinion respecting the other lines seen in the spectrum which were far more imperfectly examined; and in this we should be strengthened by the statement of Talbot, that a piece of chloride of calcium by *its mere presence* in the wick of a flame, and *without suffering any diminution*, causes a red and a green line to appear in the spectrum.

The experiments of Wheatstone ‡, Masson, Ångström, Van

* Brewster's Journal, v., 1826.

[† A short statement of Herschel and Talbot's results, as here quoted, was made by me in a lecture at the Royal Institution on April 5, 1862, and reprinted in the 'Chemical News' for May 10, 1862.—H. E. R.]

‡ Wheatstone not merely experimented with the spark from an electrical machine, but likewise with the voltaic induction-spark. (Report of the

der Willigen, and Plücker upon the spectra of the electric spark or electric light (to which I have already referred in my 'Researches on the Solar Spectrum and Spectra of the Chemical Elements,' MacMillan, London, 1862, p. 8), as well as those of Despretz*, from which this physicist concluded that the positions of the bright lines in the spectrum of the light from a galvanic battery were unaltered by variation of the intensity of the current, might serve to support the view that the bright lines in the spectrum of an incandescent gas are solely dependent upon the several chemical constituents of the gas; but they could not be considered as *proof* of such an opinion, as the conditions under which they were made were, for this purpose, too complicated, and the phenomena occurring in an electric spark too ill understood. The demonstrative power of the above experiments as regards the question at issue is rendered less cogent by the difference visible in the colour of the electric light in different parts of a Geissler's tube; by the circumstance noticed by Van der Willigen, who obtained different spectra by passing an electric spark from the same electrodes through gas of constant chemical composition if the density of the gas was varied within sufficient limits; and lastly by an observation which Ångström cursorily mentions. This physicist says†, "Wheatstone has already noticed that when the poles consist of two different metals the spectrum contains the lines of both metals. Hence it became of interest to see whether a compound of these metals, especially a chemical compound, also gives the lines of both metals, or whether the compound is distinguished by the occurrence of new lines. Experiment shows that the first supposition is correct. The sole difference noticed is, that certain lines were wanting or appeared with less distinctness; but when they were observed they always appeared in the position in which they occurred in the separate metals." In the following sentence, however, he states "That in the case of zinc and tin the lines in the blue were somewhat displaced in the direction of the violet end, but the displacement was very inconsiderable." Had such a displacement, however small, *really* occurred, we must conclude either that the bright lines of the electric spark obey other laws than those of a glowing gas, or that these latter are *not* solely dependent on the separate chemical constituents of the gas.

The question at issue respecting the lines of incandescent gases could only be satisfactorily solved by experiments carried

British Association, 1835; Chemical News, March 23, 1861; Chemical News, March 30, 1861.)

* *Comptes Rendus*, vol. xxxi. p. 419 (1850).

† *Pogg. Ann.* vol. xciv. p. 150. [Translated in *Phil. Mag.* for May, 1855.]

out under the most simple conditions—such, for instance, as the examination of the spectra of flames. Observations of this kind were made in the year 1845 by Professor W. Allen Miller, but they do not furnish any contribution towards a solution of the question. Dr. Miller has the merit of having first published diagrams of the spectra of flames*; but these diagrams are but slightly successful, although in a republication in the ‘Chemical News’† of the paper accompanying these drawings, Mr. Crookes remarks—“We cannot, of course, give the coloured diagrams with which it was originally illustrated; but we can assure our readers that, after making allowance for the imperfect state of chromolithography sixteen years ago‡, the diagrams of the spectra given by Prof. Miller are *more accurate* in several respects than the coloured spectra figured in recent numbers of the scientific periodicals.” In reply to this “assurance” of Mr. Crookes I only have to remark that, by way of experiment, I have laid Prof. Miller’s diagrams before several persons conversant with the special spectra, requesting them to point out the drawing intended to represent the spectrum of strontium, barium, and calcium respectively, and that in no instance have the right ones been selected.

Swan was the first who endeavoured experimentally to prove whether the almost invariably occurring yellow line may be solely caused by the presence of sodium-compounds. In his classical research “On the Spectra of the Flames of the Hydrocarbons”§ (referred to both in my ‘Researches’ and in the paper published by Bunsen and myself), Swan shows how small the quantity of sodium is which produces this line distinctly; he finds that this quantity is minute beyond conception, and he concludes—“When indeed we consider the almost universal diffusion of the salts of sodium, and the remarkable energy with which they produce yellow light, it seems highly probable that the yellow line R, which appears in the spectra of almost all flames, is in every case due to the presence of minute quantities of sodium.”

The strict subject-matter of Swan’s investigation was the comparison of the spectra of flames of various hydrocarbons. “The result of this comparison has been, that in all the spectra produced by substances, either of the form $C^r H^s$, or of the form $C^r H^s O^t$, the bright lines have been identical. In some cases, indeed, certain of the very faint lines which occur in the spectrum of the Bunsen lamp were not seen. The bright-

[* Phil. Mag. for August, 1845.]

† Chemical News, May 18, 1861.

[‡ Prof. Miller’s diagrams are not printed by chromolithography, but, as is seen on inspection, tinted by hand.—H. E. R.]

§ Trans. Roy. Soc. of Edinburgh, vol. xxi. p. 414.

ness of the lines varies with the proportion of carbon to hydrogen in the substance which is burned, being greatest where there is most carbon. . . . The absolute identity which is thus shown to exist between the spectra of dissimilar carbohydrogen compounds is not a little remarkable. It proves, 1st, that the position of the lines in the spectrum does not vary with the proportion of carbon and hydrogen in the burning body—as when we compare the spectra of light carburetted hydrogen $C H^2$, olefiant gas $C^2 H^2$, and oil of turpentine, $C^{10} H^8$; and 2ndly, that the presence of oxygen does not alter the character of the spectrum; thus ether, $C^4 H^5 O$, and wood spirit, $C^2 H^4 O^2$, give spectra which are identical with those of paraffin, $C^{20} H^{20}$, and oil of turpentine, $C^{20} H^8$.

“In certain cases, at least, the mechanical admixture of other substances with the carbohydrogen compound does not affect the lines of the spectrum. Thus, I have found that a mixture of alcohol and chloroform burns with a flame having a very luminous green envelope—an appearance characteristic of the presence of chlorine—and no lines are visible in the spectrum. When, however, the flame is urged by the blowpipe, the light of the envelope is diminished, and the ordinary lines of the hydrocarbon spectrum become visible.”

In this research, Swan has made a most valuable contribution towards the solution of the proposed question as to whether the bright lines of a glowing gas are solely dependent upon its chemical constituents; but he did not answer it positively, or in its most general form; he did not indeed enter upon this question, for he wished to confine his investigation to the spectra of the hydrocarbons, and was only led to the examination of this yellow line by its frequent occurrence in these spectra.

No one, it appears, had clearly propounded this question before Bunsen and myself; and the chief aim of our common investigation was to decide this point. Experiments which were greatly varied, and were for the most part new, led us to the conclusion upon which the foundations of the “chemical analysis by spectrum-observations” now rest.

2. I have likewise a few remarks to make concerning the history of the Chemical Analysis of the Solar Atmosphere.

The substance of the theory of solar chemistry which I have developed consists of a proposition which may be shortly stated as follows:—The relation between the power of emission and the power of absorption for each kind of rays (heat or light) is the same for all bodies at the same temperature. From this proposition it easily follows that a glowing body which emits only rays of certain wave-lengths, likewise absorbs only rays of the same wave-lengths; and from this we learn how the dark lines

in the solar spectrum reveal the constituents of the sun's atmosphere.

Angström, in his "Optischen Untersuchungen"* , states the proposition that a body "in the state of incandescence must emit exactly all those kinds of light which it absorbs at the common temperature." Then follow these words: "The proof of the truth of the above proposition is accompanied by great difficulties, because the elastic relations of a glowing body are quite different from those under which its power of absorption is examined." These words convey no meaning as they stand; they become, however, intelligible if we suppose that Angström really meant his proposition to imply that a body in the glowing condition must emit exactly all those kinds of light which at the same temperature it absorbs. Such an interpretation is, however, by no means favoured by the statement which immediately follows; for he adds:—

"An indirect proof of the truth of this proposition is, however, given by the fact discovered by M. Nièpce de Saint-Victor of the relation existing between the colour which a body imparts to a flame of alcohol, and that which light produces upon a silver-plate which has been treated with the chloride of the body in question. Thus a plate of silver treated with chlorine alone, assumes all the colours of the spectrum, but treated at the same time with a body capable of producing colour, it exhibits almost exclusively the colour which the body produces; this can only be explained by the prepared plate absorbing exactly the colour which the body in question imparts to a flame." Without endeavouring to follow this "proof" further, we see at once that the radiation of a soda-flame, for example, is here compared with the absorption effected by a cold plate of silver treated with common salt.

The meaning which is to be attached to this proposition is, however, rendered altogether doubtful by a remark which Angström makes in page 143 of the memoir above cited. He here states, "It is needful to observe that a medium not only absorbs the vibrations which it can most easily take up, but likewise those which stand in a simple relation to them, such as octave, third, &c." In order to see how these statements contradict each other, let us suppose a body which can take up certain vibrations with equal facility, but is incapable of taking up certain others; according to Angström's proposition, found in page 144 of his memoir, this body can only absorb the first kinds of vibrations; according to the remarks found in page 143, it must exert an

* Pogg. Ann. vol. xciv. 1853, p. 144. [Phil. Mag. vol. ix. p. 329.]
Phil. Mag. S. 4. Vol. 25. No. 168. April 1863. S

absorptive action not only upon these, but also upon certain other vibrations.

It is seen that the proposition which forms the basis of the chemical analysis of the solar atmosphere floated before Angström's mind, but only, indeed, in dim outline. The leading idea in the theoretical considerations upon which Angström endeavours to base the subject is the same as that which Stokes* has carried out more correctly on a later occasion when speaking of my first publication respecting the reversal of the spectra of flames. Stokes here compares the absorption which such a flame exerts upon the kind of rays which it emits, to the resonance which is excited in a body capable of taking up the vibrations of sound by a wave of sound of the same pitch as that which the body is capable of emitting. This comparison, if it can be followed out, may lead to the most important conclusions; but, in the manner in which Stokes gives it, it is of interest because it affords a good *illustration* of such an absorption, although it does not yield a *proof* of the proposition that a glowing body which emits only rays of certain wave-lengths also absorbs only rays of the same wave-lengths. The theory of resonance, and the theory of the production and absorption of the rays of light and heat, are still not sufficiently advanced to enable us at present to prove the proposition in question by any such comparison.

3. In the Transactions of the Royal Society of Edinburgh for the year 1858, a paper was published by Balfour Stewart, in which he describes very interesting experiments upon the radiation and absorption of partially diathermanous plates. He finds that a plate of rock-salt is less diathermanous for rays emitted by another plate of rock-salt heated to 100° C., than for those which are emitted from a surface of lampblack at the same temperature. From these and similar phenomena which plates of glass and mica exhibit, he concludes "that every body which sifts heat in its passage through its substance is more opaque with regard to heat radiated by a thin slice of its own substance, than it is with regard to ordinary heat." He then recalls the principle first enunciated by Prevost, that a body placed in a medium of the same temperature must absorb as much heat as it emits itself; and then he says, "Considering, therefore, the heat of any temperature to consist of heterogeneous rays, we may state the law thus: *The absorption of a plate equals its radiation, and that for every description of heat.*"

This proof cannot be a *strict* one, because experiments which have only taught us concerning *more* and *less*, cannot strictly teach us concerning *equality*. The proposition founded upon

* Phil. Mag. March 1860.

this conclusion cannot be considered to be thus proved, but must be taken as an hypothesis needing a stricter demonstration, as well as a greater precision in the terms employed. Stewart himself does not consider that his proposition is thus rigidly demonstrated; for immediately after the enunciation of it he says, "a more rigid demonstration may be given thus:" and then he proceeds to more abstruse considerations which are intended to give such a rigid demonstration, and in which the meaning he attaches to the expressions *absorption* and *radiation* are more nearly defined. These considerations, however, are not sufficiently general or sufficiently precise to attain the required end; so that, after all, Stewart's proposition remains an hypothesis to which some probability is attached.

Stewart finds from his experiments that partially diathermanous plates radiate the more heat the thicker they are, and hence he concludes, with perfect right, that the radiation proceeds from the interior of bodies as well as from the surface. The question then occurs, "Are we to suppose each particle of each substance to have at a given temperature an independent radiation of its own, equal, of course, in all directions?" "*À priori*," he continues, "this is the most probable supposition; and it seems likewise to be conformable to experiment." The principle expressed in these words is the test of the truth of the proposition, according to the proof which Stewart endeavours to give. He says, "The question arises, Is the law of an equal and independent radiation of each particle of a body theoretically consistent with equilibrium of temperature? That is, suppose we have any irregularly-shaped enclosure walled round with a variety of substances, and each particle of each substance radiating into the enclosure,—from the sides of which it is reflected many times backwards and forwards before it is finally absorbed,—this being the case, will the law of equal and independent radiation, and those of reflexion and refraction so fit with one another, that every particle of the walls of the enclosure shall absorb precisely as much heat as it radiates? It will be endeavoured to show that these laws are so adapted to each other."

By employing the law "of equal and independent radiation" and the laws of reflexion and refraction, Stewart forms the equation expressing the proposition which has to be proved concerning the equality of absorption and radiation for heat of every kind. It appears that this equation contains no contradiction, but expresses a possible property of the internal radiation in a body. He argues from this that the proposition concerning the equality of absorption and radiation for every kind of ray *must* hold good. This is evidently a false conclusion. The

above consideration proves that the proposition is *possibly*, but not that it is *necessarily true*.

The proposition under consideration is subsequently not carried out so generally as it is enunciated in the above-cited words, but only for a very special case. In connexion with these words he says, "and I shall select for the proof a definite form and description of enclosure—the conclusions arrived at rendering it highly probable (if not rigidly demonstrated) that the same adaptation will hold good for every enclosure however irregular or varied." The case which he considers is that of a body limited only by one plane with a black surface placed opposite and parallel to the plane. In the proof which Stewart in this case gives for his assertion, there is, lastly, an error which is rendered evident in the results at which the author arrives. He concludes, "We have, therefore, two laws necessary to the equilibrium of temperature: 1st, that the absorption of a particle is equal to its radiation, and that for every description of heat; 2nd, that the flow of heat from the interior upon the surface of a substance of indefinite thickness is proportional, *ceteris paribus*, to its index of refraction, and that for every description of heat." This second law (corresponding to the equation which, as already mentioned, expresses a possible property of the internal radiation of a body) is not correct; the magnitude therein considered is not proportional to the index of refraction but to its square*. On a later occasion † Stewart himself states (without, however, noticing his former contradictory assertion), "Now if R denote the radiation of lamp-black, and μ the index of refraction of an uncrystallized medium, it may be shown that the internal radiation as thus defined is equal to $R\mu^2$."

4. At the close of Prof. W. A. Miller's paper on coloured flames, before alluded to, he states, "It may be interesting to remark, in connexion with the speculations on the absorptive action of the sun's atmosphere, that if solar light be transmitted through a flame exhibiting well-marked black lines, the lines reappear in the compound spectrum, provided the light of day be not too intense compared with that of the coloured flame. This may be seen in the red light of the nitrate of strontia, and less perfectly in the green light of the chloride of copper. It would therefore appear that luminous atmospheres exist in which not only certain rays are wanting, but which exercise a positive absorptive influence on other lights." In his lecture "On Spectrum

* See Kirchhoff, *Untersuchungen*, &c. 2te Ausgabe, Berlin, 1862, S. 37.

† Report of British Association for the Advancement of Science, 1861, p. 107.

Analysis," reprinted in the 'Chemical News' of April 19, 1862, Prof. Miller quotes these words without making any remark upon the relation in which his observations, and the conclusion which he draws from them, stand to what I have discovered. In the Number of the same Journal for May 18, 1862, Mr. Crookes writes with reference to these same words, "This paragraph shows that Prof. Miller has anticipated by nearly sixteen years, the remarkable discovery, ascribed to Kirchhoff, of the opacity of certain coloured flames to light of their own colour." We need only to read Miller's words with some *slight* attention to perceive, not only that the conclusion to which he arrives is exactly the opposite of mine, but likewise that his conclusion is incorrect. If *weak* daylight be allowed to pass through a coloured flame, the absorption of the latter is not noticeable; its bright lines appear *brighter* than the surrounding parts, because in them the light of the flame is present in addition to the daylight.

5. Soon after my first short publication* concerning the chemical analysis of the solar atmosphere, I received the following communication in a letter from Prof. Wm. Thomson:—"Professor Stokes mentioned to me at Cambridge some time, probably about ten years, ago, that Professor Miller † had made an experiment testing to a very high degree of accuracy the agreement of the double dark line D of the solar spectrum with the double bright line constituting the spectrum of the spirit-lamp burning with salt. I remarked that there must be some physical connexion between two agencies presenting so marked a characteristic in common. He assented, and said he believed a mechanical explanation of the cause was to be had on some such principles as the following:—Vapour of sodium must possess by its molecular structure a tendency to vibrate in the periods corresponding to the degree of refrangibility of the double line D. Hence the presence of sodium in a source of light must tend to originate light of that quality. On the other hand, vapour of sodium in an atmosphere round a source, must have a great tendency to retain in itself, *i. e.* to absorb and have its temperature raised by light from the source of the precise quality in question. In the atmosphere around the sun, therefore, there must be present vapour of sodium, which, according to the mechanical explanation thus suggested, being particularly opaque for light of that quality, prevents such of it as is emitted from the sun from penetrating to any considerable distance through the surrounding atmosphere. The test of this theory must be had in ascertaining whether or not vapour of sodium has

* *Monatsberichte der K. Acad. der Wissensch.* 8vo. Berlin, Oct. 1859.

† [Professor W. H. Miller of Cambridge.—H. E. R.]

the special absorbing power anticipated. I have the impression that some Frenchmen did make this out by experiment, but I can find no reference on that point.

“I am not sure whether Prof. Stokes’s suggestion of a mechanical theory has ever appeared in print. I have given it in my lectures regularly for many years, always pointing out along with it that solar and stellar chemistry were to be studied by investigating terrestrial substances giving bright lines in the spectra of artificial flames corresponding to the dark lines of the solar and stellar spectra.”

At page 158 of the Philosophical Magazine for February 1862, Professor Thomson says, “The last eight or nine years Stokes’s principles of solar and stellar chemistry have been taught in the public lectures on natural philosophy in the University of Glasgow; and it has been shown as a first result, that there is *certainly sodium in the sun’s atmosphere*. The recent application of these principles in the splendid researches of Bunsen and Kirchhoff (who made an independent discovery of Stokes’s theory) has demonstrated with equal certainty that there are iron and manganese, and several of our other known metals, in the sun.”

From the above letter, which at my desire was printed in the Philosophical Magazine, S. 3. vol. xx. p. 20, and translated into the *Annales de Chimie et de Physique*, sér. 3. vol. lxii. p. 190, it is clear that many years ago Stokes in conversation had thrown out the *idea* that it would perhaps be possible to argue from the dark lines of the solar spectrum concerning the chemical constitution of the atmosphere of the sun. That this idea is correct—that a flame does, in fact, exert the absorption which Stokes ascribed to it, and that from the bright lines in the spectrum of an incandescent gas the chemical components of the gas can with certainty be deduced—was first proved by my theoretical considerations, and by experiments which I have made partly in conjunction with Bunsen, and partly alone. Hence it appears that no one had formerly (during a period of ten years) published anything concerning the opinion which Stokes expressed in conversation. In singular contradiction to this, stands Prof. Thomson’s recent statement, “that Stokes’s principles of solar chemistry have shown as a first result, that there *certainly is sodium in the sun’s atmosphere* ;” and further, “The recent application of these principles by Bunsen and Kirchhoff (who made an independent discovery of Stokes’s theory) has demonstrated with *equal certainty* the presence of other metals in the sun ”*.

Heidelberg, November 1862.

[* In a lecture delivered at the Royal Institution on April 12, 1862, I stated, with reference to this question (see ‘Chemical News,’ May 24,

XXXVII. *Reply to Prof. Tyndall's Remarks on a paper on "Energy" in 'Good Words.'* By P. G. TAIT, M.A., F.R.S.E., Professor of Natural Philosophy in the University of Edinburgh.

MY DEAR SIR DAVID BREWSTER,

AS an Editor of the Philosophical Magazine you are doubtless aware that Prof. Tyndall has, in the last published Number of that journal, taken exception to certain portions of an article on "Energy," published by Prof. Thomson and myself in 'Good Words' in October 1862. We feel that Prof. Tyndall's remarks cannot be allowed to pass without a reply, although we are most unwilling to introduce into the pages of the Philosophical Magazine matters directly personal to individuals. I shall therefore, as Prof. Thomson has requested me to reply for him as well as for myself, proceed to consider Prof. Tyndall's observations as briefly as I possibly can.

I think it right at starting to call Prof. Tyndall's attention to the fact that, in the Philosophical Magazine (1862, second half-year, p. 65), he has published the following words:—"I do not think a greater disservice could be done to a man of science than to overstate his claims: such overstatement is sure to recoil to the disadvantage of him in whose interest it is made,"—and to remind him that any unpleasant results which may follow from the course he has pursued are, by his own acknowledgment, to be laid to his charge.

First, then, as to the medium in which our article appeared, and the grave offence of putting two names to it instead of one. A journal which contains in nearly every Number a scientific paper by yourself, Forbes, Herschel, or Piazzì Smyth, can surely not be regarded as unsuitable for a paper on "Energy." I first saw a report of Prof. Tyndall's lecture (with its extraordinary statements regarding Mayer) in the pages of the 'Illustrated London News' and the 'Engineer.' I am not aware that these are journals specially employed by scientific men for the judicial discussion of recondite points in science or its history; nor can I consider a Royal Institution audience as a body qualified to decide upon such questions. An evident consequence of Prof.

1862), "These facts," of the coincidence of dark and bright lines, "remained altogether barren of consequences, as far as regards the explanation of the phenomena, except to a few bold minds, such as Angström, Stokes, and William Thomson; the latter two of whom, combining this fact with an ill-understood experiment of Foucault's made in 1849, foresaw the conclusions to which they must lead, and expressed an opinion which subsequent investigations have fully borne out. Clear light was, however, thrown upon the subject by Kirchhoff in the autumn of 1859, &c."—H. E. R.]

Tyndall's lecture was an article in 'Macmillan's Magazine' (August 1862), which found publicity in the peculiar society of "Water Babies," "Sunken Rocks," and "Women of Italy."

When I first saw Prof. Tyndall's lecture, I happened to be in Arran with Prof. Thomson; and, as he had been repeatedly asked by the Editor of 'Good Words' to contribute a scientific article to its columns, we seized the opportunity of distributing among its 120,000 readers a corrective to the erroneous information which we saw was stealing upon them through the medium of *popular journals*.

Second, as to our knowledge of what Mayer has done, which Prof. Tyndall allows may *now* be more complete than when our article was written. If Prof. Tyndall will refer to vol. xx. p. 262, of the Transactions of the Royal Society of Edinburgh (or to the Philosophical Magazine, 1852, second half-year, p. 9), where the paper is reprinted), he will find that so long ago as 1851 Prof. Thomson at least was well acquainted with Mayer's first paper, and had given him the full credit that his scientific claims can possibly be admitted to deserve. Prof. Tyndall is most unfortunate in the possession of a mental bias which often prevents him (as, for instance, in the case of Rendu and Glacier-motion) from recognizing the fact that claims of individuals whom he supposes to have been wronged have, before his intervention, been fully ventilated, discussed, and settled by the general award of scientific men.

So much for our ignorance of Mayer, our putting two pens to a paper instead of one, and our choice of a popular monthly magazine as a channel for publication.

Does Prof. Tyndall know that Mayer's paper has *no claims to novelty or correctness at all*, saving this, that by a lucky chance he got an approximation to a true result from *an utterly false analogy*; and that even on this point he had been anticipated by Séguin, who, three years before the appearance of Mayer's paper, had obtained and published the same numerical result from the same hypothesis? Prof. Tyndall has quoted, without comment, our note on the subject. Does he recognize the truth of that note? If he does not, let him expose its errors; and we shall be happy to acknowledge our mistake, and to hail the additions to scientific knowledge which (involving at least a reconstruction, if not a destruction, of thermo-dynamics) must result from Mayer's statement if it can be shown to be true.

As to the passage in our paper which seems to have especially displeased Prof. Tyndall, I think it sufficient to make the following remarks:—

Let Prof. Tyndall speak for himself. In his lecture he says, "To whom, then, are we indebted for the striking generaliza-

tions of this evening's discourse? All that I have laid before you is the work of a man of whom you have scarcely ever heard. All that I have brought before you has been taken from the labours of a German physician named Mayer." Now, in that lecture, there is little about the general principle of Conservation of Energy, and we certainly never intended to hint that Prof. Tyndall could have meant to put forward Mayer as having any claims to this great generalization, although his pupil in 'Macmillan' seems to have so interpreted him. What he does appear to claim for Mayer is, as in fact his lecture itself shows, a succession of results regarding transformations of energy which had been elaborated by mathematicians and naturalists from Galileo to Davy.

I am unwilling to enter upon matters of a more personal character, but it is impossible to pass over the fact that, in answer to an expostulation from Joule (having reference to this lecture), Prof. Tyndall referred (Phil. Mag. 1862, second half-year, p. 173) to statements he had made in a course of lectures which, he now tells us, were completed *before* he acquired those views of Mayer's claims to which Joule so naturally objected, and that he now replies to Prof. Thomson and me, not by showing that his lecture, to which we referred, was free from the objections which we urged against it, but by showing that these objections could not be urged against a certain statement which he quotes from a work, not published, but promised for publication. Referring to a passage quoted from his private materials for this work, he makes the following remarkable statement:—"If this recognition of Mr. Joule will not satisfy my critics, I cannot help it. It is simply a difference of estimate between them and me—a difference which may exist without the least infraction of good faith on either side. There is nothing here to 'startle' brave men, or to give the slightest colouring of truth to insinuations regarding 'depreciation' and 'suppression.'" It was not this, but something very different from this, which startled Prof. Thomson and myself. We did not insinuate, but we remarked, a tendency to depreciation and suppression—a tendency which no reader knowing anything of the history of science can fail to remark in that lecture as reported in the Philosophical Magazine and in the popular journals to which I have referred.

I am happy to find that Prof. Tyndall now takes a view of one important fundamental part of the labours of our illustrious living countryman with which Prof. Thomson and I can cordially agree. At the same time it would be unjust to Joule to omit the remark that this is but a *very* small part of what he has done for the science of energy—work which includes re-

searches on the heat generated by voltaic electricity, the heat of electrolysis and of chemical combination, vital dynamics, the calorific effects of rarefaction and condensation on the temperature of air, the ignition of meteors by friction in our atmosphere and the thermo-dynamic actions of elastic solids.

Believe me, my dear Sir David Brewster,

Yours most truly,

6 Greenhill Gardens, Edinburgh,
March 17, 1863.

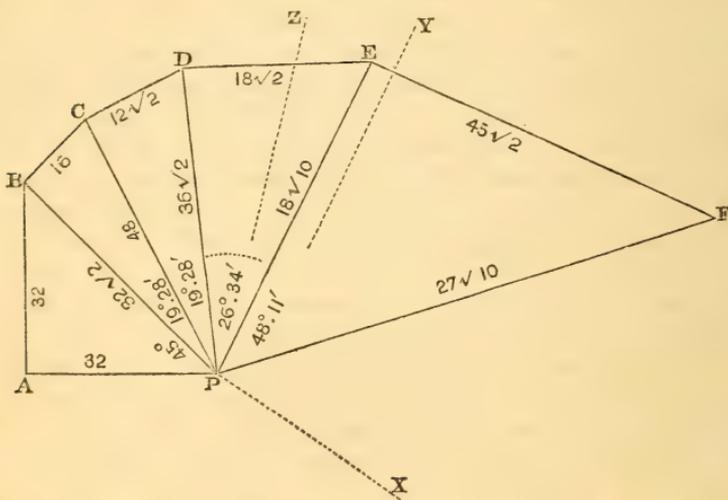
P. GUTHRIE TAIT.

XXXVIII. *The Polyhedric Fan: giving by common Geometry the Arc = Radius to within a four-millionth part of its true value.*
By S. M. DRACH, F.R.A.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

LET $AP = 32 = AB$, $BAP = 90^\circ$, join $BP = 32\sqrt{2}$, Draw $BC = 16 \perp BP$, $CP = 48$. Draw $CD = 12\sqrt{2} = \frac{3}{8}BP$ and $\perp CP$, $DP = 36\sqrt{2}$. Draw $DE \perp DP = \frac{1}{2}DP = 18\sqrt{2}$; then



$EP = 18\sqrt{10}$. Draw $EF \perp EP = 45\sqrt{2} = \frac{5}{4}$ of PD , and $PF = 27\sqrt{10}$. Lastly, let angle $FPX =$ half BPF . The dotted lines YP, ZP will be noticed afterwards. Then by Vlacq-Vega's ten fig. logs. we get

$BPC = CPD = 19^\circ 28' 16.394297$	Octahedron	} angle	
$DPE = 26^\circ 33' 54.184238$	Dodecahedron		less
$EPF = 48^\circ 11' 22.866613$	Icosahedron		90° .

Hence

$$B P F = 113^{\circ} 41' 49'' \cdot 839445 ;$$

one-half

$$= F P X = 56^{\circ} 50' 54'' \cdot 919722,$$

$$B P X = 170^{\circ} 32' 44'' \cdot 759167.$$

Now take $X P Y = \frac{2}{3}$ of $90^{\circ} = 101^{\circ} 15'$, and $Y P Z = 12^{\circ}$ or the central angle of a 30-gon (Euc. VI. last prop.), and $X P Z = 113^{\circ} 15'$; which, taken from $B P X$, leaves

$$B P Z = 57^{\circ} 17' 44'' \cdot 759167,$$

being only $0'' \cdot 04708$ (a 4370000th) below the true value $44'' \cdot 806247$.

I had previously tried other methods (*e.g.* $CPD + DPE - 11^{\circ} 15'$ gives $57^{\circ} 17' 10'' \cdot 58$); but this is incomparably nearer, and is a very curious effect of the peculiar polyhedral angles

$$2Oc + Do + Ic - \left(\frac{2}{3} + \frac{2}{15} = \frac{1}{1} \frac{5}{10}\right) 90^{\circ}.$$

As to the areas of the respective triangles, $APB = 512$, or $13 \cdot 688$ per cent.; $BPC = 256\sqrt{2}$, or $9 \cdot 679$ per cent.; $CPD = 288\sqrt{2}$, or $10 \cdot 889$ per cent.; $DPE = 648$, or $17 \cdot 324$ per cent.; $EPF = 810\sqrt{5}$, or $48 \cdot 421$ per cent. of the total area

$$P A B C D E F P = 1160 + 544\sqrt{2} + 810\sqrt{5}.$$

The angle $APF = 158^{\circ} 41' 49'' \cdot 839445$. The other polyhedral angles, viz.

$$35^{\circ} 15' 52'' = \frac{1}{2} B C P, \quad 54^{\circ} 44' 08'' = D C P - \frac{1}{2} B C P,$$

$$110^{\circ} 54' 19'' = P E F + \frac{1}{2} P F E,$$

$$70^{\circ} 31' 44'' = B C P, \quad 125^{\circ} 15' 52'' = D C P + \frac{1}{2} B C P,$$

$$121^{\circ} 43' 03'' = P E F + \frac{1}{2} P E D.$$

May I suggest the term *CORPORA DOCTI* for the five regular bodies? The second word is formed of their initial letters, and the extremes point out the numerically related Dodecahedron and Icosahedron, whilst the correlated Octahedron and Tetrahedron guard the central Cube.

Perhaps the stiffer framework of certain wings and leaves in organic nature may have geometric fans for their types, the angles being certain well-known quantities.

I have likewise found that

$$E P F + 3 D P F - 4 C P D = 49^{\circ} 59' 59'' \cdot 842139$$

differs from $51^{\circ} = 17 \times 3^{\circ}$ by $1^{\circ} 00' 00'' \cdot 157861$, or *one degree* increased by its 22800th part, which circle-graduator may perhaps find useful. There are probably many other combinations of the three angles leading to *nearly* accurate values of the sexa-

gesimal division of the circle. The square root of

$$\pi = \frac{16}{9} - \frac{16}{3000} + \frac{96}{10^7} - \frac{2}{10^7} + \frac{64}{10^{10}} + \frac{1}{16 \cdot 10^9} - \frac{1}{7 \cdot 10^{11}} + \frac{1}{7 \cdot 10^{15}}$$

true to the sixteenth decimal.

S. M. DRACH.

London, February 23, 1863.

P.S. $\sqrt[3]{2} - \sqrt{(\cdot 93^2 + \cdot 85^2)} = 0\cdot 00000037$ only, a *very near approach* to the geometrical representation of the side of the double cube.

XXXIX. *On the Events which produced and terminated the Purbeck and Wealden Deposits of England and France, and on the Geographical Conditions of the Basin in which they were accumulated.* By SEARLES V. WOOD, Jun.*

[With a Map, Plate V.]

THE Wealden deposit of the South-east of England has, from the time of its being brought into prominence by Dr. Mantell, been a fruitful subject of discussion, and has been indifferently referred to as a formation produced by a lake, a river, a delta, or an estuary, as the view of the author inclined; but the precise geographical conditions under which the deposit was produced have not, so far as I am aware, yet been made the subject of a separate essay.

In a paper read before the Geological Society in the year 1846, upon the so-called Wealden of Sutherlandshire†, Mr. A. Robertson entered upon a consideration of the conditions under which what he termed the freshwater deposits of the Portlando-Neocomian epoch were accumulated; and although he did not recognize that division between the Purbeck and Wealden which was afterwards established by Prof. E. Forbes, and has been since recognized, and refers to the same age formations that are now regarded as distinct, yet his views of the manner in which the basins containing these freshwater deposits were formed, if applied to the case of the principal one, that of the South-east of England, and if the peculiar conditions attaching to that area are considered, appear to offer an explanation of the phenomena under which the great freshwater deposit of Portlando-Neocomian age in England was produced. Mr. Robertson did not attempt to enter upon the consideration of these conditions in detail, but in general terms argues that the formations under discussion were deposited in closed basins, once

* Communicated by the Author.

† Quart. Journ. Geol. Soc. vol. iii. p. 113.

depressions of the bed of the sea of the last preceding epoch, but by the elevation of land reduced to the condition of inland basins, in which beds first of a brackish, and afterwards of purely freshwater character, were accumulated, and that these, by the subsequent depression of the area, were, in common with the intervals of dry land which severed the basin from the sea, overspread by the marine deposits of the succeeding epoch. I propose in this paper to endeavour to show in detail the applicability of this theory, with some modifications, to the case of the Wealden proper of the South-east of England.

Mons. Coquand has* described the fluviatile beds of the two Charentes, which, containing an intermixture of fluviatile and estuarine mollusca, overlie the oolite of Portlandian age, and underlie the upper cretaceous beds in those departments, and, following and recognizing the division established by Prof. E. Forbes, refers them to the age of the Purbeck rather than to that of the Wealden. He also derives these beds from lacustrine conditions rather than from the operation of a delta—conditions which he considers were produced by the elevation of the bed of the Portlandic sea, converting depressions in that bed into closed basins, much in the same manner as that suggested by Mr. Robertson.

It would also seem, from the description of M. Römer†, that most of the conditions which mark the Purbeck and Wealden respectively of England are repeated in the Purbeck-Wealden of Hanover. Before entering, however, upon the discussion of the causes which, I conceive, produced the features exhibited by the Purbeck and Wealden deposits of England, there is one other peculiarity which the beds of the Wealden in England exhibit that should be noticed; and that is the occurrence, at horizons separated from each other by a considerable vertical thickness of deposit, of footprints of air-breathing animals‡. These impressions, it is obvious, could only have occurred upon surfaces left dry by receding water, and require the assumption of a considerable and continuous depression of the bed of the lake in such a manner as to permit of a shallowness of bed (at least in the parts where the footprints occur) sufficient to allow of its being, by a reduction of the river supply, laid dry for an interval, at the same time that no such general depression of level took place as would admit of the sea regaining its place in the basin. It seems clear, from the features exhibited by modern deltas, that

* *Bull. de la Soc. Géol. de France*, vol. xv. p. 577.

† *Proc. Geol. Soc.* vol. iii. p. 323.

‡ See the papers of Mr. Beckles, *Quart. Journ. Geol. Soc.* vol. vii. p. 117; vol. viii. p. 396; vol. x. p. 456; vol. xviii. p. 443; of Mr. Tylor, vol. xviii. p. 247.

these impressions could not have thus occurred at horizons so (vertically) distant from each other during the progress of a delta-deposit; while the freshwater character of the deposits with which they are associated precludes the assumption of their being formed on the shores of an estuary undergoing a process of subsidence but silting up sufficiently fast to keep its shores in a condition to be exposed by receding tides. Thus it seems to me that the several phenomena which the character and grouping of the Wealden formation of England presents, demand the concession of geographical conditions at once peculiar and not to be paralleled by any known existing lake or river, in order that such phenomena may not conflict with each other. In the Wealden we have in places a thickness of perhaps 1400 feet, divided into at least two, if not more, horizons considerably separated, yielding evidences of having been at some time left dry to receive air-breathing animals, and afterwards silted over, while all these 1400 feet of deposit were accumulated in the immediate contiguity of the sea of the period. I cannot find that any of the great lakes with which we are familiar would furnish any conditions parallel to this; most of our existing lakes are depressions in rocky countries, high up in the course of large rivers. Even the Caspian, to which Mr. Robertson refers for the parallel of the Wealden, furnishes no parallel; for although that basin receives a very considerable sediment from the large rivers which flow into it, and seems to be laying bare the vast Aralo-Caspian deposit that surrounds it by a process of shrinking in its bed, yet that inland water, having a closed basin preventing the outflow of the water it receives, becomes in those parts which are distant from the embouchures of the rivers a brackish-water lake, harbouring a fauna which is not that of a purely freshwater basin. Moreover, the Caspian and its allied basin of the Aral, as well as the lakes of Switzerland and the great freshwater lakes of North America and of Central and Southern Africa, are situated remote from the ocean, while the grouping of the synchronous marine strata of the South of England show that the sea was in the very closest contiguity to the waters which furnished the Wealden deposit. Again, the process under which a delta formation is accumulated is one which seems to preclude the occurrence of purely freshwater strata to a thickness such as is presented by those of the Wealden. The thickness of the delta of the Mississippi is given by Sir Charles Lyell* at about 600 feet; the boring through the delta of the Ganges at Calcutta† reached, at a depth of 420 feet, beds which appeared to indicate that the bottom of the delta

* Principles of Geology, 1850, p. 219.

† Smith, in Proc. Geol. Soc. vol. iv. p. 4.

formation was almost reached; the delta of the Rhone has been pierced for 400 feet*; but I am not aware that borings have anywhere shown the existence of delta-deposits of even half the thickness of the purely freshwater beds of the Wealden proper, much less of the united formations of the Purbeck and Wealden; while in almost every case the borer appears to have passed through intercalated beds showing land-surfaces, and through conglomerates, of which the Wealden furnishes no parallel.

In attempting to realize the peculiar geographical configuration of the South-east of England during the Wealden epoch, it will be well to refer to the grouping of the strata that surround the Wealden deposits. The anticipations of Mr. Godwin-Austen, in his paper "On the possible extension of the Coal-measures beneath the South-east of England"†, were to a certain extent confirmed by the well-borings which took place about the same time in the north of London and at Harwich. In the former, after passing through the tertiaries, chalk with flints, chalk without flints, upper greensand, and gault, to an aggregate depth of 1113 feet, the borer reached a group of beds into which it passed for 188 feet, and which did not appear to belong to the lower greensand, but more probably, in the absence of recognizable fossils, to the trias. In the latter case the borer, after passing through tertiaries, chalk, upper greensand, and gault, reached a black slaty rock which, in his description of the boring, Mr. Prestwich says can, it is almost certain, only be referred to one of the palæozoic groups. The evidence afforded by the latter of these borings appears sufficient to substantiate the production, beneath the eastern counties of England, of the palæozoic axis of the Ardennes, which had been traced by borings to extend beneath the chalk as far west as Calais. The direction in which this axis is produced, however, appears to me to be more probably that adopted by Mr. Hull‡, viz. in the direction of the Warwickshire and South Staffordshire coal-fields, than in that adopted by Mr. Godwin-Austen, the direction of the coal-measures of the West of England. The borings which have been made through the chalk in the Pas de Calais and Département du Nord §, show that the coal there, occurring beneath a thickness of from 260 to 600 feet of overlying upper cretaceous deposits, lies in a trough of carboniferous limestone and Devonian rocks which stretches in the form of a slight curve having its convex side towards the S.S.W. The production of this curve extends

* Principles of Geology, 1850, p. 260.

† Quart. Journ. Geol. Soc. vol. xii. p. 38.

‡ Ibid. vol. xvi. p. 66.

§ Degoussée and Laurent, Quart. Journ. Geol. Soc. vol. xii. p. 252.

to the South Staffordshire coal-field, and precisely agrees, in that part which would be between Folkestone and Maidstone, with the chalk escarpment of the Weald Valley between those places.

The symmetrical form of the Weald Valley is interrupted at Maidstone—the axis of the elevation, which proceeds due east and west from the borders of Hampshire through Surrey and West Kent, being deflected at this point to the E.S.E., a direction that it maintains until the tumescence is lost inland of Boulogne. Now in the production of this palæozoic axis, which, bending northwards in the centre of the Pas de Calais, assumes in stretching to the Straits of Dover at Cape Grisnez the direction of E.S.E., we have presented to us a very apparent cause for the sudden deflection of the Weald anticlinal from its normal direction. The presence of a “massif” of highly indurated palæozoic rock, with its planes of stratification arranged at angles uncomformable to the beds of mesozoic age abutting up to it, could not, I conceive, fail to affect the upthrow of the beds subjected to the elevatory action, producing the anticlinal by a tendency to divert the line of upthrow into the direction of that of the ancient “massif.” The action to which I refer has, I imagine, operated in all countries in interfering with the lines of strike, the symmetrical direction of these lines being at points diverted or interfered with without any apparent cause*. By a reference to the chart annexed to the paper of MM. Degoussée and Laurent †, it will be found that the line of the carboniferous trough there indicated intersects, if produced under the chalk of Kent, the line of the anticlinal of the Weald Valley about Cranbrook in Kent. Now Cranbrook is the point at which the Weald anticlinal abruptly changes its direction of E. 5° N., W. 5° S.; to that of E. 25° S., W. 25° N., which is that of the carboniferous axis disclosed by the coal-borings of the two French departments. The point where the Isle of Wight anticlinal would intersect the carboniferous axis is east of Boulogne; but the point of intersection of a line drawn parallel to the palæozoic axis, and at a distance from it equal to the distance between the axes of the Weald and the Isle of Wight, would fall out at sea east of Beachy Head. The valleys of fracture in which the French rivers from the Seine to the Somme run, are productions of

* It would be irrelevant here to enlarge upon this subject, but there is much that might be adduced to show that the irregularity in direction of mountain-chains is proportional to the presence in them of “massifs” of older rocks which have been thrown into anticlinals of an origin anterior to the principal one of the system, and hence that the regularity of mountain-chains is (in Europe at least, which has undergone so great disturbance in every age) proportional also to the age of the system; the more ancient the system, the more regular its direction, if free from later disturbances.

† Quart. Journ. Geol. Soc. vol. xii. p. 252.

similar lines intersecting the axis of the Isle of Wight, but whose points of intersection are hidden from us by the water of the British Channel.

The drainage through Essex and Suffolk possesses a direction imparted to it by valleys which have rolled off, as it were, from the axis of the Weald upheaval, or been scooped out along lines of fracture connected with that upheaval. The valley of the Thames from Barking to Gravesend, which, after an interruption between that place and Chatham, is continued behind the Isle of Sheppey, departs but little from the line of the chalk escarpment between Folkestone and Maidstone. The valley of the next river northward (the Crouch) is parallel with the Weald axis; but the other rivers of Essex successively as we go northwards, the Chelmer, Blackwater, Colchester, Colne, and Stour, curve in the upper part of their courses towards the north, the convexity of the curve, like that of the Pas de Calais trough, being towards the south-west. The rivers of East Suffolk (the Orwell, Deben, and Alde) preserve this direction throughout their courses. The valleys, which in South Essex are parallel with the Weald, lose, like the rivers, that parallelism towards the north of the county; and the cretaceous and tertiary deposits in like manner change, although obscurely, the direction of their strike into one more and more oblique to the Weald axis, until in West Norfolk the strike of the lower chalk is north and south, changing this direction towards the east gradually, so that in East Norfolk the strike of the upper beds becomes N.W. to S.E., like that of the river-valleys of East Suffolk. On the other hand, at the extreme west of Norfolk the strike of the lower cretaceous and oolitic formations inclines from the north and south line in the opposite direction, viz. from N. by E. to S. by W., and towards that of the old Jurassic strike in this particular region—the Great Ouse following up to Shefford in Bedfordshire the Jurassic strike along the line of what I am about presently to refer to as the north-east opening of the mesozoic gulf, while the Waveney from Diss to the sea not improbably follows the outline of the north-western shore of the palæozoic barrier. I associate this gradual curving of the strike with the existence of the palæozoic barrier beneath it, and infer that the latter has made its presence felt in the counties of Essex and Suffolk by modifying the upthrow of the beds overlying it at the time of the Weald upheaval, not less than it has in Eastern Kent and Sussex, and in the portion of France opposite to those counties, by resisting the pressure of the less indurated beds of mesozoic origin there abutting up to it, and which in the upthrow were forced against it.

The conclusion at which Mr. Hull* has from other conside-

* *Quart. Journ. Geol. Soc.* vol. xvi. p. 66.

rations arrived, of the extension of this palæozoic barrier from the Warwickshire coal-field in an E.S.E. direction, appears to me therefore to obtain some confirmation from the interruptions in, or departure from, the normal direction of the anticlinal and synclinal lines produced by the Weald upheaval. If these conjectures have any sufficient foundation, the configuration of the east of England would, if the cretaceous and tertiary covering were removed, show very similar features to the west, where the palæozoic area curves northwards along the valley of the Severn—both the eastern and western palæozoic areas being separated from the Penine chain by straits, the severance of the two areas having been caused by the upheaval of that chain*. If the direction of this ancient barrier be correctly inferred, we have the following succession of geographical configuration over the south-east of England and north-west of France.

During the mesozoic period, up to the newer cretaceous epoch, a basin existed, bounded on the north-east by this barrier, on the north by the southern extremity of the Penine chain, on the north-west by the Silurian system of Wales and Shropshire, on the west and south-west by the carboniferous systems of South Wales and North Devon and the ancient rocks of Brittany, and to the south-east by the crystalline plateau of Central France. At the earlier part of this period, when the stage of greatest depression in most parts of it occurred, viz. during the Triassic age, the basin existed in the form of a strait connecting the Triassic sea of Germany with the Triassic waters of the north of Ireland on the one side, and those of the north-east of England on the other. At the commencement of the Jurassic age, however, when great changes of level had taken place, converting great parts of the Triassic sea of Germany into land, the Anglo-Frankish basin had become considerably modified. At this epoch the basin had four openings: one in Cheshire, forming the north-western opening and probably extending in a narrow

* The tract of which these palæozoic areas once formed part, appears to have been one of those axes to which I have elsewhere (*Phil. Mag.* 1862, vol. xxiii. p. 164) adverted as having come into existence during the carboniferous age in various parts of the world, with a direction generally east and west, marking the direction of the lines of volcanic action during that period. This axis, although now exposed only in fragments, appears to have been originally of great extent—the portion which extends from the west of Ireland to the east of Germany being probably connected on the east with a similar axis said by M. Abich to extend through Southern Russia, and on the west with the axis of Newfoundland. The upheaval of the Penine chain destroyed the continuity of that portion of the axis which crossed the British Isles; and its consequent and contiguous depressions formed the northern head and north-eastern and north-western openings of a basin that first, with a portion of the north-eastern opening probably as land, received the Permian sea, and afterwards, with that opening at its greatest submergence, the sea of the Trias.

channel north-westwards between Scotland and Ireland; another in Northamptonshire and the counties immediately south-east of it, forming the north-eastern opening; a third to the south-east in the department of the Côte d'Or, communicating with the Jurassic sea of the Jura and Alps; and the fourth in the south-west, forming a strait communicating with the Jurassic basin of the Charentes and Pyrenees. Of these openings, that to the south-east is considered by M. Elie de Beaumont to have closed after the termination of the Stonesfield oolite*; that to the north-west, Mr. Hull†, from a consideration of the manner in which the sediment of the Jurassic deposits of the central counties of England that are older than the upper zone of the great oolite has been accumulated, regards as having closed about the same epoch. The opening to the south-west would appear, from the grouping of the Jurassic beds of the region, to have closed also during the age of the great oolite; but the position of the basin of the Charentes and Pyrenees, the presence in the Charentes of the Portland limestone, and of a formation identical with the English Purbecks, coupled with the absence there of the older cretaceous deposits, induce me to suppose that a strait continued open, joining the Anglo-Frankish basin to that of the Charentes, until the close of the Purbeck epoch. The absence of the lower cretaceous deposits in the Charentes basin, when taken in connexion with the effects produced there by the upheaval of the Pyrenees, points to the conclusion that the only access of this depression to the sea was through a strait communicating with the Anglo-Frankish basin, which, when elevated, converted that of the Charente into land, or into a freshwater lake, until the upheaval of the Pyrenees, breaking up the barrier Atlantic-wards, gave the sea entrance to that basin from the west. That inference will derive much support if it should prove, as contended by M. Coquand, that lower cretaceous formations do not exist, as hitherto supposed, in the Pyrenees, and consequently not in any part of the Charento-Pyrenean depression.

By the close of the Portland oolite and sand formation, the only opening remaining unclosed, except the Charente Strait, was that to the north-east; and if the assumption that the Charente basin opened only into the Anglo-Frankish one be

* The presence of cretaceous beds in the departments of the Doubs and Jura conformable to the Jurassic, mentioned by M. Benoit and others, does not prove that the upheaval of the Côte d'Or system of M. de Beaumont took place at a later period than that assigned to it by M. de Beaumont, but only that the upheaval of the system was of less extent than he considered it, and was distinct from the subsequent upheaval of the Jura and part of the Côte d'Or during the cretaceous epoch, an upheaval possessing a direction more east and west than the former, and crossing it at acute angles. See Benoit, *Bull.* vol. xv. p. 316.

† See the paper last referred to.

correct, then both basins received the sea-water through the opening at the north-eastern extremity lying in or near the counties of Cambridgeshire, Buckinghamshire, and Bedfordshire; but if that assumption be untenable, then we have the Anglo-Frankish basin at least land-locked, except at its north-eastern extremity, where alone it communicated with the sea. Now it is apparent from the grouping of the oolitic deposits in the form of concentric rings round the basin, that the closing of the north-western and south-eastern openings was due to that elevatory action which, commencing at the close of the Triassic age, continued with comparatively slight and partial oscillations of level throughout the Jurassic. The last effect of that continuous elevation would be the closing of the strait which joined the English basin to the sea, and the conversion of the basin (and with it that of the Charentes) into one receiving fresh water only. Whether the closing of the Charente strait preceded the closing of that in Buckinghamshire and Bedfordshire, or the reverse, will depend on the parallelism that may be established between the Portlandian of the Charentes (upon which the gypsiferous marls of the Purbeck repose) and the Portlandian of Dorset; but the effect on the basins would in either case be the same in appearance, although in the former case the Purbeck formations of the two depressions would not be exactly synchronous, as the Charente basin would have been converted into one of brackish water before the English depression was severed from the sea, while in the latter the conversion of the English basin into one of brackish water would involve the similar conversion of that of the Charentes, which communicated with the sea through the English basin alone. No traces of the Wealden exist in the Charento-Pyrenean basin, so far as that basin is exposed; but it is not improbable that towards the more central portions of it, hidden by the overlying cretaceous and tertiary deposits, beds synchronous with the Wealden proper of England, and, like it, of purely freshwater origin, may exist. The greater distance of this basin from the principal line of depression (which, as I shall attempt to show, produced the peculiar conditions of the English basin from the Wealden to the commencement of the upper cretaceous epoch) renders it probable that it underwent a less amount of depression during the Wealden epoch than did the neighbouring basin of England (and this is to a certain extent confirmed by the absence in the Charentes of any lower cretaceous deposits); so that it would be inconsistent to find any purely freshwater deposit extending to the edges of the basin; and these edges are the only parts of it not covered up by newer deposits.

It thus appears to me that in this climax of the elevatory action of the Jurassic age, we have the clue to the configuration

which brought into existence deposits, first of brackish, and then of freshwater origin, of great thickness, in the immediate neighbourhood of the sea, that do not present any of the characters which mark deltas properly so called. In the basin thus isolated, we may easily conceive that deposits of limited thickness and of brackish-water character would accumulate during that interval in which the isolation was imperfect but progressing, and while the land was becoming stationary. The thickness of the Purbeck of England varies, according to the sections of the Geological Survey, between 180 and 380 feet. That of the Purbeck of the Charente is much inferior, being, according to M. Coquand, from 160 to 180 feet, a difference that would tend to show that the complete isolation of the latter basin preceded that of the former. In both, however, the amount of sediment is very small compared with that forming the Wealden, and not more than a river of moderate dimensions would furnish to a gradually shoaling basin.

It remains now to consider what were the conditions under which, in the English basin, a depression ensued that permitted the continuous accumulation of the far greater amount of sediment during the Wealden epoch, and under purely fluvial conditions, before the ocean re-entered the basin. In doing so, it will be useful first to consider what is the thickness of the beds in the South of England lying between the Portland sand and the lower beds of the upper cretaceous group.

The relative thickness of the Wealden beds at different points of their exposure is uncertain. At the western end of the Isle of Purbeck the section exposed is imperfect, through a fault occurring there; at Swanage Bay and in the Isle of Wight the lowest beds are not exposed; while in the Weald of Kent a similar difficulty exists, until it be determined whether the Ashburnham beds really belong to it or to the Purbeck. The exposed beds of the Wealden section at Swanage Bay, from a measurement furnished me by Mr. Jenkins of the Geological Society, show a thickness of about 1000 feet; and the unexposed portion at the same spot would, assuming the same dip to continue, give from 400 to 500 more. At the west of the Isle of Purbeck, the sections of the Geological Survey would appear to show considerably less than this, but the fault between the Wealden and Purbeck renders the thickness uncertain. Mr. Drew gives the entire thickness in Kent and Sussex, exclusive of the Ashburnham beds, at about 1200 feet*. So far as we can

* Quart. Journ. Geol. Soc. vol. xvii. p. 271, divided as follows:—

Weald clay	600 feet.
Tunbridge Wells sand	160 „
Wadhurst clay	160 „
Ashdown sand	250 „
(Base of Ashdown sand not exposed.)	
Total	1170 „

judge, therefore, the thickness seems to vary from about 1200 feet at its eastern edge, to nearly 1500 at its more central portion. We should expect to find the greatest thickness towards the centre of the basin, where the most sediment would be swept by the current; and in the case of the overlying marine deposit of the lower greensand, that is conspicuously the case. The sections of Mr. Simms* show the lower greensand at Hythe as having a thickness of 405 feet; and those of Dr. Fitton† at Atherfield give 805 feet, being nearly double that at Hythe. Atherfield is placed about the central part of the gulf, while Hythe is, as I conceive, close to the palæozoic barrier that, until the upper cretaceous epoch, shut in the basin‡.

The sum of the thickness of the deposits, from the base of the Wealden to the base of the upper cretaceous (which are absent at Harwich), would amount to about 2000 feet; and in the Pas de Calais and Département du Nord it would be more than this. We thus see that the shore of the Wealden lake was, after the close of the Purbeck epoch, skirted by a range of hills attaining a height of about 2000 feet above the *bottom* of the lake; and if, as we are compelled to do by the succession of footprint horizons, we consider the lake as shallow enough, during a considerable part of its existence, to be laid dry along its Sussex margin at intervals, then the greater part of that elevation would at the time referred to be above the *surface* of the lake. To what height above the sea the elevation of the ground in the region of the north-east opening extended, which, shutting out the sea, converted the basin into a lake, we have no means of forming any approximation. If the depression proceeded at an equal rate in the region of the north-east opening, that is, in Buckinghamshire and Bedfordshire, with what it did over the rest of the palæozoic barrier, we might, by comparing the thickness of any lower greensand beds present in those counties with that of the similar deposit in Kent and Surrey, estimate the elevation above the sea, of the land closing the north-east opening, by deducting the thickness of the lower greensand in the two latter counties from the thickness in the two first-

* Proc. Geol. Soc. vol. iv. p. 206.

† Quart. Journ. Geol. Soc. vol. iv. p. 327. Forbes and Ibbetson, *ibid.* p. 407, give 843 feet.

‡ A somewhat similar proportion between the northern and more central parts of the basin is exhibited in the relative thicknesses of the lower greensand in the Haute Marne and in the Aube—that in the former being about 280 feet, and that in the latter about 470. See Cornuel, in *Mém. Géol. Soc. France*, vol. iv.; and Leymerie, in the same. Both the Haute Marne and the Aube, being near the south-east shore of the basin, show a great attenuation of deposit from that in the more central portions of the basin lying in the South of England.

named. But this approximation would be very uncertain, for two reasons: one, because the depression was not, as I am about to attempt to show, equal over the two areas; and the other, because the flux and reflux of water through the strait during the formation of the lower greensand of Hampshire would impede the deposit of sediment in the strait itself, and carry it into the more open portions of the basin.

It is evident that the depression, of which the commencement marks the point when the elevatory movements of the oolitic age terminated and the cretaceous movements of subsidence began, had its focus upon the north-eastern sides of the Anglo-Frankish basin, and that its effects were for a considerable lapse of time confined to that part of the basin. The lower greensand deposits that overlie the Wealden in the Isle of Wight to the thickness of 800 feet, and in Kent along the border of the palæozoic barrier to a thickness of 400 feet, are, it is well known, either absent in Dorsetshire, or exist in an attenuated form like the bed occurring at the top of the Wealden at Punfield in Swanage Bay, referred to by Mr. Godwin-Austen; so that by the close of the lower cretaceous age the palæozoic barrier had sunk to the same level as the opposite shore of the basin in Dorsetshire, where the upper cretaceous overlies alike the oolite and the Wealden, being a subsidence of some 1500 feet in excess of the one region over the other during the lower cretaceous epoch. A similar effect is to be observed in the French portion of the basin. M. Triger, in his description of the cretaceous deposits of La Sarthe*, shows that as we advance southwards towards the edge of the basin, the lower beds of cretaceous age disappear, and the cretaceous beds covering the Jurassic formations become successively newer and newer,—the lower greensand, which at Cape la Hève reposes on the Jurassic, being at Honfleur replaced by the gault, which reposes there on the Kimmeridge clay; while more southwards, in the department of La Sarthe itself, the upper greensand or the chalk marl is the deposit which reposes on the oolitic formations; so that the depression which permitted the sea during the cretaceous age to reoccupy the oolitic basin, palpably diminished in amount as the distance from the line of the oolitic barrier increased.

In Norfolk, according to Mr. Rose†, the beds which represent the chalk marl and lower and middle chalk attain a thickness at Norwich of 1150 feet, while, according to the same authority, the lower greensand at its outcrop some 40 miles west from Norwich has a thickness of only from 70 to 90 feet. This circumstance appears to show the effect of the flux

* *Bull.* vol. xv. p. 543.

† *Proceedings of Geologists' Association*, vol. i. p. 227.

and reflux of the tide through the north-east opening during the formation of the lower greensand in impeding the accumulation of sediment; for if my views are well founded, the sea, after the close of the lower greensand deposit, found its way through various parts of the barrier then changing into the condition of a cluster of islands, so that the flux and reflux in the region of the old strait became less and less, until a deep-water condition was attained permitting the accumulation of this great thickness of chalk. Again, according to the same authority, at a distance of only twenty miles south of Norwich, at Diss, where, if my delineation of it be tenable, the north-western shore of the palæozoic barrier was near, and the water consequently shallower, the chalk has a thickness of only 500 feet. A much less thickness than that at Norwich appears to prevail pretty generally over the region of the barrier, except that at Harwich it rises to 880 feet; while in the Isle of Wight, over the central part of the old basin, and therefore in a deep part, the greatest thickness of any is found*. The increased thickness on the borders of Essex and Herts and in Sussex corresponds with the edges of the barrier being there passed †.

The presence of the upper greensand overlying gault in Norfolk and Essex as well as in Kent and Sussex, although very attenuated in the first county, shows that the diminished thickness of the chalk in the parts where such diminution is observed is not due to those parts undergoing submersion at a later stage of the chalk than others. Yet we may infer that the shallower condition of the water over the barrier impeded the accumulation of sediment there, a result to which perhaps some islands still remaining above water during the deposit of the upper greensand and of the earlier part of the chalk may have contributed by inducing currents between them.

It is worthy of remark, also, how the mineralogical character of the cretaceous deposits around and over the palæozoic barrier mark the successive stages of the depression which prevailed through that age, until the close of the middle chalk or chalk with flints. While the basin was a quiescent inlet, the sediment swept into it was of an arenaceous character, more or less inter-

* About 1300 feet. Fitton, Geol. Trans. N. S. vol. iv. p. 318.

† Mr. Prestwich (Quart. Journ. Geol. Soc. vol. viii. p. 256) attributes the varying thickness of the chalk to denudation anterior to the deposit of the lower tertiaries, and shows that from this cause the chalk over the North Downs has in places been reduced to the lower beds only, and to a thickness of 400 feet; and this agrees with the views above, as, the region of the Weald being beyond the barrier, the deeper-water conditions prevailed, permitting a free accumulation of sediment that is now in places extensively removed. The varying thickness of the chalk in many places over the barrier cannot, however, be explained by denudation.

mixed with lime, and in some parts, and particularly during the earlier period of the lower greensand formation, with argillaceous material; but at the point when the palæozoic barrier reached the water's edge, forming an extensive reef rising in parts into islands, the degradation of such shale as that brought up by the borer at Harwich, taking place over an extensively shallow area, furnished the dark aluminous silt that forms so marked a feature in the gault of the South-east of England, and was deposited over and around that barrier. As the depression proceeded uninterruptedly, the stages of sand, marl, and pure chalk successively occur over the region, marking the increasing stages of depth until something approaching to oceanic depth was reached, when the flint-layers grew.

In the paper before referred to, I attempted to show that the geological movements of the mesozoic period, up to the end of the oolitic age, were due to several great systems or bands of volcanic action in different parts of the earth, which possessed a direction coinciding more or less with the meridians of longitude, the effect of which was to produce a trend of the land generally from north to south; and that it was to one of these bands, which I designated as the system of Portugal prolonged into England, that the continuous elevation of the Jurassic gulfs of England and France, which lay on the eastern side of the band, was due. I also pointed out that the period of depression which succeeded this commenced (in Europe at least) during the lower cretaceous epoch, and was marked by a total change in the direction of the volcanic action from the direction of north to south which had prevailed since the commencement of the Permian age, to one from west to east, a direction which indicated, with very few exceptions, the prevalent one of all systems originating since the lower cretaceous epoch. The commencement of this total change I regarded as marked by the formation of the system of the principal part of the Pyrenees, which, according to M. d'Archiac, originated towards the close of the lower cretaceous epoch*. Now the mode in which the subsidence of the Anglo-Frankish basin took place during the formation of the Wealden deposit, seems to me well to illustrate some of the effects which this total change in the direction of the volcanic action produced. Thus, while the elevatory action of the oolitic age over the north-west of Europe was due to a line of volcanic action of which the direction may be indicated by the seventh meridian of longitude, extending from the Portuguese frontier to

* If M. Coquand's view, that the lower cretaceous deposits are absent in the Pyrenees equally as in the Charentes, be correct, it would seem that the origin of the Pyrenean system must be advanced to a later part of the cretaceous age than that assigned to it by M. d'Archiac.

Skye, the direction of the line of subsidence which prevailed over the same area through the cretaceous age, until the great continental upheaval commenced at the epoch of the Mæstricht chalk, is indicated by the parallel drawn almost at right angles to the former line, and extending from Dorsetshire to Valenciennes, coinciding in direction precisely with that of the axis of cretaceous elevation in the Pyrenees, to which it may be regarded as the complementary line of depression. We also see that this subsidence was at its maximum over the line of the old palæozoic barrier, with whose anticlinal it very nearly coincided, and that it sensibly diminished as the distance from that line increased; while along the line of Jurassic elevation neither subsidence nor elevation took place, the volcanic action of that band having become extinct at the close of the oolitic period, and the movements of the band theretofore affected by it having consequently terminated. We thus perceive how the ocean, kept out of the Wealden basin during the progress of that formation, did not, when the subsidence of the barrier permitted it to re-enter, overflow the outcrop of the upper oolites surrounding the basin—an overflow which never more than partially took place, and that not until the barrier had been submerged sufficiently to receive the deposits of the gault and upper greensand over it. By the close of the epoch of the chalk with flints this barrier had undergone a depression from its condition at the close of the Purbeck epoch of little less than 3000 feet. The maximum of depression varied over the line itself during the earlier portion of the lower cretaceous age, since, while a depression ensued at that time in Dorsetshire sufficient to deposit a great thickness of Wealden formation over the Purbeck, no further depression of any magnitude ensued at that extremity of the line until the upper greensand epoch. From the concentration of the action of subsidence in different parts of the barrier at different stages of the cretaceous age which appears to have taken place, and particularly from the chief concentration appearing to have occurred in the part between London and Valenciennes, it does not seem necessary to infer any very considerable elevation of the bed of the north-east strait during the progress of the Wealden deposit. In Oxfordshire, across the south of which county I place the northern margin of the lake, and near which, in the adjoining counties of Buckingham and Bedford, I place the north-east opening, the lower greensand underlying the upper cretaceous deposits occurs, according to Prof. J. Phillips*, in a very attenuated form, showing, unless the flow of the tide prevented the accumulation of sediment, how little depression that extremity of the barrier underwent during the lower cretaceous

* Quart. Journ. Geol. Soc. vol. xvi. p. 307.

age. The existence of the north-east opening during the oolitic age, in the form of a strait not caused by any abrupt dislocation of the land surrounding the basin to which it gave access, furnishes presumptive evidence that the bed of it was shallow and capable of being converted into land by a very moderate elevation, resembling in this respect the Sound entrance to the Baltic at the present day. The absence of all appearance of convulsive movement in that region at the close of the oolitic age would also show that the opening became converted into land by merely participating in the general elevation which so uninterruptedly marked that age until its close, becoming by the process a low-lying tract. The apparent discrepancy, that this low-lying tract should not have been submerged while the rest of the palæozoic barrier and more central part of the basin were undergoing a depression of some 1200 or 1400 feet, is removed by the proofs which exist that the extremity of the barrier which abutted on the midland counties did not undergo any material depression until the upper cretaceous age, and that even during that age it marked the north-westernmost limit of the depression then proceeding.

In realizing, therefore, the process of depression which marked the formation of the Wealden, we have to conceive the depression taking place along the line from Dorsetshire to Valenciennes to have been at its maximum over the palæozoic barrier, diminishing in amount at the western extremity of the line, and losing its force as the distance southwards from the line increased; so that by the time the central part of that line had subsided to the extent of 1200 feet and upwards, the low-lying land of the north-east opening was again submerged to an extent probably only sufficient to permit the ingress of the sea-water to the basin, and not of the deposit of any thickness of sediment over the opening itself. On the sea re-entering, it would be only the most depressed area that would be occupied by it, those portions of the basin lying west and south of the line of depression remaining land until the continuance of the process of depression or attenuation brought them successively below the sea-level. Hence the absence of lower cretaceous marine deposits in Dorsetshire, and hence the successive thinning out of the lower cretaceous deposits through the departments of Calvados, Orne, and Sarthe. Into the deeper or more central portions of a land-locked gulf the greater quantity of sediment would be swept; and hence the maximum thickness of the lower greensand occurs at Atherfield, diminishing north-eastwards towards the palæozoic barrier, and disappearing or thinning out westwards in Dorsetshire, and southwards in the Calvados.

The whole grouping of the Purbeck formation would have

pointed to a truly delta origin in the absence of the peculiar geographical conditions affecting it in common with the Wealden; but the stage at which the elevatory movements of the oolitic age terminated and converted the marine gulf into an inland basin, concurs with the formation of this deposit. As Mr. Robertson in the paper before quoted showed, the earlier deposits of a basin recently severed from the ocean would be those of brackish water; but this brackish condition could scarcely have endured, under the influence of the passage of fresh water through it, long enough for the accumulation of 300 feet of sediment. The brackish condition of the Caspian and its allied basins, and of some other lakes, is due to their being closed basins, where the evaporation balances or exceeds the supply of fresh water, so that the salts washed from the soil and held in solution in imperceptible quantities in the river-water gradually accumulate. Where this is not the case, the basin has an outlet and the waters are fresh. We can scarcely conceive the conditions of the basin as regards river-supply and evaporation to have been so different during the Purbeck epoch, to what was the case during the Wealden, as to allow us to seek an explanation of the brackish-water condition of the basin during the former epoch by assuming that it was a closed one where evaporation equalled the precipitation. We must rather infer that during this epoch channels through the north-east opening permitted the access of the tide, and thus made brackish the waters, and that the final movements which terminated the oolitic age rendered the fall of these channels too great for the tide during the succeeding age to reach the basin.

Of the many formations of oolito-neocomian age containing freshwater mollusca, either solely or intermixed with estuary forms, a few only would, if the foregoing conclusions are well founded, be common to the same basin with the Wealden of England. Of these, the ferruginous sands of Shotover near Oxford, shown by Prof. J. Phillips* to repose at Coombe Wood and Garsington in Oxfordshire, and in parts of Buckinghamshire and Bedfordshire, on beds referable to the Purbeck, seem alone to be unequivocally referable to the deposits formed in the basin during the Wealden epoch; but the underlying brackish-water beds in these counties, and the brackish-water beds in the Bas Boulonnais, appear clearly to be parts of the earlier deposits of the same basin, and synchronous with the Purbeck deposits of Dorset and Wilts. The unfossiliferous sands and marls of small thickness containing slight vegetable impressions, described by M. Cornuel as occurring in the Haute Marne, between the lower greensand beds and the Portland limestone, and regarded by

* *Quart. Journ. Geol. Soc.* vol. xiv. p. 240.

Dr. Fitton as of Purbeck or Wealden age, undoubtedly belong to the Anglo-Frankish basin; but whether they are to be referred to the Purbeck or to the Wealden, or to neither, but to the overlying or underlying marine formations, is a matter of much doubt; at least we may affirm that, if they are eventually referred either to the Purbeck or Wealden, they formed the extreme south-easternmost margin of the lake. The other fluviatile formations of oolitic age, viz. those of the Hautes Alpes, of Skye, of Sutherlandshire, and of Lincolnshire, have no geographical relation to the formations occurring in the South-east of England, or, except those of Skye and Sutherlandshire, with each other. The great Purbeck-Wealden formation of Hanover, although most probably synchronous with the similar formations of the South-east of England, was manifestly geographically severed from them by the palæozoic barrier. On the other hand, the fluviatile deposits of Beauvais, and those described by M. Coquand as occurring in the Isle of Aix, belong to a later geological horizon, and have no connexion either geographically or geologically with the fluviatile formations discussed in this paper.

It follows also, from the conclusions here drawn, that the marine equivalent of the Wealden (the lower Neocomian of the South-east of France) should occur in the north-eastern counties of England beyond the north-east opening, being necessarily absent over that area which the Anglo-Frankish basin occupied during the progress of the Wealden deposit. In these counties and in Western Norfolk, however, the contiguity of the shore may have rendered the water too shoal to permit of the deposit of any older cretaceous formations approaching in thickness to the lower greensand of the basin itself, and in West Norfolk the action of the tide setting in towards the opening may have tended to check the accumulation of sediment.

In considering the sources and direction from which came the waters that fed the basin with sediment during the Wealden epoch, we have to consider the nature of this sediment, and the character during the time of its deposit of the land which surrounded the basin. As to the first, those who have made the Wealden their study appear agreed that the source of the sediment was chiefly an area of metamorphic rocks. The absence or rarity of limestone in the beds of the Wealden itself is one of its most marked features; but this absence may not be altogether due to the drainage-area being destitute of limestone tracts; the soluble nature of lime permits of its being carried into the sea, and held there in suspension after all the aluminous and siliceous sediment has been deposited. The rarity of limestone cannot, therefore, I think be adduced as an argument that the Wealden waters were derived from an area in which the

presence of limestone formations was the exception. The beds of the Wealden, however, are of a highly argillaceous character throughout, the extensive sand-beds among them being rarely free from a considerable intermixture of aluminous particles, as the wet and intractable soil of nearly the whole Wealden area attests. It is therefore not unreasonable to infer that districts abounding in metamorphic rocks formed the larger portion of the area from which the waters that furnished the Wealden sediment were collected. There are no such areas from which we could derive these waters at all comparable in magnitude with those surrounding the western part of the English Channel, or the shores of the St. George's Channel, both of which channels are formed by depressions dating back into the palæozoic period, and consequently existing as valleys of drainage during the Wealden epoch.

To estimate the lay or inclination of the land surrounding the Wealden basin during the progress of that formation, it is necessary to eliminate from the features which that land now presents the effects that have been produced by the great tertiary anticlinals of the South-east of England. Anterior to the formation of these anticlinals the South-east of England remained, until the close of the Wealden formation, a basin more or less landlocked; and after the eastern barrier had become submerged, and during the upper cretaceous age, it formed the site of a deep bay or gulf, after which, with an interval of dry land between the secondary and tertiary periods, it remained an open gulf, although diminished in extent, until the age of the crag. The anticlinals, in converting this sea-bed into elevated land, reversed the inclination that the area of the British Channel possessed during the entire mesozoic period. The extent of this reversal is to be measured by the amount of elevation which the sea-bed has undergone where these anticlinals occur. The amount of this is probably not less than 4500 feet*, and has affected the whole of England east of a line joining Dorsetshire and the Norfolk coast. Although this fact is so well understood, I enlarge upon it here to show how great must have been the effect of these anticlinals in modifying the inclination which, previously to the upheaval taking place, the land of the South-west of England possessed. The whole of the British Channel east of Dorsetshire

* In the Isle of Wight we have at the least the following above the sea-level:—

Tertiaries to the top of the Hempstead series (Forbes)	2030	feet.
Upper cretaceous and gault (Fitton)	1500	„
Lower cretaceous (Fitton)	800	„
Wealden incompletely exposed, say (Forbes)	200	„
Total	4530	„

has been made by it, and that west of Dorsetshire has been submerged, and its inclination (which was eastward towards the shore of the mesozoic basin) reversed, so that the water of the British Channel now deepens as we go westwards.

The area of Portugal, which during the secondary period up to the cretaceous age was affected by a line of volcanic upheaval running north and south, is, with the exception of the mesozoic deposits accumulated on its western flank, one of metamorphic rocks, these rocks extending through the Spanish province of Galicia to the Atlantic and to the Bay of Biscay.

This area was, I conceive, during the oolitic period joined to that of the British Isles and under the elevatory influence of that line of volcanic action to which I have before referred as the system of Portugal prolonged into England. The junction was probably severed by the western depression correlative to the upheaval of the Pyrenees, partly during the cretaceous, and partly during the older tertiary epochs. The extent of the correlative depression may be inferred from the extent to which the cretaceous and older tertiary sea-bed of the Pyrenees has been elevated by the upheaval of that chain. If we in imagination restore the junction of the metamorphic system of Portugal with Finisterre and the west of England, and eliminate from the problem the western depressions complementary to the tertiary anticlinals of the South-east of England, and the cretaceous and tertiary anticlinals of the Pyrenees, we shall have little difficulty in restoring the areas of the St. George's Channel and of the English Channel west of Dorsetshire as great valleys—the former opening into the latter, and the latter opening into the mesozoic basin between Dorsetshire and the mouth of the Seine. These great valleys opening thus, and surrounded as they are, chiefly by lower Silurian formations in which limestone is the exception, or by Devonian or Silurian formations in a more or less metamorphic condition, appear, without speculating on the now submerged formations lying between Portugal and the British Isles, to offer the most probable valleys of drainage from whence came the waters which furnished the Wealden sediment. Assuming, however, that this was so, we may nevertheless admit that many subordinate valleys of drainage surrounding the basin contributed to furnish the sediment. But the chief mass of the sediment being accumulated in the region where a river emptying itself through this valley would discharge into the basin, points to that as the principal source of the supply.

In parting with the subject, I cannot refrain from advancing the conjecture that the Jurassic basin of the Charentes, in which such similar phenomena occur as are present in the Jurassic basin of England and France, was governed by geographical

conditions not dissimilar to those of the latter basin, that is, that during the Jurassic age it was a basin becoming more and more landlocked with each successive epoch, under the elevatory influence of the system of Portugal and England, on the eastern flank of which, like the Anglo-Frankish basin, it existed, until at the close of the Jurassic age it ceased for a long interval to be a marine basin, all connexion between it and the ocean being severed by the elevation, at the close of the Purbeck epoch, of the strait communicating with the basin of England and France. After the lapse of this long interval, when the sea during the upper cretaceous age occupied both basins, there appears no reason for assuming that any connexion between them again existed. It is true that, according to the researches of MM. Triger, Sæmann, and Coquand, features occur in the grouping of the upper cretaceous deposits of La Sarthe (forming the south-western margin of the Anglo-Frankish basin) similar in all respects to those occurring in the Charentes, and indicating a corresponding amount of depression during the upper cretaceous epoch on either side of the isthmus formed by the Jurassic deposits of the Vienne; but this similarity has reference to a correspondence of depression only, and not to an intimate zoological connexion—the zoological relations of the upper cretaceous deposits of the Charentes appearing to be with those of the Pyrenees rather than with those of England and North France*. It appears to me, therefore, that while, during the upper cretaceous age, the Anglo-Frankish basin was well opened to the ocean north-eastwards by the subsidence of the palæozoic barrier, the Charento-Pyrenean basin was opened in the contrary direction to the ocean by the subsidence of the tract which, during the previous part of the mesozoic period, joined Portugal to England, and bounded that basin on the west, but that both series of movements were alike correlated to or consequent upon the elevation of the Pyrenean chain then in progress. In using the expression “correlated to or consequent upon,” I desire to guard myself against being understood to mean that these movements were entirely synchronous with the elevation of the Pyrenees. Indeed it is not improbable that the subsidence of the palæozoic barrier preceded the elevation of the Pyrenees; but the relation of the movements in either area to each other was due to one great band of volcanic action, which came into existence from west to east during the lower cretaceous age, and of which we have the most conspicuous effects exhibited in the great anticlinal of the Pyrenees, and consequently refer to it, in one comprehensive term, as the system of that chain.

* Lyell's 'Manual,' 1851, p. 221.

The accompanying sketch map (Plate V.) will enable the arguments that I have attempted to adduce to be followed more readily, and the conclusions I have drawn from them to be seen at a glance; and in it I have traced the conjectural outline of the Charento-Pyrenean basin.

XL. *Note on the Compressibility of Gas*

By C. K. AKIN, Esq.*

IN the following paper will be proposed a few remarks, made some considerable time ago, the gist or moot point of which is to be the degree of numerical accuracy belonging to the several formulæ which have been computed by M. Regnault in the latter part of his celebrated memoir on the law of Boyle (or Mariotte), from the experimental data contained in the early part †—the purpose which these formulæ are to serve being to supplement or (if properly written) to supersede provisionally the Boylean formula to a certain extent, until the complete law of gaseous compressibility shall have been theoretically or otherwise determined. The importance of the formulæ referred to being obvious, the necessity of evaluating the several constants which they imply with all possible rigour, or with so much, at least, as the nature of the experimental groundwork upon which the formulæ rest will not render futile to attempt, will be no less evident. And if the actual deduction of the constants supplied should, as really appears to the present writer, *not* fulfil the requirement specified, within limits at once attainable and intended to be attained by the computer, the obligation to direct attention to the matter could not well be avoided, and is in this case the more easy to comply with, as the means of remedying the observed shortcomings may be readily pointed out.

1. The first of the remarks to be submitted involves, as a necessity, some preliminary statements as to the proper mode of applying the law of Boyle in certain very common instances. If there be nothing namely to object to in the ordinary definition of this law, as the postulate requiring the direct proportionality of pressures and densities, or the inverse proportionality of pressures and volumes, in gases when at a constant temperature, so far as it goes, it must still be observed to stand in need of some special help to interpretation, in the case of gaseous masses the volumes encompassed by which are finite in regard of vertical extent. Or rather, whenever it is expedient to apply the law in

* Communicated by the Author.

† See "Mémoire sur la Compressibilité des Fluides élastiques," *Mémoires de l'Académie des Sciences*, vol. xxi. p. 329.

this case, it will presently be proved to be necessary to depart from what are conventionally called the pressure and density of a gaseous mass in daily practice, and to exchange at least one or other, density or pressure as habitually understood, for one different from either. For since it is known, in the first place, that, consequent upon the action of gravity, each separate layer of infinitesimal vertical extent, an aggregate of which only forms a finite mass, has its own particular density and pressure, it will follow as a first consequence that only such densities and pressures as in a given mass coexist in the same infinitesimal layer can be considered as congruous or correlative within the meaning of the Boylean law; whilst such densities and pressures as belong to *different* layers must be connected by some more complicated formula, dependent on the magnitude of the mass which separates the layers considered. Practically, however, what is denominated the pressure of a gaseous mass is the pressure of its lowest-situated layer, which is in contact with the liquid employed for evaluating the pressures in the manometer, this last pressure being generally alone accessible to measurement; but for density, on the other hand, following the extended definition of the term, is taken the mean of the densities of all the component layers, or the mass divided by the volume—and this also in the sense of density of the integer mass. Since this last density, however, is necessarily identical with that of some medium layer situated between the top and bottom of the mass, considered as made up of a continuous pile of infinitesimal horizontal layers, whilst the pressure, before adverted to as generally viewed in the light of that of the mass considered as a whole, belongs, in fact, to the layer at the bottom only, it must be evident that these two, pressure and density as just described, are *not* correlatives in the sense which is implied by the Boylean law. Hence the necessity, as bottom-pressure and mean density are generally alone measurable, to take one *or* other of the two couples, pressure and density at the bottom, or mean pressure and mean density, as characteristic of a gaseous mass—at least whenever it is desired to apply to such the law of Boyle, which above was shown to be inapplicable to the densities and pressures of different layers promiscuously combined.

The precaution enjoined in the preceding sentences it is certainly needless to take in ordinary applications of the Boylean law, for reasons which are sufficiently evident: in the case of M. Regnault's experiments, however, to neglect it will entail appreciable numerical errors, as shall now be shown. For this purpose it will be necessary to quote a formula (the demonstration of which shall be given in an Appendix) exhibiting the actual relation between ordinary pressures and volumes (the former

being bottom-pressures, and the latter convertible with mean densities) in gases, as deducible from the Boylean law. The formula is the following,—

$$\frac{p_0}{p_1} = \frac{1 - e^{-\frac{D}{H} a_1}}{1 - e^{-\frac{D}{H} a_0}} = \frac{a_1}{a_0} \frac{1 - \frac{1}{2} \frac{D}{H} a_1 + \&c.}{1 - \frac{1}{2} \frac{D}{H} a_0 + \&c.}, \quad \dots \quad (\text{I.})$$

wherein p_0 and p_1 denote two pressures corresponding to the two volumes, or rather the two heights a_0 and a_1 , of the identical cylindric or prismatic vessel in which the gas, invariable itself in mass, is supposed to be contained in either case; whilst D is the density of the gas when of the normal pressure, the height of which is designated by H , it being understood that both D and H are expressed in units referring to the same substance (for instance, mercury, whose density Q is to be assumed = 1), as also H to mean inches or centimetres, according to the nature of the units in which the a 's are expressed. According to this formula, if the law of Boyle were accurate, we should have

$$\frac{p_0 a_0}{p_1 a_1} = \frac{1 - \frac{1}{2} \frac{D}{H} a_1 + \&c.}{1 - \frac{1}{2} \frac{D}{H} a_0 + \&c.} = k,$$

and not, as supposed by M. Regnault,

$$\frac{p_0 a_0}{p_1 a_1} = 1.$$

Hence also, if it be ascertained that the above relation is not always practically verified,

$$\frac{p_0 a_0}{p_1 a_1} - k = \alpha,$$

and not

$$\frac{p_0 a_0}{p_1 a_1} - 1 = \beta$$

as stated by M. Regnault, will be the true measure of deviations from the law of gaseous compressibility as displayed in the Boylean formula; whilst

$$\beta - \alpha = k - 1$$

will measure the error committed by substituting one expression for the other from misapprehension. But since $\frac{D}{H}$ is a very small magnitude, k may be written simply

$$k = 1 + \frac{1}{2} \frac{D}{H} (a_0 - a_1); \quad \dots \quad (\text{II.})$$

which in the case of M. Regnault's experiments, for all of which

$$a_0 = 3 \text{ metres, and } a_1 = 1.5 \text{ metre,}$$

becomes, by specializing,

$$k_r = 1 + \frac{1}{2} \frac{D}{H} a_1.$$

So that, finally, we arrive at the results presented in the following Table:—

Gas.	$k_r - 1.$
Atmospheric air.....	0.000094
Nitrogen.....	0.000091
Carbonic acid.....	0.000144
Hydrogen	0.000007

It appears also, on inspection of formula (I.), that there does not exist, as implicitly assumed by M. Regnault in choosing the variables of his empirical formulæ, any generally valid relation between the proportion of pressures and the proportion of volumes in gases altogether independent of the absolute magnitudes of the volumes. For, even if the law of Boyle held good, and adopting for k its simplest expression, contained in formula (II.), the above relation would still be dependent also on the difference of volumes; so that to the same ratio of pressures different ratios of volumes might correspond, according to the values of the differences of the latter. But the bearing of this last observation, as well as the influence of the $(k_r - 1)$ on the final results of M. Regnault's calculations, could only be fully explained after premising some other statements, which are now to follow. It will be well, however, to state at once that it is not intended to resume the above subject, as what is to be brought forward in the very next sentences is of so much greater numerical importance, that it would appear superfluous, on comparison, to bestow any more lines on the present matter.

2. The particular method adopted by M. Regnault in the investigation here under review has been incidentally spoken of already in the preceding article; it consisted simply in this—a compressing of different masses of gases of like initial volume, but varying density, to the same amount $= \frac{1}{2}$ the primitive volume, noting at the same time the corresponding pressures. But since it was the aim of these researches, the insufficiency of the Boylean law being once made patent, to determine empirically some other *general* formula by means of which the pressure could be calculated on knowing the density, or conversely*, the following subsidiary processes were resorted to for the purpose. The known values of β

* At least in cases where the temperature is not very different from that at which the actual measurements were made.

in a certain number of cases in which they had been experimentally determined, allowed of the construction of a continuous curve having the initial pressure p_0 for general abscissa, and β for ordinate; which curve might hence be considered as a graphical representation of the function β of p_0 in the instance of

$\frac{a_0}{a_1} = 2$, according to the general meaning of β , and the particular values of a_0 and a_1 adopted by M. Regnault. By the aid of these curves (to each gas belonging, of course, a distinct one) four values of β were determined for each gas, as indicated below—where, for brevity's sake, $(\alpha + 1)$ has been made $= m$; viz.

$$\frac{v_0 p_0'}{v_1 p_1'} = m', \quad \frac{v_0 p_0''}{v_1 p_1''} = m'', \quad \frac{v_0 p_0'''}{v_1 p_1'''} = m''', \quad \text{and} \quad \frac{v_0 p_0^{iv}}{v_1 p_1^{iv}} = m^{iv}, \quad (A)$$

corresponding to the following values of the abscissæ p_0 ,

$$p_0' = P_0 = (1^m \text{Hg}), \quad p_0'' = p_1', \quad p_0''' = p_1'', \quad \text{and} \quad p_0^{iv} = p_1'''. \quad (B)$$

Forming now the products*

$$n' = m', \quad n'' = m'm'', \quad n''' = m'm''m''', \quad \text{and} \quad n^{iv} = m'm''m'''m^{iv},$$

we obtain four special values of the general function $\frac{V_0 P_0}{V_1 P_1}$

analogous to $\frac{v_0 p_0}{v_1 p_1}$, but supposing at present P_0 (instead of $\frac{v_0}{v_1}$) to

be constant, and $\frac{V_0}{V_1}$ (instead of p_0) to be variable. In the case specified, P_0 was made $= 1$ metre mercury, and $\frac{V_0}{V_1}$ successively

$$\frac{1}{2}, \quad \frac{1}{4}, \quad \frac{1}{8}, \quad \frac{1}{16};$$

so that the above $\frac{V_0 P_0}{V_1 P_1}$'s, or n 's, gave the values of the ratio of pres-

ures when the compression or ratio of mean densities (for which, however, the proper bottom-densities should be substituted) varies from 1 to 2, 4, 8, 16. Any two or more such values n might hence be used to determine the two or more constants of any such empirical formula, showing the pressure in the form of function of the density, or conversely, which has before been designated as the end of all these various operations; which last calculations M. Regnault of course actually accomplished.

It is evident, however, that for the n 's to have really the meaning assigned to them above, it is necessary that the equations (B) be strictly attended to. Now it was found by M. Regnault in the first instance, and is mentioned by him as the primary result of his researches in question, that the law of Boyle is *not* accurate. This, applied to the case in hand, means

* It is easy and instructive to translate the meaning of this arithmetical operation into the language of experiment.

that (since $\frac{a_0}{a_1}=2$) the following equations, referring to the p_0 's in equations (A), will *not* hold good, viz.

$$p_1' = 2p_0, \quad p_1'' = 2p_0'', \quad p_1''' = 2p_0''', \quad \text{and} \quad p_1^{iv} = 2p_0^{iv};$$

consequently the following will be equally false,

$$p_0'' = 2p_0', \quad p_0''' = 2p_0'', \quad \text{and} \quad p_0^{iv} = 2p_0''';$$

and putting now for p_0' its value P_0 , these last will be false *à fortiori*:

$$p_0'' = 2P_0, \quad p_0''' = 4P_0, \quad \text{and} \quad p_0^{iv} = 8P_0.$$

Hence it would be erroneous to use in the above calculations, as M. Regnault has done, values of m having respectively for abscissæ,

$$1^m, \quad 2^m, \quad 4^m, \quad \text{and} \quad 8^m;$$

but such values ought to be chosen as the equations (B) necessitate.

The values of the proper m 's, and consequently their numerical variation from those actually employed, may be readily computed in the following manner. The value of m' , as to which there is no difference, being known in numbers, as well as that of $p_0' = 1$, $p_0'' = p_1'$ may be calculated. Corresponding to p_0'' , considered as an abscissa, the curves will furnish a new ordinate, which is the true m'' ; this, in its turn, will give $p_0''' = p_1''$: hence m''' ; and so on. By this means it is easy to ascertain that in the case of CO_2 gas, for instance, m^{iv} as assumed and its accurate value differ by more than 0.006; and since in the values n , which are formed by multiplication of the several m 's, the errors of the latter become successively summed (or nearly so), it is evident that n^{iv} , for example, which is one of the numbers actually employed in computing the constants so often already mentioned, will be considerably more erroneous than even m^{iv} . It is evident that in the case of CO_2 gas, which is more compressible than according to the Boylean law should happen, the compressibility has been actually *exaggerated* in consequence of the above misunderstanding. But it is fair to add that the example quoted shows this exaggeration far greater than would the other three gases; in regard of one of which, viz. H, the error committed amounts even to a diminution of apparent compressibleness, though still an augmentation of deviation from law.

3. In the Appendix will be found briefly indicated a simple but rigorous mode of evaluating the constants of any preferred empirical formula, by which to render more accurate, or rather to supersede provisionally, the Boylean law, with the sole aid of the experimental data respecting this matter furnished by M.

Regnault's investigation, and obviating any such intermediate deductions as are described above.

Before entering, however, on the Appendix, it is necessary to speak yet of a series of apparently casual errors which have crept into M. Regnault's calculations,—those mentioned above being, if we err not, of the class generally designated as systematic. The occurrence of such casual errors was first noticed by Prof. Neumann of Königsberg (who kindly directed my attention to the matter) in the case of two of the values m belonging to atmospheric air obtained in the manner stated in the previous article, by observing that they varied more from the actually tabulated similar values of nearly coincident argument than the difference of arguments might seem to warrant. On reference to the curves on which the m 's were professedly measured, the discrepancy, and consequent presumption of mistake, was found confirmed; and on further extending the examination to the several m 's belonging to other gases, and their respective curves, a number of additional disagreements, similar to those first remarked in the instance of atmospheric air, were detected. The greatest of these amounts numerically to 0.001 exactly, belonging to CO_2 gas; but whilst here the divergence is easy to explain, as owing to a simple oversight in counting the divisions on the scale of ordinates (each of which is = 0.001), as regards the minor errors no account can apparently be given.

A last series of errors (seemingly in writing) is met with in pp. 411, 418, 419, and 424 of M. Regnault's memoir, to which, however, this is not the place to furnish corrections. Similarly it will be sufficient merely to advert to a singular statement in p. 416, referring to the value of β or $(m-1)$ in the case of infinitesimal values of the argument p_0 , which clearly originates in some misapprehension.

APPENDIX.

4. The formula serving to express the accurate relation between finite volumes and ordinary pressures of gases, in the case specified in art. 1, is due to Prof. Neumann, by whom it was deduced after the following manner, as communicated to the writer. The mass M of a column of gas of the height a and base = 1^{\square} , is given by the integral

$$M = \int_0^a \rho dx, \quad (i)$$

where ρ denotes the density of the layer whose distance from the top of the column, supposed to be a cylinder or prism, is x ; and

according to Boyle's law,

$$\rho = \frac{R}{P} \left\{ p_a - g \int_x^a \rho dx \right\},$$

R being the density corresponding to the normal pressure P; p_a the pressure at the bottom of the column to which the layer of the density ρ belongs; finally, g = the acceleration of gravity. Differentiating this last expression, and integrating the result between the limits a and x , we shall find

$$\frac{\rho}{\rho_a} = e^{\frac{R}{P}(x-a)};$$

or observing that

$$P = gQH, \text{ and making } \frac{R}{Q} = D :$$

where Q denotes the density of the matter, and H the height of the column, in which the normal pressure of the gas is to be evaluated, whilst D is introduced only for brevity: it will result that

$$\rho = \rho_a e^{\frac{D}{H}(x-a)}.$$

Employing this last expression in the integral (1), we get

$$M = \frac{H}{D} \rho_a \{ 1 - e^{-\frac{D}{H}a} \} = \frac{p_a}{g} \{ 1 - e^{-\frac{D}{H}a} \},$$

since $\frac{H}{D} \rho_a = \frac{P}{Rg} \rho_a = \frac{p_a}{g}$.

Let a assume now in two special cases the values a_0 and a_1 , and the corresponding pressures be simply designated as p_0, p_1 ; we shall then have, since the mass is supposed the same, the equation

$$Mg = p_0 \{ 1 - e^{-\frac{D}{H}a_0} \} = p_1 \{ 1 - e^{-\frac{D}{H}a_1} \},$$

from which at once follows the relation

$$\frac{p_0}{p_1} = \frac{1 - e^{-\frac{D}{H}a_1}}{1 - e^{-\frac{D}{H}a_0}}, \dots \dots \dots (I.)$$

identical with that first quoted in art. 1.

5. In establishing the above formula, it was necessary to employ the law of Boyle. As the latter, however, is not quite accurate, it shall now be shown how a formula similar to (I.), but not implying the truthfulness of the Boylean law, may be deduced, and ultimately serve to accomplish the purpose adverted to in the beginning of art. 3. To arrive at this new formula, we

start from the general relation

$$\rho = \phi \cdot \frac{R}{P} \left\{ v_a - g \int_x^a \rho dx \right\},$$

which is similar to that expressed by the Boylean law, except that it involves an additional magnitude ϕ , by which that law may be corrected to any degree, on considering ϕ as a function, be it of ρ or the corresponding pressure p , to be determined, as to form, ultimately by the aid of experimental or other data. Differentiating the last expression will furnish

$$\frac{d\rho}{\rho} = \frac{d\phi}{\phi} + \phi \cdot \frac{D}{H} dx,$$

after some simplification, and on introducing again D and H in the sense of the preceding article. Integrating again between the same limits as before, we shall find

$$\log \frac{\rho}{\rho_a} = \log \frac{\phi}{\phi_a} + \frac{D}{H} \int_a^x \phi dx;$$

but since the above integral may be written

$$-\phi_\mu \int_x^a dx \text{ instead of } \int_a^x \phi dx \text{ or } - \int_x^a \phi dx :$$

to warrant which it is only necessary to give ϕ_μ a proper value in accordance with the inequalities below,

$$\phi_a > \phi_\mu > \phi_x :$$

we get at last

$$\rho = \frac{\rho_a}{\phi_a} \phi e^{\phi_\mu \frac{D}{H} (x-a)},$$

and, on substituting this value of ρ in the expression for the mass,

$$M = \int_0^a \rho dx = \frac{\rho_a}{\phi_a} \int_0^a \phi e^{\phi_\mu \frac{D}{H} (x-a)} dx.$$

To perform the integration above only indicated, it is necessary to introduce two new ϕ 's, similar in meaning to the original ϕ_μ , but now in accordance with the following inequalities,

$$\phi_{\hat{a}} > \phi_{(m)} > \phi_0 \text{ and } \phi_{\mu, a} > \phi_{(\mu)} > \phi_{\mu, 0},$$

by which means M may be written

$$M = \frac{\phi_{(m)}}{\phi_{(\mu)}} \cdot \frac{H}{D} \cdot \frac{\rho_a}{\phi_a} \left\{ 1 - e^{-\phi_{(\mu)} \frac{D}{H} a} \right\};$$

or also

$$M = \frac{\phi_{(m)}}{\phi_{(\mu)}} \cdot \frac{\rho_a}{g} \left\{ 1 - e^{-\phi_{\mu, a} \frac{D}{H} a} \right\},$$

since

$$\frac{H}{D} \rho_a = \phi_a \frac{p_a}{g}.$$

Employing now again two different values of a , to be denoted by a_0 and a_1 , and discriminating properly, moreover, between the various p 's and ϕ 's, we obtain the equation

$$Mg = \frac{\phi_{(m)}^0}{\phi_{(\mu)}^0} \cdot p_0 \{1 - e^{-\phi_{(\mu)}^0 \frac{D}{H} a_0}\} = \frac{\phi_{(m)}^1}{\phi_{(\mu)}^1} p_1 \{1 - e^{-\phi_{(\mu)}^1 \frac{D}{H} a_1}\},$$

from which follows

$$\frac{p_0}{p_1} = \frac{\phi_{(m)}^0 \cdot \phi_{(\mu)}^1}{\phi_{(m)}^1 \cdot \phi_{(\mu)}^0} \cdot \frac{1 - e^{-\phi_{(\mu)}^1 \frac{D}{H} a_1}}{1 - e^{-\phi_{(\mu)}^0 \frac{D}{H} a_0}}.$$

To simplify this formula, it may be observed, in the first place, that, according to their signification, the ratios $\frac{\phi_{(m)}^0}{\phi_{(\mu)}^0}$ and $\frac{\phi_{(m)}^1}{\phi_{(\mu)}^1}$ cannot but be each extremely near in value to 1; and being both at the same time either > 1 or < 1 , the fraction of which one of these ratios is the numerator and the other appears as the denominator, which occurs in the above formula, may be safely replaced by 1. Similarly $\phi_{(\mu)}^1$ and $\phi_{(\mu)}^0$, which, like all the ϕ 's, are but little different from unity each, and appear multiplied in the above exponentials by the very small quantity $\frac{D}{H}$, may be exchanged for two other values ϕ_1 and ϕ_0 , to be considered as similar in meaning to the simple ϕ_a of former formulæ; from which definition it is easy to conclude that $\phi_{(\mu)}^1$ and ϕ_1 , and $\phi_{(\mu)}^0$ and ϕ_0 are of necessity very nearly identical in numerical value. In this manner we find at last

$$\frac{p_0}{p_1} = \frac{1 - e^{-\phi_1 \frac{D}{H} a_1}}{1 - e^{-\phi_0 \frac{D}{H} a_0}};$$

from which, after developing and on retaining but first powers of $\frac{D}{H}$, which will be sufficient, we deduce

$$\frac{\phi_0}{\phi_1} \cdot \frac{p_0 a_0}{p_1 a_1} = 1 + \frac{1}{2} \frac{D}{H} \{\phi_0 a_0 - \phi_1 a_1\}; \dots \quad (a)$$

while for $\frac{\rho_0}{\rho_1}$, which is the ratio of pressures corresponding to the ratio of densities $\frac{p_0}{p_1}$, will be got the simpler expression

$$\frac{\rho_0}{\rho_1} = \left\{ 1 + \frac{1}{2} \frac{D}{H} \{\phi_0 a_0 - \phi_1 a_1\} \right\} \frac{a_1}{a_0} \dots \quad (b)$$

By means of formula (a) it is now possible to employ the ex-

perimental data of corresponding p 's and a 's which have been supplied by M. Regnault, *immediately* and in a legitimate manner, for the purpose of determining the constants of any empirical formula, to be substituted for the general ϕ , by which we may wish to supplement the Boylean law. With this object ϕ should be expressed as a function of p , as it would be improper to make it a function of a , or the mean density, and the density strictly corresponding to p is not ascertained*. It would be superfluous to insist here on what would be the best way to avail oneself of the *whole* body of observations tabulated by M. Regnault, as it will become obvious of itself to everyone on inspection of the Tables. Before concluding, however, we may be allowed yet to make this simple observation, that for formula (a) may be advantageously substituted the simpler form

$$\begin{aligned}\phi_0 \cdot \frac{p_0 a_0}{p_1 a_1} &= \phi_1 + \frac{1}{2} \frac{D}{H} \{a_0 - a_1\} \\ &= \phi_1 + k - 1\end{aligned}$$

(k having the same meaning as in art. 1), which, on account of the small value of $\frac{D}{H}$, is sufficiently exact for practical purposes; at least if we are satisfied with ascertaining the numerical values of ϕ to about $\frac{\pm 0.0001}{2}$, beyond which order the concordance of the experimental data to be employed does not promise reliable results.

P.S. Since the above paper was written, a second volume of M. Regnault's *Relation*, &c.† has come to hand, in which some fresh experiments on gaseous compressibility are mentioned which call for a short supplemental remark. It should be stated, in the first place, that these new measurements are not intended to supersede the older, but were only made to meet an incidental want in some other research; for which purpose a much narrower sweep of pressures and a wider range of substances was in respect of the former sufficient, and in respect of the latter a necessity. The investigation in this case was carried on after another method, and with rather different apparatus, than in the former; as a consequence of which first departure from precedent (or rather return to the precedent as set by Dulong),

* It would be easy, however, to evaluate a formula $\phi = f(p)$ by means of data to be derived from the first established form of $\phi = F(p)$ and of formula (b).

† Recently published as vol. xxvi. of the *Mémoires de l'Académie des Sciences*. In an Errata appended to this volume the errors in pp. 418, 419 of vol. xxi., mentioned in art. 3, have been corrected.

only the first of the remarks of this paper, viz. that of art. 1, finds here application,—and this also apparently with somewhat diminished arithmetical import, owing to the change instanced above as second. It might be stated, on the other hand, as noteworthy, both in this last and the former research, but more especially the first-mentioned, that no more equations were employed for determining the constants of the empirical formulæ, by which the result of the whole investigation ought to be embodied, than what are strictly necessary for the purpose; so that in the latest example, of from eight to eleven measurements separately performed for each gas, only three on the whole do any practical duty in each case.

Oxford, February 1863.

XLI. Notices respecting New Books.

On Matter and Æther, or the Secret Laws of Physical Change. By the Rev. THOMAS RAWSON BIRKS, M.A. Cambridge: MacMillan & Co., 1862.

IT might be questioned whether this work ought to receive any notice in a scientific Journal, the character and contents of it being such that it can hardly be permitted to take rank among scientific productions. It is, in fact, singularly deficient in one particular, which has hitherto been considered essential in theoretical physics, namely, the development of the results of assumed principles by means of mathematical reasoning. As, however, advantage may be derived from the scrutiny of a false or defective way of philosophizing, we have thought it worth while to bring the work under review, especially as the too confident tone in which it is written may gain for it, among readers not qualified to judge of its contents scientifically, an influence which may need to be counteracted. It is certainly a very readable book, and embraces a great variety of scientific facts, for the most part clearly and correctly stated; for which reasons there is the more necessity for pointing out its radical faults.

In page 10 there is a quotation from Newton's 'Principia,' introduced with this remark:—"The closing words of the 'Principia' are like a prophecy, and show in what direction the second main series of physical discovery must be attained." We quite agree with this estimate of the passage referred to, which, together with one quoted in the next page from the 'Optics,' may well be regarded as wonderful evidence of Newton's genius and foresight. To make intelligible the subsequent criticisms it will be proper to adduce both these passages. The one from the 'Principia' is as follows:—"I might add something about a certain very subtle spirit which pervades dense bodies and lies hid in them; by the force and agency (*vi et actionibus*) of which, bodies at very small distances attract each other, and when brought close together, cohere; and electrical bodies act at greater distances, both repelling and attracting neighbouring bodies; and light is

emitted, reflected, refracted, inflected, and warms bodies; and all sensation is excited, and the limbs of animals are moved at will, that is, by vibrations of this spirit propagated through the solid fibres of the nerves from the external organs of sense to the brain, and from the brain to the muscles. But these things cannot be expounded in few words; neither does there exist a sufficient abundance of experiments, by which the laws of the action of this spirit can be accurately determined and demonstrated." The following is the quotation from the 'Optics':—"Is not heat conveyed through a vacuum by the vibrations of a much more subtile medium than air? Is not this medium the same by which light is refracted and reflected, and communicates heat to bodies, and is put into fits of easy reflexion and transmission? Do not hot bodies communicate their heat to cold ones by the vibrations of this medium? And is it not exceedingly more rare and subtile than air, and exceedingly more elastic and active? And does it not readily pervade all bodies? And is it not, by its elastic force, expanded through all the heavens?" Two inferences may clearly be drawn from these views:—first, that according to Newton the forces of nature are resident in a very subtile spirit, or medium, which pervades all other bodies, and is extremely elastic; that they are, in fact, modes of action of this medium, one of which he particularly specifies, namely, action by vibrations: secondly, that he ascribes no active power, or virtue, to bodies which are distinct from the elastic medium. In another part of the third book (Regula III.), he says expressly, "I by no means affirm that gravity is essential to bodies." Also there is no evidence in the above passages, or elsewhere in Newton's writings, that he took account of an atomic constitution of the medium, the very term "elastic," as used in his sense, implying that he regarded it as continuous, and acting by *pressure* just as the air does, only in much greater degree. Now let us turn to Mr. Birks's views. At the same time that he adduces Newton's "prophecy," he appears to do all he can to falsify it. The general view that he takes of matter is that "it consists of monads, or moveable centres of force, unextended, but definite in position." Such monads, which in other places he calls "atoms" and "particles," without expressly ascribing to them extension or inertia, he considers to be the constituents both of the æther, and of visible and tangible bodies. Thus he opposes Newton in two most important particulars: first, in making *active* force an essential quality of the constituents of visible bodies, whereas Newton in Regula III. allows of no other essential force (*vim insitam*) in such bodies than the *vis inertiae*; and again, in making it an essential condition of his physical theory to regard the æther as consisting of monads.

Again, there is a passage in the Regula III., as remarkable as the two before quoted, which also contains views directly opposed to those of Mr. Birks. Newton says, "The extension, hardness, impenetrability, mobility, and *vis inertiae* of the whole arise from the very same qualities of the parts; and thence we conclude that all the least parts of all bodies are extended, hard, impenetrable, moveable, and endowed with *vis inertiae*. And this is the foundation of all

philosophy." Against this dictum the author argues as follows (p. 27): "Our first impressions of matter are derived from solids, and their resistance to touch, or pressure of the hand. Hence the nature of matter has often been defined by extension and solidity. But the progress of science has shown the error of such a definition. The sensation of solidity is evidently compound, and arises from a repulsive force exerted along a well-defined surface. This repulsion begins before actual contact, and appears to be a rapidly decreasing power, which emanates from the outer particles of the resisting substance. When it is melted or vaporized, the substance remains, but its law of force is altered, and the sensation of solidity disappears. Again, the impenetrability of matter, in the popular sense, is disproved by the facts of chemical combination." The answer to all this is, that the arguments are directed against non-essentials in Newton's definition of the "least parts of bodies." The point of primary importance is that, as all experience shows, we have no perception by the senses of body, and, therefore, no conception of body, apart from *extension*. On this point metaphysics and physics, Locke and Newton, agree. But if the ultimate parts of bodies have extension, they must be solids (in the geometrical sense), and if they are solids, they must have *form*. Here, however, so much as this may be conceded, that the terms "hardness" and "impenetrability" are open to the objection made by Mr. Birks that, as these qualities are understood by experience, they admit of *degrees*. But this objection is met, and at the same time the definition in all that is essential is retained, if we say that an ultimate particle which has form, has a *constant* form. This hypothetical property of an atom is to be adopted on the principle, laid down by Mr. Birks himself in his Axiom II. (p. 4), that it is the "simplest" conceivable. The variability of form exhibited by masses is shown by the progress of science to be due to their being composed of discrete atoms, and it would therefore be introducing a needless complexity to attribute the same variability to the atoms.

In short the grand and leading principle of the Newtonian Philosophy is to deduce by means of mathematics from the fundamental ideas of form, inertia, and pressure, results by which observed phenomena and laws may be explained, the data for the solution of the particular problems being also furnished by experimental observation. If then the philosophy which Newton taught and endeavoured to hand down to succeeding times be true, and be truly represented above, works such as that before us must contain a kind of scientific heresy. For not only have we here direct antagonism in defining a monad, or atom, to be a centre of force irrespective of the quality of extension, but this first departure from the simplicity of Newton's fundamental ideas necessitates other complexities, and gives rise to the conceptions of axes, rotations, polarities, and centrifugal force of atoms, which play a large part in the author's system, and to which effects are ascribed that seem incapable of illustration by reference to commonly observed phenomena. On the contrary, the Newtonian "foundation of all philosophy" involves only ideas that

are perfectly comprehensible by the commonest experience, and it is this very circumstance that gives reality to the scientific erection that may be reared upon it.

But it is not because Mr. Birks's views are opposed to Newton's, and cannot, therefore, be considered as making any advance in the direction which Newton indicated, that we hesitate to call his work scientific. Other writers of the present day have equally disregarded Newton's authority; for, in fact, in matters of science there is no such thing as absolute authority. It may therefore be conceded to Mr. Birks that he is able by a kind of intuition to perceive that each monad of matter forms an indissoluble union with a monad of æther. But when from this beginning he goes on to account for a great variety of physical facts, which he appears to do with much facility and confidence, ordinary individuals will ask to be conducted to the conclusions step by step. The intuition which may serve him will not serve them. There is a recognized method by which a philosopher whose perception of physical causes is in advance of that of his contemporaries can compel assent to his views, and make them common property,—the method of reasoning by symbols, and by the formation and solution of simple and differential equations. Newton's principles of Natural Philosophy were mathematical principles, and his mathematics of the highest order for the time. Nothing short of the Intelligence which framed the Universe can dispense with such means of ascertaining the causes of phenomena. It is scarcely credible that a mathematician in these days should have written a book which pretends to give explanations in the whole range of physical science, without introducing a single differential equation. This is showing a disregard of methods that have been generally thought to be indispensable, which on no ground can be justified. We suspect that if Mr. Birks attempted to transfer his thoughts into symbolic language for the purpose of tracing consequences by the aid of mathematical reasoning, he would begin to see the magnitude and difficulty of the task he has undertaken. This radical defect in the work, which excludes the possibility of deciding whether or not the explanations it offers are purely imaginary, obliges us to pronounce it to be unscientific.

In order to show that these strictures are not made without good reason, it may suffice to adduce one specimen of the author's mode of accounting for natural phenomena. To explain the chemical production of heat he writes (p. 109) as follows:—

“The present hypothesis, I believe, provides a full and complete explanation. Heat is simply atomic or molecular *vis viva*. Sensible heat depends on the oscillations of the solid atoms, transferred through the repulsion of their constituent or adjacent æther to neighbouring atoms, and producing vibrations which can radiate freely through air or *in vacuo*, like the waves of light. Heat of fluidity consists in the *vis viva* of each atom in revolving on its own axis of greatest moment, whereby the polarity of neighbouring atoms is weakened or destroyed. Heat of vaporization consists in the *vis viva* spent or absorbed in removing the chemical atoms to a greater mean distance beyond the limit of maximum cohesive power, so that the centrifugal force is

balanced only by some external pressure. But besides these varieties of heat, depending on the solid, fluid, or gaseous structure of the whole body, there will be another variety, an atomic heat, depending on the essential structure of the compound atoms themselves. It will be least where their monads are most compact, and greatest when they are at greater intervals from each other."

Surely these numerous assertions can only be regarded as expressions of personal conceptions, the correspondence of which to physical realities is proved neither by immediate explanations of phenomena, nor by explanations deduced by mathematical reasoning. There is, for instance, a wide gap which requires filling up, between asserting that each atom revolves about its axis of greatest moment and by its *vis viva* weakens or destroys the polarity of neighbouring atoms, and explaining how by this action fluidity results. Besides, if the views in the above passage corresponded to realities, the realities would just as much require explanation as the fact of chemical combustion which it was proposed to explain. Nothing therefore is gained by multiplying such conceptions. Science advances in proportion as the explanations of phenomena are reduced to few and definite ideas derived immediately from experiment and observation. Thus Newton added greatly to our knowledge by proving mathematically that the force which produces the motions of planets and satellites, whatever intrinsically it may be, is not different in kind from that which causes a stone to fall to the ground.

Although we have thought it right to express our dissent from Mr. Birks's theoretical principles, we are ready to admit that his work is not altogether unworthy of a mathematician who gained high distinction in the University of Cambridge. Much care and labour have evidently been bestowed on its composition, and it shows large and accurate acquaintance with the existing state of physical science. Excepting where he is attributing influences to his wonder-working "dual atom," his theoretical views are generally sensible and discriminating. They are not, however, always new. Some of the explanations, for instance, contained in the articles in Chap. V., on the relations between Light and Sound, and on Musical Tones and Colours compared, will be found in the Report of the British Association for 1834, and in vol. xii. (S. 4) of this Magazine.

XLII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 233.]

June 19, 1862.—Major-General Sabine, President, in the Chair.

THE following communications were read :—

"On the Photographic Transparency of various Bodies, and on the Photographic Effects of Metallic and other Spectra obtained by means of the Electric Spark." By Prof. W. Allen Miller, M.D., LL.D., V.P. and Treas. R.S.

In this paper the author pursues an inquiry the commencement

of which was communicated to the Chemical Section of the British Association last year. Owing to the employment of a prism of bisulphide of carbon, he was then led to believe that the photographic effects of the electric spectra produced by the different metals were in a great degree similar, if not identical. Subsequent investigations have, however, shown him that the absorbent effects of the bisulphide upon the chemical rays are so great, that the conclusions then drawn from observations made by this refracting medium require very considerable modification. Notwithstanding the great length of the chemical spectra obtained by the aid of the bisulphide, not more than *one-sixth* or *one-seventh* of the true extent of the spectrum produced by the electric spark between various metals is procured, as may be shown by comparing the spectrum with one of the same metal furnished by the use of a lens and prism of rock-crystal.

Rock-crystal, however, possesses but a comparatively small refractive and dispersive power, whilst it almost always affords some trace of double refraction in one portion or other of the spectrum procured by its means.

In searching for some singly refracting medium which should possess sufficient refractive and dispersive power to enable it to be used advantageously in the construction of lenses and prisms suitable for this inquiry, the author was led to examine the photographic absorption of a variety of colourless substances which appeared perfectly transparent to the luminous rays. The experiments detailed in the first portion of the present paper refer to this absorbent action of various media upon the chemical rays of the spectrum; whilst the second portion of the paper is devoted to a description of the electric spectra of some of the more important elementary bodies, and the effect of varying the gaseous media in which the sparks producing these spectra are made to originate.

1. *The Photographic Transparency of Bodies.*—In the experiments upon the absorbent action of the different media, the source of light employed was the electric spark obtained between two metallic wires (generally of fine silver), connected with the terminals of the secondary wires of a ten-inch induction-coil. The light, after passing through a narrow vertical slit, either before or after traversing a stratum of the material the chemical transparency or *diactinic* quality of which was to be tested, was allowed to fall upon a quartz prism placed at the angle of minimum deviation for the mean of the refracted rays. Immediately behind this was a lens of rock-crystal, and behind this, at a suitable distance, the spectrum was received upon a collodion-film coated with iodide of silver; this was supported in the frame of a camera, and after an exposure, generally lasting for five minutes, the image was developed by means of pyrogallic acid, and fixed with cyanide of potassium.

The general results of these experiments were as follows:—

1. Colourless bodies which are equally transparent to the visible rays, vary greatly in permeability to the chemical rays.

2. Bodies which are photographically transparent in the solid form, preserve their transparency in the liquid and in the gaseous states.

3. Colourless transparent solids which exert a considerable photographic absorption, preserve their absorptive action with greater or less intensity both in the liquid and in the gaseous states.

Whether the compound is liquefied by heat or dissolved in water, these conclusions respecting liquids are equally true. The perfect permeability of water to the chemical rays, conjoined with the circumstance that in no instance does the process of solution seem to interfere with the special action upon the incident rays of the substance dissolved, renders it practicable to submit to this test a great number of bodies which it would otherwise be impossible to subject to this species of experiment on account of the extreme difficulty of obtaining them in crystals of sufficient size and limpidity.

Glass vessels cannot be employed to contain the liquids during the trial. Flint-glass, crown, hard white Bohemian, plate-glass, window-sheet, and Faraday's optical glass, all, even in thin layers, shorten the spectrum by from three-fifths to four-fifths or even more of its length. Mica produces a similar effect. Indeed, the only substance which the author found could be employed with advantage is rock-crystal cut into thin slices and polished. The value of this material in researches upon the more refrangible end of the spectrum was pointed out by Prof. Stokes and M. E. Becquerel several years ago. In order to hold the liquids for experiment, a small trough was prepared by cutting a notch in a thick plate of plate-glass, the sides being completed by means of thin plates of quartz, which were pressed against the ground surfaces of the plate-glass by the aid of elastic bands of caoutchouc; a stratum of liquid of 0.75 inch in depth was thus obtained for each experiment.

The substances which, after atmospheric air and certain other gases, are most perfectly diactinic, are rock-crystal, ice, as well as pure water, and white fluor-spar. Rock-salt is scarcely inferior to them, if at all. Then follow various sulphates, including those of baryta, and the hydrated sulphates of lime and magnesia, as well as those of the alkalies. The carbonates of the alkalies and alkaline earths, as also the phosphates, arseniates, and borates, are likewise tolerably transparent, though saturated solutions of phosphoric and arsenic acids exerted considerable absorbent power; so also did those of the alkalies, potash, and soda, possibly from the presence of a trace of some foreign colouring matter, as those liquids had an extremely faint greenish tinge.

The soluble fluorides, as well as the chlorides and bromides of the metals of the alkalies and alkaline earths, are freely diactinic, but the iodides are much less so, and exhibit certain peculiarities. All the organic acids and their salts which were tried by the author exerted a marked absorptive action upon the more refrangible rays. Amongst those subjected to experiment were the oxalates, tartrates, acetates, and citrates, those mentioned first in order having the greatest absorptive action. It is, however, much more difficult to obtain organic compounds in a state of purity sufficient to furnish trustworthy results, than is the case with the salts of the inorganic acids. The author, therefore, expresses himself with more reserve upon some of

these organic bodies, particularly the acetates, than in other cases. The different varieties of sugar are freely diactinic.

Amongst the salts of inorganic acids, the nitrates are the most remarkable for their power of arresting the chemical rays. A solution of each of these salts, in all the instances tried, cut off all the more refrangible rays, and reduced the spectrum to less than a sixth of its ordinary length. The chlorates, however, do not participate in this absorptive power to nearly the same extent.

Although the sulphates, as a class, are largely diactinic, the sulphites are much less so; and the hyposulphites cut off about three-fourths of the length of the spectrum, leaving only the less refrangible portion.

Of eighteen different liquids tried by the author, two only can be regarded as tolerably diactinic, viz. water, which is eminently so, and absolute alcohol, which, however, exhibits a considerable falling off. The liquids which follow are mentioned in the order of their chemical transparency, those most transparent being mentioned first:—Dutch liquid, chloroform, ether; then benzol and distilled glycerin, which differ but little; then fousel oil, wood-spirit, and oxalic ether, which are also nearly alike; acetic acid, oil of turpentine, glycol, carboic acid, liquid paraffin, boiling at 360° F., and bisulphide of carbon. Finally, terchloride and oxychloride of phosphorus, although perfectly colourless and limpid, arrest all the chemical rays.

The experiments upon aëriform bodies yielded important results; they show but little coincidence with those of Tyndall on the absorptive power of the gases for radiant heat. These experiments were made by interposing in the track of the ray between the vertical slit and the quartz prism, a brass tube two feet long, closed at each end air-tight by means of a plate of quartz. Each gas or vapour in succession was introduced into the tube, and the results compared with those produced by causing the rays to traverse the tube when filled with atmospheric air.

Amongst the colourless gases, oxygen, hydrogen, nitrogen, carbonic acid, and carbonic oxide exhibit no absorptive power.

Olefiant gas, protoxide of nitrogen, cyanogen, and hydrochloric acid exert a slight but perceptible absorbent effect. But in the case of coal-gas the absorptive action is extremely marked, the more refrangible half of the spectrum being cut off by it abruptly. The absorption exerted by sulphurous acid is still more powerful and as sharply defined; sulphuretted hydrogen and the vapour of bisulphide of carbon exhibit a still more decided absorbent action; the effect of the terchloride and oxychloride of phosphorus is not less marked. This absorbent action of these different compounds of sulphur and phosphorus is very striking.

Coal-gas appears to owe its remarkable power of arresting the chemical rays to the presence of the vapour of benzol and other heavy hydrocarbons; since the vapour of benzol at 65°, diffused to saturation through a column of atmospheric air two feet long, exerts a still more powerful absorptive effect than coal-gas.

On the other hand, the effect of a similar arrangement, in which

the vapour of ether, of chloroform, and of oil of turpentine was substituted for that of benzol, gave effects which, though perceptible, were much less marked. An arbitrary scale is laid down, by which a comparative estimate of the absorptive power of each compound, whether solid, liquid, or gaseous, may be effected with tolerable accuracy.

With a view of facilitating the production of a spectrum on a flat field, at a uniform distance at all points from the prism, the author instituted a series of experiments, in which a small metallic speculum was substituted for the lens of rock-crystal; but the loss of chemical power in the reflected rays was so considerable, and this loss occurred so unequally at different points, that the method was abandoned. The results of the photographic action of light reflected at an angle of 45° from the polished surface of several of the principal metals is given. The reflexion from gold, although not very intense, was found to be more uniform in quality than that from any other metal that was tried. Burnished lead also gave very good results. The reflexion from silver is singularly deficient in some portions of the less refrangible rays, although in most other parts the reflexion is tolerably perfect, except for rays of extremely high refrangibility.

2. *The Electric Spectra of the Metals.*—The author proceeds then to detail his experiments upon the spectra obtained by causing the sparks produced by the secondary current from the induction-coil to pass between electrodes composed of various elementary substances, and he gives photographs of the impressions obtained from collodion negatives of a considerable number of different elementary bodies. The spectra were procured by arranging a quartz-train in the manner already described. Among the elements so examined are the following :—

Platinum.	Arsenic.	Copper.
Palladium.	Tellurium.	Aluminum.
Gold.	Tungsten.	Cadmium.
Silver.	Molybdenum.	Zinc.
Mercury.	Chromium.	Magnesium.
Lead.	Manganese.	Sodium.
Tin.	Iron.	Potassium.
Bismuth.	Cobalt.	} Graphite, and } Gas-coke.
Antimony.	Nickel.	

The commencement of each spectrum in its less refrangible portion is similar in nearly all cases; and as it is this portion only which is transmissible through bisulphide of carbon, this circumstance explains the similarity of all the spectra procured by the author from different metals in his earlier experiments, already laid before the British Association. In the more refrangible parts of the spectrum great and characteristic differences between the results obtained with the different metals are at once manifest. In some cases, as in those of copper and nickel, the action is greatly prolonged in the more refrangible extremity, whilst the intense and highly characteristic spectrum of magnesium is much shorter.

In many cases metals which are allied in chemical properties exhibit a certain similarity in their spectra. This occurs, for example, with the magnetic metals, iron, cobalt, and nickel, and with the group embracing bismuth, antimony, and arsenic. The more volatile metals exhibit generally the most strongly marked lines. Cadmium, for instance, gives two intense groups. Zinc, two very strong lines near the less refrangible extremity, three near the middle, and four nearly equidistant lines towards the termination of the more refrangible portion, whilst in the spectrum of magnesium the chemical action is almost suddenly terminated near the middle by a triple group of very broad and strong lines.

It will be observed, on examining the photographs of these spectra of the various metals, that the impressions, particularly in the more refrangible portions, consist of a double row of dots, running parallel with the length of the spectrum, and forming the terminations of lines rather than lines themselves, as though the intense ignition of the detached particles of metal, necessary to furnish rays capable of exciting chemical action, had ceased before the transfer of these particles to the opposite electrode had been completed.

If each electrode be composed of a different metal, the spectrum of each metal is impressed separately upon the plate, as is evident on examining the photographs.

When alloys are employed as electrodes, the spectrum exhibited is that due to both the metals; but if the metals made use of are approximatively pure, the spectrum is hardly to be distinguished from that of the pure metal. In the case when alloys are used as electrodes, it is not always the more volatile metal which impresses its spectrum most strongly. A specimen of brass, for example, containing 38 per cent. of zinc, gave a spectrum which could not be distinguished from that of pure copper, though an alloy of three parts of gold and one of silver gave a spectrum in which the lines due to silver predominated.

The author then proceeds to describe a number of experiments upon the transmission of sparks between electrodes of different metals in a current of several different gases. The apparatus employed consisted of a glass tube; into the side an aperture was drilled, which could be closed by a plate of quartz; the ends of the tube were closed by ground brass plates, each supporting a pair of brass forceps, into which the electrodes were fitted; through the axis of the tube a current of each gas was transmitted at the ordinary atmospheric pressure.

Among the gases thus tried were hydrogen, protoxide of nitrogen, carbonic acid, carbonic oxide, olefiant gas, marsh-gas, cyanogen, sulphuretted hydrogen, sulphurous acid, nitrogen, and oxygen. The spectrum obtained from the same metal varied considerably in these different media. In hydrogen the intensity of the spectrum was greatly reduced, and the more refrangible rays were wanting, but no new rays made their appearance. In carbonic acid, carbonic oxide, olefiant gas, marsh-gas, and cyanogen, the special lines due to the metal were produced, but in each a series of identical lines

appeared, and these new lines were referable to the carbon contained in each of these gases. Each gas exhibits special lines which are continued across the spectrum, and are never interrupted like those of the metals.

The author observed that many of these gases, such as protoxide of nitrogen, hydrochloric and sulphurous acid, presented a considerable obstacle to the passage of the sparks from the induction-coil.

“On the Long Spectrum of Electric Light.” By Professor George G. Stokes, M.A., Sec. R.S. &c.

The author's researches on fluorescence had led him to perceive that glass was opaque for the more refrangible invisible rays of the solar spectrum, and that electric light contained rays of still higher refrangibility, which were quite intercepted by glass, but that quartz transmitted these rays freely. Accordingly he was led to procure prisms and a lens of quartz, which, when applied to the examination of the voltaic arc, or of the discharge of a Leyden jar, by forming a pure spectrum and receiving it on a highly fluorescent substance, revealed the existence of rays forming a spectrum no less than six or eight times as long as the visible spectrum. This long spectrum, as formed by the voltaic arc with copper electrodes, was exhibited at a lecture given at the Royal Institution in 1853; but the author, for reasons he mentioned, did not then further pursue the subject. Having subsequently found that the spark of an induction-coil with a Leyden jar in connexion with the secondary terminals yielded a spectrum quite bright enough to work by, he resumed the investigation, and examined the spectra exhibited by a variety of metals as electrodes, as well as the mode of absorption of the rays of high refrangibility by various substances. The spectra of the metals may be viewed at pleasure by means of fluorescence, and the mode of absorption of the invisible rays by a given solution may be at once observed; but there are difficulties attending the preparation in this way of sufficiently accurate maps of the metallic lines; and the great liability of the rays of high refrangibility to be absorbed by impurities present in very minute quantity renders the certain determination of the optical character, in this respect, of substances which are only moderately opaque a matter of considerable difficulty. Having found that Dr. Miller had been engaged independently at the same subject, working by photography, the author deemed it unnecessary to attempt a delineation of the metallic lines (for which, however, he has recently devised a practical method that was found to work satisfactorily), or to examine further the absorption of rays of high refrangibility by solutions of metallic salts, &c.

The present paper contains therefore mainly results obtained in other directions in the same wide field of research. Among the metals examined, the author had found aluminium the richest in invisible rays of extreme refrangibility; and accordingly aluminium electrodes were employed when the department of such rays had to be specially examined. As the bright aluminium lines of high refrangibility do not appear to have been taken by photography, a

drawing of the aluminium spectrum is given, with zinc and cadmium for comparison.

The author has also described and figured the mode of absorption of the invisible rays by solutions of various alkaloids and glucosides. Bodies of these classes, he finds, are usually intensely opaque, acting on the invisible spectrum with an intensity comparable to that with which colouring matters act on the visible. This intensity of action causes the effect of minute impurities to disappear, and thereby increases the value of the characters observed. It very often happens that at some part or other of the long spectrum a band of absorption, or maximum of opacity, occurs; and the position of this band affords a highly distinctive character of the substance which produced it.

Among natural crystals, besides the previously known yellow uranite, the author found that in adullaria, and felspar generally, a strong fluorescence is produced under the action of the rays of high refrangibility, referable not to impurities, but to the essential constituents of the crystal. A particular variety of fluor-spar shows also an interesting feature, though in this case referable to an impurity, exhibiting a well-marked reddish fluorescence under the exclusive influence of rays of the very highest refrangibility. This property renders such a crystal a useful instrument of research.

With some metals broad, slightly convex electrodes were found to have a great advantage over wires, exhibiting the invisible lines far more strongly, while with some metals the difference was not great.

The blue negative light formed when the jar is removed, and the electrodes are close together, was found to be exceedingly rich in invisible rays, especially invisible rays of moderate refrangibility. These exhibited lines independent of the electrodes, and therefore referable to the air. This blue light has a very appreciable duration, and is formed by what the author calls an arc discharge.

The paper concludes with some speculations as to the cause of the superiority of broad electrodes, and of the heating of the negative electrode.

“On the Loess of the Valleys of the South of England and of the Somme and the Seine.” By Joseph Prestwich, Esq., F.R.S.

“On the Simultaneous Distribution of Heat throughout superficial parts of the Earth.” By Professor H. G. Hennessy, F.R.S.

The principal object of this memoir is to develop the laws of the distribution of temperature in the portion of the atmosphere in contact with the earth, and to point out the connexion between the phenomena of aerial temperature and those of soil and oceanic temperature. The author maintains that hitherto no perfect physical representation of the distribution of heat over the earth's surface has been obtained. Humboldt's luminous method of representing the distribution of mean temperatures necessarily presents us with the temperatures of places at those hours of local time when the temperature happens to be equal to that of the entire day. But such hours

occur at different places not at the same moment of absolute time, and therefore the isothermal lines traced by the aid of their results alone, are not true isothermal lines in the same sense as we understand an isothermal line or surface within crystals, or other definite geometrical solids which have been recently the subjects of thermological inquiry.

The distribution of sunshine at the outer limits of the atmosphere and at its base is first considered, and the nearly circular shape of the lines of equal sunshine is pointed out. After showing the connexion between these lines and the simultaneous isothermals for the air, land, and water, the author proceeds to discuss more particularly the *ærothermal* lines. As the term isothermal line has become universal in the sense of a line joining places possessing the same mean temperatures, the author proposes to designate the true lines of simultaneous equal temperature as *synthermal* lines. If any number of places have the same temperature at a given hour corresponding to the mean time of any one meridian, these places will be *synthermal*, and a line joining them will be a *synthermal* line. For this purpose the meridian of Greenwich has been selected, and a series of *synthermal* Tables have been calculated for different places corresponding to the Greenwich hours. For the construction of these Tables, the hourly observations of temperature made at the British Home and Colonial Observatories, the observations of Russia, Austria, Prussia, and Central Europe, as well as those of the United States, have been employed. The few series of hourly observations made by Arctic and African travellers have been also applied; and in addition to the Tables thus directly constructed, others have been deduced by interpolation for stations whose geographical position rendered it desirable to bring them into the general view of temperature-distribution. All results expressed in Centigrade and Reaumur degrees have been reduced to the Fahrenheit scale. A fresh set of Tables has been formed from those corresponding to local time, with hours corresponding to the meridian at Greenwich.

The *synthermal* Tables thus obtained show, as might be *à priori* expected, still greater differences between the temperatures of places in the same parallels of latitude than the Tables of mean temperature. Thus Rome and Tiflis differ in latitude by only 13', and the mean temperature of Rome is $5^{\circ}\cdot 1$ in excess of that of Tiflis. At 8 A.M. Greenwich time, they are *synthermal*, both possessing the temperature of $59^{\circ}\cdot 1$, while at 7 A.M. Tiflis surpasses Rome by $0^{\circ}\cdot 6$, and at all other times besides these Rome surpasses Tiflis. At 4 A.M. this excess amounts to $9^{\circ}\cdot 5$. Although Peking is situated in the isothermal line which passes close to the Isle of Wight, it is *synthermal* at 5 A.M. (Greenwich) to some place 6° warmer than Rome, and probably therefore on the north coast of Africa, and is *synthermal* with a point north of the Orkneys at between 8 and 9 in the evening. Similar comparisons of distant places in both hemispheres lead to similar results. It appears that during certain periods of the day, alternately hot and cold spaces exist in the interior of the continents compared to the surrounding oceans. In the southern hemi-

sphere the rising of synthermal temperatures appears to be a little inferior to what it is in the northern, if we compare together stations with nearly corresponding latitudes and differences of longitude in both hemispheres.

From the results tabulated in his synthermal Tables, the author has projected on an equatorial map of the world, the synthermal lines of 4 A.M. and 2 P.M. Greenwich time. This map clearly exhibits the risings of the synthermals, and the existence of spaces of maximum temperature. The synthermals in both hemispheres rise towards the poles opposite these spaces, and converge towards the space of minimum tropical temperature. In islands circumstanced like the British Isles, the synthermals may be represented by two systems of closed curves, one for the day with an interior space of maximum temperature, and the other for the night with an interior space of minimum temperature. These groups would be connected somewhat in the way of the magnetic curves delineated by Gauss in his *Theory of Terrestrial Magnetism* (Taylor's *Scientific Memoirs*, vii.). The shapes of these groups would closely resemble the isothermals already published by the author*, and which, from the small differences of longitude in our islands, may be conceived to represent very closely the synthermals of 9 A.M. and 8 P.M.

The probable shapes of the lines of equal soil temperature, or syn-geothermals as the author calls them, are next considered; and it is shown that they must not only present far more remarkable deviations from equatorial parallelism than the synaërothermals, but also that their diurnal rising must be very considerable.

The author points to the connexion between some of his results and the diurnal law of the wind force discovered by Mr. Osler; and he also shows how the abnormal regressions of temperature in the latter months of spring may be partly explained by the circumstance that, although the isothermals of mean temperature during these months do not deviate widely from equatorial parallelism, the synthermals not only swing to a greater extent than during most of the other months of the year, but that they are also more closely crowded together.

These results are most strikingly developed during the month of May.

“On the Differential Coefficients and Determinants of Lines, and their application to Analytical Mechanics.” By A. Cohen, Esq.

“On the Theory of Probabilities.” By George Boole, Esq., F.R.S.

This paper has for its object the investigation of the general analytical conditions of a method for the solution of questions in the *Theory of Probabilities*, which was published in a work entitled “An Investigation of the Laws of Thought, &c. †”

The application of the method to particular problems has been

* *Proceedings*, vol. ix., and *Atlantis*, vol. i.

† London, Walton and Maberly, 1854.

illustrated in the work referred to, and more fully in a Memoir published in the Transactions of the Royal Society of Edinburgh, entitled "On the Application of the Theory of Probabilities to the Question of the Combination of Testimonies or Judgments." The latter contains also the foundations of the general analytical theory of the method. But the complete development of that theory is attended with mathematical difficulties which I have only just succeeded in overcoming.

A correspondence on the subject between Mr. Cayley and myself also appears in the May Number of the 'Philosophical Magazine,' and I owe it to Mr. Cayley that these further researches, of the results of which an account will here be given, were undertaken.

I shall make but few remarks here upon the *à priori* grounds of the method. Generally it may be said that the solution of a question in the Theory of Probabilities depends upon the possibility of mentally constructing the problem from hypotheses which appear, whether as a consequence of our knowledge or as a consequence of our ignorance, to be simple and independent. When the data are the probabilities of simple events, and no conditions are added, the problem is in theory sufficiently easy, the sole difficulty consisting in the calculation of complex combinations. But when the data are the probabilities of compound events, or when the events are connected by absolute conditions expressible in logical propositions, or when both these circumstances are present, the difficulty of the required mental construction becomes greater. If we assume the independence of the simple events from which the compound events according to their expression in language are formed, we meet, first, the difficulty that the number of equations thus formed may be greater or less than that which is requisite to obtain a solution; and, secondly, the far more fundamental difficulty that the conditions under which the solution, supposing it to be obtained, is analytically valid may not coincide with the conditions under which the data are possible. It seems indeed likely—at any rate the evidence of particular examples points uniformly to the conclusion—that any attempts to construct the problem upon hypotheses which, while not involved in the actual data are of the same nature as those data (*i. e.* which might conceivably have resulted as facts of observation from the same experience from which the data were derived), *limit* the problem, and lead to solutions which are analytically valid under conditions narrower than those under which the data are possible.

But the processes of mathematical logic enable us, without any addition to the actual data, to effect the required construction of the problem formally—formally because the hypotheses which are regarded as ultimate and independent in that construction refer to an ideal state of things. The nature of the conceptions employed, and their connexion with the conceptions involved in the actual statement of the problem, are discussed in the paper. It is sufficient to say here that, whatever difficulty there may be in these conceptions as conceptions, there is nothing arbitrary in the formal procedure of

thought with which they are connected. The probabilities of the ideal events enter as auxiliary quantities into the process of solution, and disappear by elimination from the final result, but they are throughout treated as probabilities, and combined according to the laws of probabilities. I will only say here that the difficulty which has been felt in the conception of the ideal events appears to me to arise from a misdirected attempt to conceive those events by means of the events in the statement of the problem—the true order of thought being that the events in the statement of the problem are, not indeed in their material character, but as subjects of probability and of relations affecting probability, to be conceived by means of the ideal events.

Now the probabilities which constitute the actual data will in general be subject to conditions in order that they may be derived from actual experience. Those conditions admit of mathematical expression.

Generally, if the events in the data are all or any of them compound, and if $p_1, p_2, \dots p_n$ represent their probabilities, those quantities will be subject to certain conditions, expressible in the form of linear equations or *inequations*, beside the condition that, as representing probabilities, they must be positive proper fractions. All such conditions of either kind are ultimately expressible in the general form

$$b_1 p_1 + b_2 p_2 \dots + b_n p_n + b \leq 0,$$

the coefficients $b_1, b_2, \dots b_n, b$ differing in the different conditions so as to indicate that each of the quantities $p_1, p_2, \dots p_n$ varies between a system of inferior limits expressed by linear functions of the other quantities, and a system of superior limits also so expressed.

Thus, if A, B, C represent any simple events, and if p_1 represent the probability of the concurrence of B and C, p_2 that of the concurrence of C and A, p_3 that of the concurrence of A and B, then p_1, p_2, p_3 must, in order that they may be derived from experience, satisfy the conditions

$$p_1 \leq p_2 + p_3 - 1, \quad p_2 \leq p_3 + p_1 - 1, \quad p_3 \leq p_1 + p_2 - 1,$$

as well as the conditions implied in their being positive proper fractions.

On the other hand, the ideal events being by hypothesis simple and independent, the auxiliary quantities which represent their probabilities will be subject to no other condition *à priori* than that of being positive proper fractions—to no other condition *à priori*, because their actual values are determined in the process of solution.

Now the most general results of the analytical investigation are—

1st. That the auxiliary quantities representing the probabilities of the ideal events admit of determination as positive proper fractions, and, further, of a single definite determination as such, precisely when the original data supply the conditions of a possible experience.

2ndly. That as a consequence of this the probability sought will

always lie within such limits as it would have had if determined by actual observation from the same experience as the data.

The proof of these propositions rests upon certain general theorems relating to the solution of a class of simultaneous algebraic equations, and, auxiliary to this, to the properties of a functional determinant.

The following are the principal of those theorems:—

1st. If the elements of any symmetrical determinant are all of them linear homogeneous functions of certain quantities $a_1, a_2, \dots a_r$ —if the coefficients of these quantities in the terms on the principal diagonal of the determinant are all positive—and if, lastly, the coefficients of any of these quantities in any row of elements are proportional to the corresponding coefficients of the same quantity in any other row, then the determinant developed as a rational and integral function of the quantities $a_1, a_2, \dots a_r$ will consist wholly of positive terms.

And, as a deduction from the above,

2ndly. If V be a rational and entire function of any quantities $x_1, x_2, \dots x_n$, involving, however, no powers of those quantities, and all the coefficients being constant, and if in general V_i represent the sum of those terms in V which contain x_i as a factor, and V_{ij} the sum of those terms in V which contain the product $x_i x_j$, then the determinant

$$\begin{vmatrix} V & V_1 & V_2 & \dots & V_n \\ V_1 & V_{11} & V_{12} & \dots & V_{1n} \\ V_2 & V_{21} & V_{22} & \dots & V_{2n} \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ V_n & V_{n1} & V_{n2} & \dots & V_{nn} \end{vmatrix}$$

will on development consist wholly of positive terms.

3rdly. The definitions being as above, and the function V being in form complete, *i. e.* containing all the terms which by definition it can contain, the system of simultaneous equations

$$\frac{V}{V} = p_1, \quad \frac{V_1}{V} = p_2, \quad \dots \quad \frac{V_n}{V} = p_n,$$

in which $p_1, p_2, \dots p_n$ represent positive proper fractions, will admit of one, and only one, solution in positive integral values of $x_1, x_2, \dots x_n$.

4thly. The function V being incomplete in form, *i. e.* wanting some of the terms which it might by definition contain, the system of equations

$$\frac{V_1}{V} = p_1, \quad \frac{V_2}{V} = p_2, \quad \dots \quad \frac{V_n}{V} = p$$

will admit of one, and only one, solution in positive integral values of $x_1, x_2, \dots x_n$, provided that $p_1, p_2, \dots p_n$, beside being proper fractions, satisfy certain conditions depending upon the actual form of V .

These conditions are expressible by linear equations or inequations

of the general form,

$$b_1 p_1 + b_2 p_2 \dots + b_n p_n + b = 0.$$

In the application to the theory of probabilities, the form of the function V depends upon the explicitly determined logical connexion of the events in the data; the equations or inequations of condition correspond to the conditions of possible experience as a source of the data.

It appears, therefore, that, quite independently of any question of the validity of the logical, and I ought perhaps to add philosophical, grounds of the method, it is a perfect method of interpolation. The analytical investigation, however, shows that, for the mere purpose of interpolation, the process might be modified by altering the coefficients of V without affecting its form; but it indicates at the same time that such modifications have no definite analogy with that process by which weight is assigned to astronomical observations, and, from their arbitrary character, lead to results which cannot properly be regarded as expressions of probability in any sense.

XLIII. *Intelligence and Miscellaneous Articles.*

ON THE STRATIFICATION OF THE ELECTRIC LIGHT.

BY M. REITLINGER.

M. REITLINGER has proposed, in a note presented to the Academy of Sciences of Vienna (January 3, 1861), a new explanation of the phenomenon known under the name of the stratification of the electric light, which he has imagined from some of the results of his observations on Geissler's tubes. A great number of these tubes are formed with large and narrow portions alternately, and emit different colours of light in the two portions when they are traversed by an electric discharge. This curious phenomenon has probably been perceived by most of those who have experimented on the electric light. M. von Ettingshausen found that the different luminous parts have totally different constitutions, and intimated to M. Reitlinger this particularity as an interesting subject for study.

In front of a narrow slit, a rectangular prism is placed in a situation so that the lateral rays of light from some source may so come, on being reflected entirely on the hypotenuse face, as to illuminate the upper half of the slit, while the other half is illuminated by direct rays from another source.

It is possible in this manner to compare the spectra of the two lights and perceive their identity or their differences. M. Reitlinger has found that in this manner the spectrum of the narrow parts of an undulating tube constructed by Geissler, and sold by him as a hydrogen-tube, was the spectrum of pure hydrogen, while the spectrum of the larger parts was that of oxygen. The diversity of lights consequently was owing to the diversity of the luminous substances; and the passage of the electricity appeared to separate from one

another the two mixed gases, at the same time that it rendered them luminous.

The presence of oxygen in a hydrogen-tube had probably something to do with its preparation. M. Plücker has said that it was possible to use steam for preparing these tubes, and that the passage of the first electric discharges sufficed to place the hydrogen at liberty: such a separation of mixed gases has appeared to M. Reitlinger to furnish the explanation of the stratification of the electric light. He admits, for instance, that in a tube prepared with steam, the hydrogen and oxygen dispose themselves in alternate layers; and that the hydrogen, a much better conductor than the oxygen, is less heated and becomes less luminous, that is, relatively obscure: generally speaking, stratification would result from the disposition in alternate layers of two gases unequally conductive.

1st. A tube full of dry air under the pressure of 1.5 millim. gave a spectrum in which only the rays characteristic of nitrogen were seen, and which presented not the slightest trace of stratification: no very sensible difference was perceivable between the two wide tubes and the capillary tube of communication of which the entire apparatus was formed; the spectrum of the capillary part offered merely some rays which were wanting in the spectrum of the wider parts. The introduction of a small quantity of pure hydrogen caused the stratification to appear in the wide tubes without sensibly modifying the aspect of the corresponding spectra. In the narrow tube no appearance of stratification was produced at first, but to the spectrum of nitrogen there was superposed a bright spectrum characteristic of hydrogen. The introduction of hydrogen being continued, and the pressure raised to 6 millims., the light of the capillary tube became stratified in its turn, and the tube presented somewhat the aspect of a chaplet of brilliant pearls.

2nd. The electric light was developed in the barometrical vacuum, the mercury itself supplying the place of one of the electrodes: only a white homogeneous light was perceived, without stratification. By the introduction of some globules of air, there was immediately formed a series of luminous layers alternately more or less brilliant, but without the inequality degenerating into obscurity. A spectrum of the light showed at the same time the rays of the mercury and that of the air. By regulating properly the intensity of the current and the electric force of the introduced air, the layers less brilliant can be brought to an almost complete obscurity.

3rd. With simple gases no stratifications have been produced.—*Ann. de Chim. et de Phys.*, Jan. 1863, p. 114.

ON THE SPECTRUM PRODUCED BY THE FLAME EVOLVED IN THE
MANUFACTURE OF CAST STEEL BY THE BESSEMER PROCESS.
BY PROFESSOR ROSCOE.

The spectrum of this highly luminous and peculiar flame exhibits, during a certain phase of its existence, a complicated but most

characteristic series of bright lines and dark absorption-bands. Amongst the former the sodium, lithium, and potassium lines are most conspicuous; but these are accompanied by a number of other, and as yet undetermined, bright lines; whilst among the absorption-bands those formed by sodium-vapour and carbonic oxide can be readily distinguished. Professor Roscoe expressed his belief that this first practical application of the spectrum analysis will prove of the highest importance in the manufacture of cast steel by the Bessemer process.—*Proceedings of the Literary and Philosophical Society of Manchester*, February 24, 1863.

ON THE EXISTENCE OF A CRYSTALLIZABLE CARBON COMPOUND
AND FREE SULPHUR IN THE ALAIS METEORITE. BY PROFESSOR
ROSCOE.

Through the kindness of R. P. Greg, Esq., of Manchester, I was placed in possession of about a gramme and a half of this peculiar meteorite, which fell near Alais, in France, on March 15, 1806, and was examined by Berzelius in 1834. This distinguished chemist states* that the Alais meteorite is remarkable as containing an organic carbon-compound, soluble in water, which turns brown on heating, deposits a black carbonaceous mass, and burns without residue. In the year 1860, Wöhler† discovered the presence of small traces of a crystallizable hydrocarbon, soluble in alcohol and ether, in two meteorites, one of which fell at Kaba, in Hungary, on April 15, 1857, and the other at Bokkevelde, in South Africa, on October 13, 1838. The fact thus undoubtedly proved, of the existence in these two meteorites of crystallizable carbon-compounds, which in terrestrial matter are solely the results of vital action, rendered a further confirmation of the existence of organic matter in the Alais meteorite of special interest.

In general appearance the small fragments of the meteorite experimented upon coincided exactly with the minute description of the substance given by Berzelius; the white efflorescence which covers the surface of the mineral was found to consist mainly of small crystals of sulphate of magnesium; the only other bodies which could be detected by spectrum analysis were soda and lime. Iron was not contained in the soluble salts. On extracting 1·0583 gramme of the meteorite with water, 0·1155 gramme of soluble salts was dissolved, corresponding to 10·91 per cent., and thus closely agreeing with Berzelius's estimation of 10·3 per cent.

Ether was found to dissolve from the residue 1·94 per cent. of the original meteorite, a substance which on evaporation was deposited in distinct crystals. The crystals possessed a peculiar aromatic odour, and melted at 114° C., subliming on heating, and leaving a slight carbonaceous residue. Under the microscope the

* *Pogg. Ann.* vol. xxxiii. p. 113.

† *Wien. Acad. Ber.* vol. xxxv. p. 5; xli. p. 565.

crystals were seen to be of two forms, one acicular, the other rhombic. The acicular crystals were difficultly soluble in absolute alcohol, but easily soluble in ether, bisulphide of carbon, turpentine, and cold nitric acid, and dissolved in sulphuric acid with formation of a brown colouring-matter; the rhombic crystals likewise dissolved in ether and bisulphide of carbon, but were unaltered by cold nitric and sulphuric acids, or turpentine. The ethereal extract gave no reaction for sulphuric acid; but after boiling with nitric acid, a copious precipitate of sulphate of barium was deposited. When burnt in a stream of dry oxygen gas, 0.0078 gramme of the extract, dried at 100° C., yielded 0.010 gramme of sulphurous acid, 0.008 gramme of carbonic acid, and 0.003 gramme of water. Hence the meteorite contained 1.24 per cent. of free sulphur, 0.54 per cent. of carbon, and 0.1 per cent. of hydrogen, in a form soluble in ether. The meteorite contains a considerable quantity of carbon (probably as graphite) which is insoluble in ether. The total percentage of carbon found on igniting the meteorite in oxygen amounted to 3.36 per cent.; this closely corresponds with the amount found by Berzelius, viz. 3.05 per cent.

From the above it is evident that the Alais meteorite contains at least a half per cent. of a hydrocarbon which is deposited in acicular crystals when the mass is treated with ether, together with considerable quantities (more than 1 per cent.) of free sulphur, crystallizing from the ethereal solution in rhombic octahedra. To judge by the melting-point, the hydrocarbon may be analogous to a mineral wax called Könlite, discovered by Kraus in the lignite of Uznach, which contains an equal number of atoms of carbon and hydrogen, and melts at 114° C.—*Proceedings of the Literary and Philosophical Society of Manchester*, February 24, 1863.

ON A NEW AND EXTREMELY SENSITIVE THERMOMETER.

BY DR. JOULE, F.R.S.

Some years ago I remarked the disturbing influence of currents of air on finely suspended magnetic needles, and suggested that it might be made use of as a delicate test of temperature. I have lately carried out the idea into practice, and have obtained results beyond my expectation. A glass vessel in the shape of a tube, 2 feet long and 4 inches in diameter, was divided longitudinally by a blackened pasteboard diaphragm, leaving spaces at the top and bottom, each a little over 1 inch. In the top space a bit of magnetized sewing-needle, furnished with a glass index, is suspended by a single filament of silk. It is evident that the arrangement is similar to that of a "bratticed" coal-pit shaft, and that the slightest excess of temperature on one side over that on the other must occasion a circulation of air, which will ascend on the heated side, and, after passing across the fine glass index, descend on the other side. It is also evident that the sensibility of the instrument may be increased to any required extent, by diminishing the directive force of the magnetic needle. I purpose to make several improvements in my present

instrument; but in its present condition the heat radiated by a small pan, containing a pint of water heated 30°, is quite perceptible at a distance of three yards. A further proof of the extreme sensibility of the instrument is obtained from the fact that it is able to detect the heat radiated by the moon. A beam of moonlight was admitted through a slit in a shutter. As the moon (nearly full) travelled from left to right the beam passed gradually across the instrument, causing the index to be deflected several degrees, first to the left and then to the right. The effect showed, according to a very rough estimate, that the air in the instrument must have been heated by the moon's rays a few ten-thousandths of a degree, or by a quantity no doubt the equivalent of the light absorbed by the blackened surface on which the rays fell.—*Proceedings of the Literary and Philosophical Society of Manchester*, March 11, 1863.

ON THE MOTION OF VAPOURS TOWARD THE COLD.

BY THOMAS WOODS, M.D.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In some of the late numbers of your Magazine I perceive there is a discussion between Dr. Draper and Mr. Tomlinson as to priority of the true explanation of the cause of the motion of certain volatile substances towards the light. It is very remarkable how often scientific men dispute priority of invention or discovery, and claim it with the most sincere belief in the truth of their statements. These disputes would form an interesting chapter in the curiosities of scientific literature. I believe these claims arise from want of sufficient attention on the part of the disputants to their true position. For instance, one man devotes much time to a particular pursuit, and arrives at certain broad principles which satisfy him. Another, perhaps years after, happening to work out some part of the same investigation in detail, publishes as an independent discovery what might, *when it is known*, be deduced from the researches of his predecessor, but which would for ever be a dead letter were it not for his labour. The philosopher who was first in the field, however, is astonished (unjustly, I think) that any discovery should be claimed; and hence many of these disputes.

Dr. Joule and myself were, I believe, somewhat in this predicament with respect to the heat absorbed by the decomposition of compounds*; and I fancy such *désagréments* will constantly be occurring, and not altogether without benefit to the public, as every detail is so fully and prominently brought out.

I make these remarks because, if Mr. Tomlinson had delayed another month to publish his ideas on "the motion of camphor

* *Phil. Mag.*, 1856. vol. xii.

towards the light," I might have also laid claim to the explanation, as I happened to be engaged in a course of experiments to illustrate the proposition he proved. My reason for alluding to the subject now is that I may mention an hitherto undervalued fact which I observed during their progress.

I commenced my research with the idea that it might possibly happen that Light would act as a sort of analyser, separating volatile substances from each other. I fancied, for instance, if camphor moved towards the light, other volatile matters might move in an opposite direction, or that certain circumstances might determine such a course. It would be useless to mention any of the substances I experimented on, or the circumstances in which I placed them, as I ultimately came to the conclusion that light was not the moving power at all, but temperature.

During these trials I dissolved, in a saturated solution of camphor in alcohol, some iodine, so as to colour it darkly; and having exposed a little of this mixture in a corked flat phial in a window, I found that on whichever side was the warmer the coloured fluid rose by capillary attraction, and on that side only. With other coloured fluids I observed the same action; and I found, in fact, that I had a most sensitive differential thermometer, for no matter how slight the difference of temperature of the sides, although inappreciable to the most delicate thermometer, the capillary attraction caused a rise of the liquid on the side of the vessel which was nearer to the heat, and the height attained seemed to be in proportion to the difference of temperature.

If vapour of water be in the phial—a condition easily brought about by placing a drop or two of this fluid on *the side* of the vessel within, not mixing them with the spirit,—it will always be deposited on the side opposite to that on which the rise of fluid by capillary attraction takes place, no matter how the light falls on the bottle; showing, of course, that the cold surface condenses the vapour, and the hot one causes the fluid to rise.

And when we consider that the sides of the phial containing the coloured fluid and enclosing the vapour are only about half an inch apart, and that the vessel is placed where no difference of temperature of its sides apparently exists, we are tempted to think that the vapour is condensed more in *the direction* than in the actual presence of the cold, while the coloured fluid is raised in the opposite—radiation having more to do with the phenomena than sensible difference of temperature.

However this may be, the delicacy of this simple contrivance (a flat corked phial, in which is contained a little coloured spirituous liquid and vapour of water) for showing difference of temperature in opposite points is remarkable, and might, I think, be taken advantage of in meteorological investigations.

When the fluid is raised, and the vapour deposited, if the vessel is turned round so as to reverse the aspect of the sides, the fluid entirely leaves the side now pointing to the colder region, and rises

on the other, and the vapour acts in a similar manner, so that only one side at the same time is affected by each.

Your obedient servant,

THOMAS WOODS.

Parsonstown, February 1863.

ON SOME SPECIMENS OF PSEUDOMORPHS IN THE IMPERIAL
MUSEUM OF VIENNA*. BY DR. TSCHERMAK.

The Imperial Museum of Vienna contains many very curious specimens of Pseudomorphs, and among them are several calculated to throw light on controverted geological questions. In a hand-specimen of gneiss from the Rathhaus-Berg, near Gastein, amphibole has replaced mica. This gneiss contains altered amphibole in the interior of masses of mica; and it may therefore be inferred, with a certain degree of probability, that the whole of this gneiss, whose felspathic components likewise bear traces of metamorphism, is an altered amphibolic rock. The same, so far as may be inferred from cabinet-specimens, may be the case with other varieties of gneiss, especially with those of Brazil.

Pseudomorphs of compact felspar, replacing crystals of the same mineral species, occur in the antique green porphyry of Italy and Egypt, as also in the diabasic porphyry from the Hartz,—a fact not unimportant in regard to the origin of certain varieties of this group of rocks. A double Pseudomorph—crystallized gypsum metamorphosed into fibrous gypsum, and this, in the course of time, into fibrous quartz—occurring, together with spaces left by the decomposition of crystallized iron pyrites, in a specimen of chloritic argillaceous slate from the Eifel, may suggest some of the processes undergone by this rock before it arrived at its present condition. Some other cases of Pseudomorphs, as specular oxide of iron replacing olivine in the trappean rocks around Edinburgh; opal replacing nephrite, from Elbingerode; opal replacing augite, from the same locality; and calcareous spar having taken the place of augite in the metamorphosed pyroxenic porphyry of Tökörö (Transylvania), have been neither noticed nor described before.—*Proc. Imp. Acad. Vienna*, November 6, 1862.

NOTES ON THE OLD EGYPTIAN AND GREGORIAN CALENDARS.

BY S. M. DRACH, F.R.A.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In Schubert's *Reise in das Morgenland* (vol. ii. p. 179) the author states that the Egyptians made 309 synodical months equal to 25 years of 365 days, equivalent to a month of 29^d 12^h 44^m 16^s.3107. With the hitherto accepted secular acceleration of Laplace (*Vince*,

* Communicated by Count Marschall.

vol. iii. p. 29) = $10'' \cdot 18621268n^2 + 0 \cdot 0185384408n^3$, the above month-duration actually occurred about 1000 B.C. Assuming, with Professor Adams, only the half of the above coefficient, the aforesaid month-duration occurred about 2200 B.C. This period having actually been observable by man, and the years being *twenty-five*, render the above German remark very interesting.

If, in the Gregorian calendar, 999 years had been made a complete cycle, disallowing the leap-day of 100, 200, 300, 500, 600, 700, 900, there would have been introduced $\frac{1}{4}(996) - 7 = 242$ leap-days, so that the average year = $365 \cdot 242242242$, very nearly true = $365^d 5^h 48^m 49^s \cdot 792$ (or $49 \frac{88}{111}$). Thus the Gregorian cycle-epochs would have been 1600, 2599, 3598, 4597, 5596, &c.

S. M. DRACH.

March 1863.

ON A PSEUDOMORPH OF MICA AFTER CORDIERITE*.

BY DIRECTOR HAIDINGER.

This curious specimen was lately found at Grünburg (Upper Austria) in a granular white orthoclase, intermixed with small granules of quartz, small icositetrahedra of reddish-brown garnet, and minute and scarce particles of iron- and copper-pyrites. The crystals are dodecagonal prisms, with a terminal plane vertical to the principal axis; they are 2 inches long and 1 inch in diameter, of a dark-green colour, reminding one in their outward aspect of pinite, gigantolite, or chlorophyllite. Their completely crystallized portion is in immediate contact with greyish-white diaphanous quartz. The planes by which they are in contact with the orthoclastic matrix are irregular and not well defined. In their interior the whole of the cordierite has been metamorphosed into mica, whose lamellæ lie parallel to the surface of the crystals. Chev. C. von Hauer, who has analysed them, found their constituent elements to be:—

Silica	44·94
Alumina	24·90
Oxide of iron (with a little oxide of manganese)	13·18
Magnesia	2·64
Potash	8·94
Soda	2·06
Loss by ignition	2·74
	<hr/> 99·40

The last item answers to a combination of one atom of trisilicate of potash with one atom of protosilicate of alumina. In the unaltered condition of Cordierite, two atoms of bisilicate of magnesia are combined with one atom of protosilicate of alumina.—*Proc. Imp. Acad. Vienna*, Dec. 11, 1862.

* Communicated by Count Marschall.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MAY 1863.

XLIV. *Account of some Experiments showing the Change of Rate produced in a Clock by a particular case of Magnetic Action.*
By WILLIAM ELLIS, Esq., of the Royal Observatory, Greenwich.*

HAVING made a series of experiments, the particular object of which was to ascertain whether it would be possible easily to change permanently the rate of a clock by the action of magnets, and so avoid the necessity of touching its pendulum screw, I have been requested by the Astronomer Royal to draw up an account of these experiments, since the results appear to possess some features which are in themselves likely to be interesting generally, independently of the possible practical application of the principle. The account is not, however, offered as containing anything which adds in any important degree to fundamental laws already known, but chiefly because the experiments so well illustrate the peculiar action of two magnets on each other when brought very near together.

The clock made use of for trial of the principle was one of the spare clocks at the Royal Observatory—one whose pendulum, vibrating seconds, consisted of a wooden rod fitted with a lenticular-shaped bob of lead. Near the lower end of the pendulum-rod there was fixed a permanent bar magnet, in a vertical position. Above this, and supported by the clock-case, there was fixed another magnet, entirely similar, also in a vertical position, and so placed that, when the pendulum-rod was at rest, the lower end of the fixed magnet was precisely over the upper end of the pendulum-magnet. The broad part of the magnets was towards the front. The clock-rate having been found with the pendulum-magnet only in position, the fixed magnet was then added, and the rate determined with this magnet placed at different distances above the pendulum-magnet. The clock was rated with the poles of each magnet in reversed positions with

* Communicated by the Author.

respect to the other magnet, giving four sets of observations. Between each set and again at the completion of the observations, the clock-rate was determined with the pendulum-magnet only in position: these determinations of the clock's normal rate were very satisfactory as respects steadiness of rate: the changes produced by the action of the magnets on each other, as exhibited in the following Table, may therefore be considered (for the magnets used) very exact. When the adjacent poles of the two magnets were similar, the effect of the repulsion retarded the clock; when the adjacent poles were unlike, the effect of the attraction accelerated the clock. In the following Table is given the increase or decrease of hourly gaining rate for every position of magnets and distance of magnets tried, with the corresponding total arc of vibration:—

Distance between adjacent poles of magnets.	Lower pole of fixed magnet.				Arc of vibration.	
	S.	N.	N.	S.		
	Upper pole of pendulum-magnet.				Repelling magnets.	Attracting magnets.
	S.	S.	N.	N.		
Increase of hourly gaining rate.						
in.	s.	s.	s.	s.	°	'
0.03	-2.08	4	42
0.04	+7.40	4 11
0.05	-2.04	4	44
0.06	-2.42	4	38
0.06	+5.11	4 14
0.07	+5.18	4 24
0.12	+4.10	4 26
0.18	-1.78	4	49
0.20	-2.02	4	49
0.21	+2.72	4 19
0.26	+2.84	4 27
0.34	-1.43	4	45
0.35	+2.16	4 33
0.58	+1.58	4 37
0.67	-0.94	4	44
0.76	-0.97	4	37
0.81	+1.02	4 31
0.98	-0.59	4	51
1.13	+0.66	4 42
1.71	-0.22	4	43
1.71	-0.29	4	32
1.79	+0.26	4 38
1.97	+0.24	4 38
2.26	-0.16	4	42
2.40	+0.11	4 34

It is to be understood that the arc of vibration given in the foregoing Table is the total arc reckoning from the extreme limit of vibration of the pendulum on one side of the perpendicular to its extreme limit on the opposite side. The amount of arc when no magnetic action existed (that is, with the pendulum-

magnet only in position) was $4^{\circ} 38'$. As respects change of the arc, it will be noticed that, as the distance is decreased, there is a marked decrease of arc when the magnets attract, but a very slight tendency to increase when the magnets repel.

As respects change of rate, it appears, for a given distance of magnets, that when the adjacent poles were similar, the clock was not retarded to the same extent to which it was accelerated when the poles were unlike. The ratio between the two appears to be somewhat similar to the ratio between the two corresponding changes of arc noticed above.

It will be seen that the distances between the magnets in the different trials were not the same: the fixed magnet was placed at about the desired distance from the pendulum-magnet, and the exact distance afterwards accurately measured. In consequence of this a direct comparison of results for a given distance is not possible with the numbers only as given in the preceding Table. To obtain such comparison, the numbers of that Table were laid down graphically, taking the distance of magnets for abscissa, and change of rate as ordinate: curves were then drawn to include all the points. The following Table contains the numbers read off from these curves:—

Distance between adjacent poles of magnets.	Lower pole of fixed magnet.				Mean values	
	S.	N.	N.	S.		
	Upper pole of pendulum-magnet.				of columns 3 and 5, attraction.	of columns 2 and 4, repulsion.
	S.	S.	N.	N.		
Increase of hourly gaining rate.						
in.	s.	s.	s.	s.	s.	s.
0.05	-2.04	+6.25	-2.44	+5.55	+5.90	-2.24
0.075	-1.99	+4.98	-2.37	+4.35	+4.67	-2.18
0.10	-1.94	+4.40	-2.30	+3.79	+4.10	-2.12
0.125	-1.89	+4.02	-2.23	+3.41	+3.71	-2.06
0.15	-1.84	+3.73	-2.16	+3.13	+3.43	-2.00
0.175	-1.79	+3.48	-2.09	+2.94	+3.21	-1.94
0.20	-1.74	+3.26	-2.02	+2.80	+3.03	-1.88
0.25	-1.63	+2.90	-1.88	+2.57	+2.73	-1.76
0.30	-1.52	+2.59	-1.75	+2.36	+2.47	-1.64
0.35	-1.41	+2.33	-1.64	+2.16	+2.24	-1.52
0.40	-1.31	+2.12	-1.54	+1.97	+2.05	-1.42
0.45	-1.22	+1.94	-1.44	+1.80	+1.87	-1.33
0.50	-1.15	+1.79	-1.35	+1.66	+1.73	-1.25
0.60	-1.02	+1.52	-1.19	+1.42	+1.47	-1.10
0.70	-0.90	+1.29	-1.05	+1.21	+1.25	-0.97
0.80	-0.78	+1.11	-0.92	+1.04	+1.07	-0.85
0.90	-0.67	+0.96	-0.80	+0.90	+0.93	-0.74
1.00	-0.58	+0.82	-0.70	+0.77	+0.80	-0.64
1.20	-0.44	+0.61	-0.54	+0.57	+0.59	-0.49
1.40	-0.33	+0.45	-0.42	+0.43	+0.44	-0.38
1.60	-0.25	+0.34	-0.33	+0.34	+0.34	-0.29
1.80	-0.20	+0.26	-0.27	+0.28	+0.27	-0.24
2.00	-0.17	+0.20	-0.23	+0.23	+0.22	-0.20
1	2	3	4	5	6	7

The last two columns of the preceding Table show, for the magnets used, the mean effect of attraction and the mean effect of repulsion. Confining our attention to these numbers, we perceive how rapidly the change due to attraction accelerates as the distance between the magnets is decreased, whilst that due to repulsion advances only by small increments. This is, however, a natural result of the known action of magnets; for when the poles of two magnets are brought near together, each one tends to develop or induce in the other an opposite magnetism, which may combine with or act against the magnetism already existing therein. So that when the poles are unlike, the total force in action is the sum of two effects—one depending on the permanent magnetisms, which is attractive, the other on the induced magnetisms, which is also attractive; the total force is consequently attractive and strong. But when the poles are similar, the total force in action is the difference of two effects—one (probably always the larger when the magnets are equal in size and similar in constitution) depending on the permanent magnetisms, which is repulsive, the other on the induced magnetisms, which is, as before, attractive; and under such conditions the total force is repulsive but weak. Had, however, one of the magnets been much larger than the other, it might have happened, when similar poles were brought near together, that the repulsive part of the force (depending on the permanent magnetisms) might have been less than the attractive part (depending on the induced magnetisms), giving not a total repulsive force at all, but an attractive force which might be weak or strong according to the relative difference in size and constitution of the two magnets.

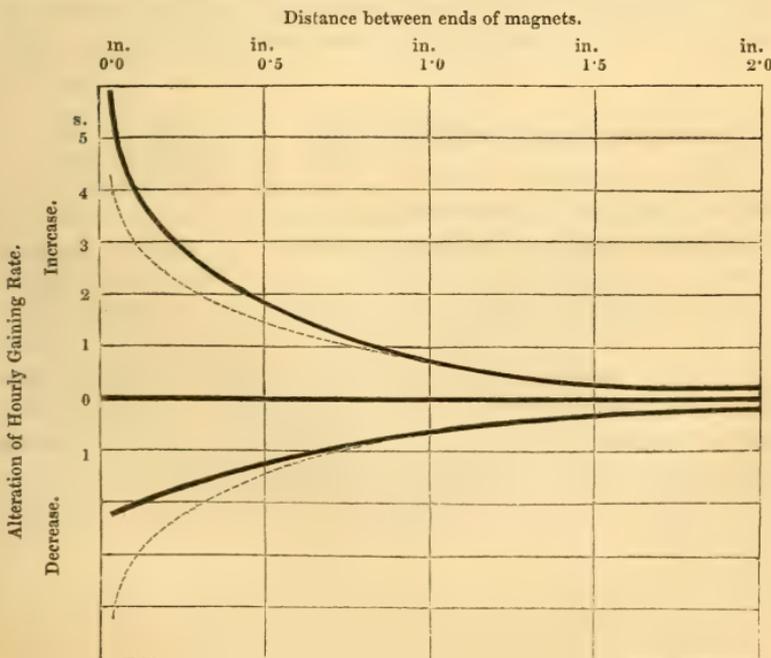
If, in the two cases considered above, the effect of induction in the one case, when the poles were unlike, may be considered equal to its effect in the other case, when the poles were similar, the half sum of the numbers contained in the last two columns of the preceding Table (taken without regard to sign) will give the change of rate due to the permanent magnetism; and the half difference (similarly without regard to sign) will give the change due to the induced magnetism. Numbers formed in this manner for all the different values of distance are given in the following Table, P denoting the change due to permanent magnetism (attractive, and increasing the gaining rate, for unlike poles; repulsive, and decreasing the rate, for similar poles), I denoting the change due to induced magnetism (always attractive and increasing the rate):—

Distance.	Values of P.	Values of I.	Distance.	Values of P.	Values of I.
in.	s.	s.	in.	s.	s.
0.05	4.07	1.83	0.50	1.49	0.24
0.075	3.425	1.245	0.60	1.285	0.185
0.10	3.11	0.99	0.70	1.11	0.14
0.125	2.885	0.825	0.80	0.96	0.11
0.15	2.715	0.715	0.90	0.835	0.095
0.175	2.575	0.635	1.00	0.72	0.08
0.20	2.455	0.575	1.20	0.54	0.05
0.25	2.245	0.485	1.40	0.41	0.03
0.30	2.055	0.415	1.60	0.315	0.025
0.35	1.88	0.36	1.80	0.255	0.015
0.40	1.735	0.315	2.00	0.21	0.01
0.45	1.60	0.27			

It will be noticed in the above Table how rapidly the values of I diminish for increase of distance as compared with the values of P, the inductive action becoming much sooner comparatively insignificant. And the ratio of change as regards distance varies considerably, as will be seen by the following numbers selected from the Table last given :—

Distance.	P.	I.
in.	s.	s.
0.05	4.07	1.83
0.10	3.11	0.99
0.20	2.455	0.575
0.40	1.735	0.315
0.80	0.96	0.11
1.60	0.315	0.025

The general character of the variations is so much more strikingly seen when the results are thrown into the shape of curves, that it seemed desirable to add the following diagram :—



In the above diagram the upper of the strong curved lines shows the total change produced by attraction, the lower the total change produced by repulsion: they are formed from the sixth and seventh columns of the second Table. The dotted lines, which on the right-hand side of the diagram are lost in the strong lines, represent the values of P, as given in the third Table. The length of ordinate between the dotted line and the strong line in each case represents the effect of I as contained in the same Table: this combined with P, bending P upwards in both cases, produces the results indicated by the strong lines.

It remains to be stated that the length of the clock-pendulum to the effective end of the pendulum-magnet (the upper end) was 33·25 inches; also that the centre of the magnet in that part swung to 1·34 inch on each side of the perpendicular, corresponding to a total arc of 4° 38'. This is the normal arc, with the pendulum-magnet only in position. With both magnets in position and magnetic action existing, the extent of swing, if required, can be inferred from the numbers given in the last two columns of the first Table. The total weight of the pendulum as fitted with its magnet was 7 lbs. 14 oz. avoirdupois.

The magnets were similar in size, their dimensions being, length 5·6 inches, breadth 0·6 inch, and thickness 0·13 inch. The following observations of the deflection of two light pocket compass-needles by the magnets may give an idea of their strength. The magnets were placed in an east and west direction, and in reversed positions of poles, both on the east side and on the west side of the compass-needles. Each of the following numbers is consequently the mean of four observations. Needle A is the heavier:—

At a distance of 6 inches (measuring from the nearest end of the magnet to the centre of the compass-needle)—

	A.	B.
Pendulum-magnet deflected needle . .	50 $\frac{1}{4}$	49
Fixed " " " . .	39 $\frac{3}{4}$	39 $\frac{1}{2}$

At a distance of 12 inches—

Pendulum-magnet deflected needle . .	10 $\frac{3}{4}$	12 $\frac{1}{4}$
Fixed " " " . .	7 $\frac{3}{4}$	9 $\frac{1}{2}$

The pendulum-magnet appears to be stronger than the other.

As respects the possible application of the principle now treated of to the special object of changing the rate of a clock, it will be understood that, in such application, means must be provided for delicately moving in a vertical direction the magnet corresponding to that here called the fixed magnet: then the

magnets being mounted, and the clock once adjusted to time by the pendulum screw, any future change of rate may be counteracted by advancing or withdrawing the fixed magnet through a very small space. The magnets should not be placed too near together, for which, indeed, there is no necessity, as a quite sufficient amount of action may be commanded even when they are separated by a comparatively large distance.

Royal Observatory, Greenwich,
March 27, 1863.

XLV. Experimental Researches on the Laws of Evaporation and Absorption. By T. TATE, Esq.

[Concluded from vol. xxiii. p. 289.]

On the Cooling Effects of Evaporation.

1. **D**ALTON determined the amount of evaporation from liquids at different temperatures. Now the object of the present inquiry is to determine the cooling effects produced by the evaporation of liquids at different temperatures. The following law of cooling seems to be fairly established by the experiments.

The cooling effect of the evaporation of liquids, at a constant atmospheric pressure, varies as the tension of the vapour at the given temperature multiplied by the latent heat of the vapour at that temperature.

Thus, let v = the rate of cooling per minute, estimated in degrees of the thermometer at T temperature, p pressure of vapour, and L latent heat of vapour at that temperature; v_1 = the rate of cooling of the same or any other liquid at T_1 temperature, p_1 pressure of vapour, and L_1 latent heat of vapour; then

$$\frac{v}{v_1} = \frac{p \times L}{p_1 \times L_1}.$$

Experiment I. A tin canister, clothed with woollen cloth, and filled with hot water, had its mouth closed by a perforated cork through which a thermometer passed into the centre of the hot water. The whole was suspended in the air, and the intervals of time requisite to cool down the thermometer every successive ten degrees were noted, first when the woollen cloth was dry, and second when it was damped with water. In the first case the rate of cooling due to radiation alone was found, and in the second case the rate of cooling due to radiation and evaporation was found, the difference of these giving the rate of cooling due to evaporation alone. The experiments were made when the air

was unusually dry, the temperature being 53° ; so that the vaporization, considering the elevation of the temperature of the water above that of the surrounding air, might be regarded as taking place in dry air.

TABLE of results of Experiments giving the rate of the cooling effect of Evaporation.

Temperature, in degrees Fahr.	Wet canister.		Dry canister.		Rate of cooling due to evaporation, v .	Corresponding tension of vapour, p .	Value of v by formula $v = \frac{p \times L}{2330}$.
	Intervals of time, in minutes, for 10° .	Rate of cooling per minute, in degrees Fahr.	Intervals of time, in minutes, for 10° .	Rate of cooling per minute, in degree Fahr.			
180°	0	0				
175	6.90	1.20	5.70	13.635	5.62
170	1.45	8.33				
165	5.71	1.09	4.62	10.820	4.50
160	1.75	9.17				
155	4.5495	3.59	8.512	3.56
150	2.20	10.50				
145	3.6383	2.80	6.633	2.79
140	2.75	12.00				
135	2.8573	2.12	5.116	2.17
130	3.50	13.70				
125	2.2263	1.59	3.907	1.67
120	4.50	15.70				
115	1.7555	1.20	2.949	1.26
110	5.70	18.00				

The values of L are calculated from Regnault's formula for the latent heat of steam, viz. $L = 1082 - .695T$; thus for the temperature 175° , we find $L = 1082 - .695 \times 175 = 960.375$; and so on for other temperatures. The near coincidence of the results in the sixth and eighth columns shows that the cooling effect of the evaporation of water nearly varies as the product of the tension of the vapour and the latent heat of that vapour. This result was also verified in the following manner.

Experiment II. Two tin canisters were made of the same form and capacity; but the one was closed at the top, as in the foregoing experiments, whilst the other was open at the top. They were filled with the same quantity of hot water and exposed to the atmosphere, and the times of cooling, &c., were noted as before. In this case, presuming the radiation from the surface of the water to be the same as the radiation from the corresponding surface of the tin, the difference between the corresponding rates of cooling obviously gives us the rate of cooling due to evaporation alone. In this experiment the observations were made at intervals of 5° Centigrade.

TABLE of results of Experiments giving the rates of the cooling effect of Evaporation.

Temperature, in degrees Cent.	Canister open at top.		Canister closed at top.		Rate of cooling due to evaporation, v .	Corresponding tension of vapour, p .	Value of v by formula $v = \frac{p \times L}{14000}$.
	Intervals of time, in minutes, for 5° C.	Rate of cooling per minute, in degree Cent.	Intervals of time, in minutes, for 5° C.	Rate of cooling per minute, in degree Cent.			
65°	0	0
62·5	·926	·469	·457	6·533	·458
60	5·40	10·66
57·5	·757	·384	·373	5·208	·367
55	6·60	13·00
52·5	·612	·316	·296	4·094	·291
50	8·16	15·83
47·5	·460	·230	·230	3·191	·228
45	10·85	19·50
42·5	·379	·213	·166	2·450	·176
40	13·17	23·41

The values of L are taken in degrees Fahrenheit, as in the foregoing Table of results. The near coincidence of the results in the sixth and eighth columns confirms the law above enunciated.

The following series of experiments not only verify this law, but also determine the maximum depression of temperature produced by the evaporation of water in dry air of a given temperature.

Experiment III. A large wide-mouthed bottle, containing a portion of strong sulphuric acid, had its mouth closed by a perforated cork having the stem of a delicate thermometer passing through it. The thermometer was graduated into tenths of a degree Centigrade; and having an elongated bulb, a calico cap could be readily slipped on or taken off the bulb as required. By agitating the sulphuric acid, the air in the bottle was dried or deprived of all watery vapour. The temperature of the air being first ascertained by the naked bulb of the thermometer, the bulb of the thermometer with its calico cap moistened with water slightly above the atmospheric temperature was placed in the bottle, and the rate of cooling, &c., were carefully noted. The rate of cooling, given in the second column of the following Table of results, is the number of degrees, or parts of a degree, which the thermometer fell per minute below the atmospheric temperature.

TABLE of results of Experiments, giving the rates of cooling, &c. produced by the Evaporation of Water in a dry Air at different Temperatures.

Temperature, in degrees Cent., T.	Ratio of cooling per minute, in degrees Cent., v.	Maximum depression, in degrees Cent. D.	Corresponding minimum temperature, T ₁ .	Corresponding tension of vapour at T temp., p.	Corresponding tension of vapour at T ₁ temp., p ₁ .	Value of v by formula v=3·5p.	Value of D by formula (1).
1	0·67	4·5	-3·5	·194	·133	0·68	4·50
5·2	0·85	5·5	-0·3	·259	·178	0·90	5·77
10	1·25	6·8	3·2	·361	·227	1·26	7·06
14	1·63	8·0	6·0	·467	·274	1·63	8·22
17	2·00	9·0	8·0	·566	·315	1·98	9·20
24·1	3·00	11·5	12·6	·868	·426	3·04	11·50

Here the near coincidence of the results given in the second and seventh columns shows that *the rate of cooling, by the evaporation of water, varies as the tension of vapour at the given temperature*, the comparatively small differences of the latent heat in this case being neglected. Again, the near coincidence of the results given in the third and eighth columns shows that formula (1) very nearly represents the maximum depression. This formula is derived from theoretical reasoning, in the following manner.

At the point of maximum depression, the rate of cooling by evaporation must be equal to the rate of heating due to radiation. Now the rate of cooling varies as p_1 , the tension of vapour at the corresponding temperature; but by Dulong and Petit's formula the rate of radiation varies as $1\cdot0077^T (1\cdot0077^D - 1)$; therefore at the point of maximum depression we have

$$1\cdot0077^T (1\cdot0077^D - 1) = ap_1,$$

where a is a constant, which we find by elimination to be $\cdot265$. Whence we find

$$D = \frac{1}{\log 1\cdot0077} \log \left(\frac{ap_1}{1\cdot0077^T} + 1 \right) = 300 \log \left(\frac{\cdot265p_1}{1\cdot0077^T} + 1 \right). \quad (1)$$

An approximate value of D may be found in the following manner. Putting $1+e$ for $1\cdot0077$, we find by development, and neglecting the higher powers of e , $(1+e)^D - 1 = eD$ nearly; hence we have $a_1^T \times eD = ap_1$;

$$\therefore D = \frac{ap_1}{ea_1^T} = \frac{34\cdot274p_1}{1\cdot01^T} \dots \dots (2)$$

Experiment IV. When the temperature of the air was 12°C ., the evaporation of ether caused the thermometer to fall at the

rate of $6\cdot85$ per minute; whilst that of water, in *dry* air at the same temperature, produced a fall of $1\cdot33$ per minute.

Hence we have for the ratio of the velocities $\frac{V}{v} = \frac{6\cdot85}{1\cdot33} = 5\cdot15$.

Now the tension P of the vapour of ether at 12° is $12\cdot243$, and that of water at the same temperature is $p = \cdot4104$. Moreover, taking the latent heat of the vapours of ether and water for equal weights to be 168 or L , and 1000 or l respectively, we have

$$\frac{P \times L}{p \times l} = \frac{12\cdot243 \times 168}{\cdot4104 \times 1000} = 5\cdot01,$$

showing that $\frac{V}{v} = \frac{P \times L}{p \times l}$ very nearly.

Again, at the temperature of 5° C., it was found that $V = 5^\circ$, and $v = 0\cdot85$;

$$\therefore \frac{V}{v} = \frac{5}{\cdot85} = 6 \text{ nearly, and } \frac{P \times L}{p \times l} = \frac{9\cdot058 \times 168}{\cdot256 \times 1000} = 6.$$

Other things being the same, it may be presumed that the cooling effect of evaporation varies inversely as the density or pressure of the air.

Heating Effects produced by the Absorption of Vapour by Dry Substances, &c. from an Atmosphere saturated with Vapour.

2. As evaporation produces cold, so the condensation of vapour, from whatever cause, produces heat. The force with which dry substances (such as woollen, cotton, and other textile fabrics) attract and condense the vapour of water is so great as, under favourable circumstances, to raise the temperature of a thermometer 30° F. Acting in this manner, I have found that the heating power of dry woollen cloth is as great as any other substance, being about the same as that of fused chloride of calcium. The heat developed not only depends on the quantity of the substance used, but also on the temperature of the air at the time, as shown by the following experiments.

The heat produced by the absorption of vapour, by dry substances, is proportional to the tension of the vapour.

Experiment V. A double roll of woollen cloth was fitted as a cap to the bulb of the thermometer before described, and inserted in the sulphuric-acid bottle described in Experiment III., where it was allowed to remain for some hours. The cork of this thermometer fitted the mouth of a large humid-air bottle containing fifteen half-pints, in which a portion of water was placed. By occasionally agitating the water of this bottle and allowing it to

stand closed for some hours, the air in it became fully saturated with the vapour of the water at the given temperature. The thermometer with its dry woollen cap was then placed in the humid-air bottle, and the rates of augmentation of temperature, &c., were duly observed.

TABLE of results of Experiment. Heating effects of dry Woollen Cloth placed in Air saturated with the vapour of Water.

Temperature, in degrees Cent., T.	Rate of augmentation of temperature per minute, v.	Maximum augmentation of temperature, E.	Corresponding tension of vapour at T temperature, p.	Value of v by formula $v=4.3p$.	Value of E by formula $E=5.15 \times 1.0512^T$.
5	0.9	5.7	.208	0.89	5.6
7	1.2	7.3	.295	1.24	7.27
11.6	1.8	9.2	.410	1.76	9.19
15	2.0	10.8	.497	2.13	10.89
20	3.0	14.0	.686	2.95	13.98

Here the near coincidence of the results given in the second and fifth columns shows that *the rate of augmentation of temperature is proportional to the tension of the vapour at the given temperature*. Also, the near coincidence of the results given in the third and sixth columns shows that, within this range of temperature, *the maximum augmentation of temperature is in the geometrical ratio of the temperature*. This remarkable result admits of the following theoretical exposition.

Assuming, in accordance with the principles derived from the foregoing experimental results, $E = \frac{ap}{a_1 T}$; then, within certain limits, $p = p_0 k^T$, where p_0 = the tension of vapour at 0° , and $k = 1.0686$, the ratio of the successive values of the tension of vapour for increments of 1° ; hence we find $E = ap_0 \left(\frac{k}{a_1}\right)^T = ce^T$; whence, by eliminating the constants c and e , we find $E = 5.15 \times 1.0512^T$.

3. The maximum augmentation of temperature was found to be affected by the thickness of the material; thus a single fold of woollen cloth at the temperature of 11° gave an augmentation of only 6° , whilst the double fold gave 9° ; again, the single fold at the temperature of 20° gave an augmentation of 9° , whilst the double fold gave 14° . This double fold, for ordinary temperatures, was found to give the maximum effect. Also a single fold of calico at 14° gave an augmentation of 4.6° , whilst the double fold gave 5.8° .

4. As the ratio $\frac{p}{v}$ is constant for all temperatures, a thermo-

meter with a *dry* cap may be advantageously used as a hygrometer. Putting v' for the velocity of augmentation per minute, when the thermometer with its *dry* cap is exposed to air whose vapour has the tension p' , then $p' = av'$, where the constant $a = \frac{p}{v}$ = the ratio of the velocity of augmentation, in air saturated with vapour, to the tension of that vapour.

5. The same principle was found to hold true in relation to certain hydrated salts, and other compounds having an affinity for watery vapour.

Experiment VI. In these experiments the salt was deposited on the bulb of the thermometer by repeatedly dipping it in a hot saturated solution of the salt and then placing the thermometer in the *dry*-air bottle, where it was allowed to remain until the salt was thoroughly dried. The bulb with the *dry* salt upon it was then placed in the humid-air bottle, and the velocity of augmentation of temperature per minute, as well as the maximum augmentation, &c., were observed as before. As in the case of the woollen cloth cap, the maximum effect depends on the quantity of the salt deposited on the bulb; but by using the same quantity in each series of experiments, the results admit of being compared with one another.

The results of experiment with chloride of calcium were as follows:—

At the temperature of 19° C., the rate of augmentation per minute v was found to be 4° , and the maximum augmentation E was found to be $13^{\circ}\cdot 3$. The temperature being variable, the rate of augmentation was found to be expressed by the formula $v = 6\frac{1}{4}p$, and the maximum augmentation by

$$E = \frac{25\cdot 3p}{1\cdot 011^T} = 4\cdot 6 \times 1\cdot 057^T.$$

The maximum augmentation, in degrees Centigrade, of the following substances was found at mean temperature (15° C.) as follows:—

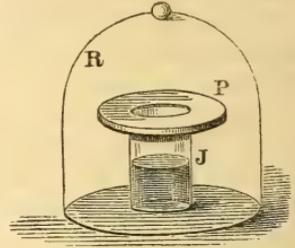
Potassa, 10° ; chloride of zinc, $7^{\circ}\cdot 4$; nitrate of lime, 7° ; sulphuric acid, $12^{\circ}\cdot 6$; oxalic acid, 6° ; sulphate of copper, 6° ; chloride of sodium, 5° ; carbonate of soda, 5° ; charcoal, $4^{\circ}\cdot 6$; sulphate of soda, $4^{\circ}\cdot 5$; nitrate of ammonia, $4^{\circ}\cdot 4$; oxalate of lime, $4^{\circ}\cdot 3$; sugar, 3° ; plaster of paris, 3° ; starch, 2° ; tartaric acid, 2° . These numbers probably express the relative force with which the respective substances attract watery vapour.

Charcoal in ammoniacal vapour gave an augmentation of 22° C., or $39^{\circ}\cdot 4$ F.; and plaster of paris in the same vapour gave 20° C., or 36° F.

On the Amount of Vapour absorbed at different Temperatures.

6. *The weight of vapour absorbed by an absorbent in a given time from an atmosphere charged with watery vapour is proportional to the tension of the vapour.*

Experiment VII. A jar (J), 5 inches section, containing strong sulphuric acid, was covered with a plate P, having a portion cut out, of the same section as the interior of the jar; a damp cloth was laid on this plate, and the whole was covered by a large receiver (R) standing on a tray containing water. After remaining for twelve hours at a uniform temperature, the augmentation of weight of the jar and acid was ascertained, which gave the weight of the vapour absorbed by the acid in that time at the observed temperature. The following Table gives the augmentations of weight corresponding to different temperatures, other things being the same.



Temperature, in degrees Cent., T.	Corresponding weight of vapour absorbed, in grains, w.	Corresponding tension of vapour at T, p.	Value of w by formula $w = 38p$.
6.5	10.4	.284	10.78
7.5	11.2	.304	11.54
12.5	16.2	.423	16.06
13.3	17.4	.447	17.00
18.0	24.8	.603	24.90

The near coincidence of the results given in the second and fourth columns shows that *the weight of vapour absorbed by the acid, in a given time, is nearly proportional to the tension of the vapour at the given temperature.*

At first sight it may seem that the amount of vapour here diffused is very small; but if we convert the weight of water absorbed into watery vapour at the given temperature, we shall find that the volume of vapour is really something considerable. Thus we find that 1 grain of water at 12°·5 produces about 450 cubic inches of vapour, so that the volume of vapour, at this temperature, diffused per hour, from a unit of surface through a unit of distance, would be about 120 cubic inches.

7. In like manner, it was found that a *dry* clean woollen cloth, placed in an atmosphere saturated with watery vapour, absorbed in equal times a weight of moisture proportional to

the tension of the vapour. But it was found that the maximum weight of moisture absorbed was the same, or very nearly the same, for all temperatures or tensions of vapour. Thus a piece of clean woollen cloth, weighing 398 grains when perfectly dry, absorbed 92·5 grains of moisture from an atmosphere saturated with vapour at 15° C.; whereas at 5° C. the weight of moisture absorbed was 91·4 grains. Moreover, it was found that, when this cloth was exposed to the air, the weight of moisture absorbed (approximately) varied directly as the tension of the vapour in the air at the time, and inversely as the tension of the vapour

of saturation, as expressed in the formula $w = 92 \frac{p'}{p}$. Thus, when the temperature of the air was 9° C., and the tension of its vapour, as determined by Daniell's hygrometer, was ·26, the weight w of the moisture absorbed by the cloth was found to be 70·6 grains; but by the formula we find $w = 92 \times \frac{.26}{.337} = 70·9$.

Again, when the temperature was 14°·8 C., and the tension of the vapour p' was ·31, the weight w was found to be 59 grains;

but by the formula $w = 92 \times \frac{.31}{.49} = 58·2$. When $p' = p$, then $w = 92$, which is the weight of the maximum absorption, as before given. Hence it would appear that a piece of clean woollen cloth might be used as a hygrometer, probably giving nearly as reliable results as the ordinary form of the wet- and dry-bulb thermometers. Thus we should have $p' = \frac{wp}{a}$, the constant a being the maximum amount of absorption of the piece of cloth employed.

On the Diffusion of Vapour in the Air.

8. *The diffusion of vapour from a damp surface through a variable column of air, varies (approximately) in the inverse ratio of the depth of the column, the temperature being constant.*

Experiment VIII.—In this case Experiment VII. was repeated at constant temperature, 14° C., but with a variable depth of the acid from the surface of the plate D.

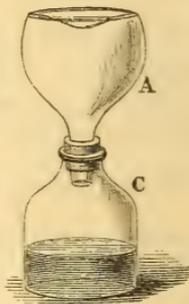
Depth of the acid from the damp surface, in inches, D.	Corresponding weight of vapour absorbed, in grains, w .	Value of w by formula $w = \frac{21}{D}$.
1	21·0	21·0
2	10·4	10·5
3	6·6	7·0
3½	5·8	6·0

Here the near coincidence of the results given in the second and third columns, shows that the weight of vapour diffused through the different columns of air in the same time is very nearly inversely as the depths of these columns. In this respect the law of diffusion of vapour through air is similar to the diffusion of a liquid through the pores of an absorbent.

It has been shown, the atmospheric column being constant and the temperature variable, that the amount of vapour diffused is proportional to the tension of the vapour at that particular temperature; hence we conclude that *the amount of vapour diffused varies directly as the tension of the vapour at the given temperature, and inversely as the depth of the column of air through which the vapour has to pass.*

9. *The time in which a given volume of dry air becomes saturated with vapour (or saturated within a given relative degree) is nearly independent of the temperature, the source of vapour being constant.*

Experiment IX.—A large bottle, containing fifteen half-pints, was provided with a thermometer, as in Experiment III. The air in the bottle was dried in the following manner:—The bottle A, containing the air to be dried, was provided with a cork fitting the mouth of the bottle C, containing a considerable portion of strong sulphuric acid, as shown in the diagram. The apparatus remained in this state for several hours, the acid being occasionally agitated to secure the absorption of all the watery vapour in the air. The thermometer, with a damp calico cap upon its bulb, was then placed in this dry-air bottle, and the times corresponding to different points of depression were duly observed. After the maximum point of depression had been attained, the thermometer gradually rose until the air in the bottle became saturated with vapour, and then the temperature became the same as the surrounding air. It is presumed that the temperature indicated by the thermometer in the bottle, towards the close of the experiment, corresponds with the rate of evaporation going on from the damp calico, and therefore becomes a measure of the degree of saturation of the air in the bottle at the instant of observation. The following are the results of two experiments at the temperatures of 7° and 13° C.



At 7° the maximum depression was found to be $3^{\circ}\cdot 2$, the corresponding time being 8.5 minutes; the times corresponding to $0^{\circ}\cdot 9$ and $0^{\circ}\cdot 6$ below the temperature of saturation were found to be 70 and 90 minutes respectively.

At 13° the maximum depression was found to be 5° , the

corresponding time being 7.5 minutes ; the times corresponding to 0.9 and 0.6 below the temperature of saturation were found to be 73 and 92 minutes respectively.

Here it will be observed that the times for the same distance from the point of saturation nearly coincide with each other. It will be further observed that the maximum depressions are pretty nearly as the tensions of the vapour at the respective temperatures.

The rationale of this result would appear to be this : whilst the rate of diffusion increases as the tension of the vapour at the particular temperature, at the same time the amount of vapour to be diffused also increases according to the same law. Assuming that the rate of evaporation, v , varies as $p-p'$, we have

$$\frac{dp'}{dT} = v = a(p-p'); \therefore T = q \log \frac{p}{p-p'} = q \log \frac{1}{1-\frac{p'}{p}}$$

Now if $\frac{p'}{p} = a$ constant, then $T = a$ constant ; that is, the times of diffusion are equal for equal degrees of saturation. When $p' = p$, $T = \infty$; that is, theoretically the time of complete saturation is infinite ; but practically the time requisite to saturate the air within a small limit is definite.

10. *The times in which different volumes of dry air become saturated with watery vapour (or saturated within a given relative degree) are nearly proportional to the volumes.*

Experiment X.—The foregoing experiment was performed with a bottle containing three half-pints ; that is, in this case the volume of the air was one-fifth of the volume of the air in the last experiment.

The temperature being 13° C., the maximum depression was found to be 2°.3, the time being 4 minutes, and the time corresponding to 0.6 below the temperature of saturation was found to be 18 minutes, whereas for five times the volume of air it was found to be 92 minutes, that is, nearly five times the time.

If water be placed at the bottom of a deep open jar, without soiling the sides, the humidity of the air within the jar, after it has stood for several hours, is nearly uniform, and only slightly in excess of that of the external air.

Experiment XI.—The jar used in this experiment was 18 inches in depth and $4\frac{3}{4}$ inches in diameter. After the jar had stood, with the water covering its bottom, for three hours, the temperatures at the bottom, at the middle, and at the top of the jar were found, by means of a thermometer with the naked bulb, to be 13°.6, 13°.6, and 13°.5 C. respectively. The wet-

bulb thermometer was then inserted, and the depressions were found as follows:—at the top $2^{\circ}\cdot4$, at the middle $2^{\circ}\cdot4$, and at the bottom $2^{\circ}\cdot2$. Hence by Glaisher's factors we find the differences between the temperature of the air and the temperatures at the dew-points to be $4^{\circ}\cdot6$, $4^{\circ}\cdot6$, and $4^{\circ}\cdot2$ respectively.

A cover being placed upon the jar, it was allowed to stand for four hours, and then it was found that the whole of the air in the jar had become saturated with vapour; for then the wet-bulb thermometer indicated the same temperature as the naked bulb. The cover was then taken off, and after the lapse of 15 minutes it was found that nearly the whole of the vapour in the jar had become diffused in the atmosphere in that time; showing that *vapour already formed diffuses itself in the atmosphere much more rapidly than it is formed from the surface of the water*. In the former case no change of specific heat takes place, whereas in the latter case there is a great change of specific heat in the passage of the liquid to the state of vapour, which tends to retard the process of vaporization. These results explain the following phenomena: if the covered jar be left over night, a copious deposit of dew will be found on the sides of the jar; but, on the contrary, if the cover be off, no dew will be formed.

Hastings, March 2, 1863.

XLVI. *On the Motions of Camphor towards the Light, and on Variations in the Fixed Lines of the Solar Spectrum.* By JOHN WILLIAM DRAPER, *Professor of Chemistry and Physiology in the University of New York.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT has so happened that I have not seen the February Number of the *Philosophical Magazine* until today.

I regret to find that Mr. Tomlinson has not frankly done me the justice which was due, as respects the explanation of the motions of camphor to the light, but has endeavoured to strengthen his case by, as I trust, an unconscious misstatement of dates.

Mr. Tomlinson admits that I published in the *Philosophical Magazine* for February 1840, the true explanation of these motions—that camphor crystallizes on the side of a glass vessel nearest the sun because that side is the coldest. He endeavours, however, to show that I had so little faith in this explanation, as to publish in 1844 other experiments to prove that it is incorrect.

But Mr. Tomlinson knows better than this. He sees on page 99 of the Appendix of the book he has quoted, and in the fourth line from the top of the page, that the experiments he refers to, and the paragraphs he quotes, were published, not in 1844, as he would have his readers suppose, but in 1837, in the Journal of the Franklin Institute of Philadelphia for June, July, August, and September of that year. They are not *later* contradictions of the true explanation given in 1840, but the *earlier* steps by which that conclusion was gradually reached. They were published, not four years *subsequently*, but three years *previously* to the correct explanation.

I have made no new experiments on these camphor-motions since 1840, knowing that the explanation I then gave is the true and final one.

In 1844 I collected together and reprinted several of the papers I had published during the preceding ten years in English and American journals, those in the Philosophical Magazine among the rest.

Having been drawn into this camphor-controversy very needlessly and very reluctantly, you, Gentlemen, will perhaps accord me the privilege of the space of a few additional lines to ask the attention of your readers to another topic.

In the same Number of the Philosophical Magazine in which Mr. Tomlinson's original communication occurs, November 1862, p. 407, there is a brief account of the recent discoveries of Dr. A. Weiss on the lines of the solar spectrum, to the effect that he had observed in Greece, during the preceding year, the Fraunhofer lines at sunrise and sunset, and had remarked their increase of number and their condensation. Dr. Weiss points out the advantages of the Ionian Islands for these observations, remarking that the dense layers of vapour observed almost every day on the coasts of Africa and America, there scarcely disturb the observer. He also refers to his previous publication on changes of the breadth of these lines at sunset, especially in the red and yellow rays, and to the explanation he has given of their dislocation by lateral absorption, Phil. Mag. July 1861, p. 80. With that explanation I am not at present interested.

But I wish to relieve the atmosphere of America from the reproach here unfairly put upon it, since it was in it, and by myself, that the fact in question was many years ago remarked. If your readers will turn to the Philosophical Magazine for May 1843, p. 361, they will find the following paragraph, in connexion with the photographic depicting of Fraunhofer's lines :—

“ Before proceeding to the description of the mode which is to be followed, and of the characters of the lines themselves, I can-

not avoid calling attention to the remarkable circumstance which has frequently presented itself to me, of a great change in the *relative visibility* of Fraunhofer's lines when seen at different periods. There are times at which the strong lines seen in the red ray are so feeble that the eye can barely catch them; and then again they come out as dark as though marked in India ink on the paper. During these changes the other lines may or may not undergo corresponding variations. The same observation equally applies to the blue and yellow rays. It has seemed to me that the lines in the red are more visible as the sun approaches the horizon, and those at the more refrangible end of the spectrum are obvious in the middle of the day."

The appearance of the spectrum, as well as its chemical action, varies with the hour of the day. MM. Favre and Silbermann, in their memoir "On the Quantities of Heat disengaged in Chemical and Molecular Actions," published in the *Annales de Chimie*, 1853, vol. xxxvii. p. 500, have given a projection of three curves furnished by their own observations, and one derived from mine. From the discussion of these results they deduce that there is a connexion between the absorptive action of the atmosphere, due to vapour contained in it in greater quantity in the afternoon, and the chemical power of the different spectrum-rays. They suggest that my experiments must have been made after midday, as was in fact the case.

I intended to add some remarks respecting the so-called recent discovery of the variations of the spectrum and its lines when substances are burnt at different temperatures, but, fearing that you will grow tired of these reclamations, will content myself with the hope that those of your readers who are occupying themselves with the spectroscope will not think their time wasted if they consult the pages of old Numbers of the Philosophical Magazine.

Yours truly,

University, New York,
March 26, 1863.

JOHN W. DRAPER.

XLVII. *On the Polarization of Light by Rough and White Surfaces.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S.*

THE laws of the polarization of light when reflected from the surfaces of solids and fluids, and when refracted and transmitted by translucent and transparent bodies, have been successfully investigated; but no experiments, I believe, have been made on the polarization of light by rough or unpolished surfaces, such as ground glass, painted surfaces, pounded glass,

* From the Transactions of the Royal Society of Edinburgh, vol. xxiii. part 2. Communicated by the Author.

snow, white powders, and solids and fluids reflecting white light from their interior. When studying the polarization of the atmosphere, and anxious to discover the cause of its partial polarization, and of the three neutral points or spots in which there is no polarization, I investigated the action of rough surfaces upon light, under the conviction that the sky or atmosphere was a rough surface like any aggregation of white or coloured particles. Had the atmosphere been specular, like water or any body with a polished surface, the image of the sun would have been seen in it by reflexion; but being composed of ærial and aqueous molecules, it must reflect the sun's rays like pounded glass, or any white or coloured powders.

The results of this inquiry, which I now submit to the Society, are such as I anticipated, and afford an explanation not only of the partial polarization produced by the atmosphere, but of each of the three neutral points, which, it will be shown, can be produced artificially by the combination of rays polarized by the reflexion and refraction of any rough or molecular surface.

The experiments by which these results were obtained were made chiefly with rectangular plates of glass, 9 inches by 7, of various degrees of roughness, some with only one side, and others with both sides rough. These plates, some of which are now on the table, were made at the Smethwick Glass Works, near Birmingham, and were kindly presented to me by Messrs. Chance and Brothers, the proprietors of that great establishment.

The angle of complete polarization for light reflected from the polished surface of this glass is about $56\frac{1}{2}^{\circ}$, and the polarization is a maximum at this angle, diminishing on one side to 0° of incidence, and on the other to 90° . When the light thus polarized is examined by the band polariscope, the bands are nowhere interrupted, and therefore there can be no neutral point, the polarization of the bands being everywhere positive or vertical.

In order to observe the effect of a single rough surface, such as that of glass ground with the finest emery, I blackened with melted wax the polished surface of a plate which was ground only on one side, in order to prevent the light scattered by the ground surface from being reflected at the second surface. When this single rough surface reflected the light of a gas-flame, it polarized it almost completely at nearly the polarizing angle of $56\frac{1}{2}^{\circ}$; and there is no interruption or neutral point in the bands of the polariscope. The rotation, therefore, or measure of polarization, is nearly 45° .

If this single rough surface is placed at an open window when the sun is not shining, and reflects the light of the sky or clouds, the light is only partially polarized, and the degree of polarization R is only 19° . In the open air it is much less. Within

the room the polarization increases as the glass recedes from the window. In the open air it is a maximum when the plane of reflexion passes through the sun and the observer. The rotation is then 15° , and it diminishes with the distance of the plane of reflexion from the plane passing through the sun and the observer*.

When the second or polished surface is not blackened, it reflects the light scattered by the ground surface, and the maximum polarization of the rough surface is greatly diminished. The rotation is 16° with a gas-burner very near, and 20° when the flame is still nearer. The polarization is diminished by holding a lighted candle on one side of the plane of reflexion.

With a similar plate having a rougher surface, the degree of the polarization of the sun's rays directly reflected was $16\frac{1}{2}^\circ$ close to a south window. Before noon the angle of maximum polarization was 44° , and at noon, with a brighter sun, it was 40° .

When the whole light of the sky fell upon the rough surface MN, the rotation was 21° . When AB was covered up, the rotation was diminished; and when CD was covered up, the rotation was increased.

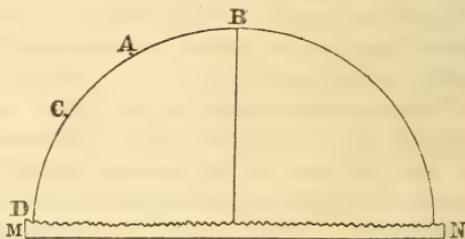
When both the surfaces of the plate of glass are rough, the polarization is a maximum, at an angle greater than the normal polarizing angle, or $56\frac{1}{2}^\circ$, and the degree of polarization is about 20° .

In making similar experiments with ivory, bone, porcelain, white or coloured surfaces painted in oil, paper, parchment, silk, linen, and cotton cloths, milk, flour, and other white powders, &c., I found that the polarization was partial in all of them, the normal or complete polarization being reduced by its combination with the oppositely polarized rays produced by refraction.

In all these experiments the partial polarization was positive or vertical from an incidence of 90° up to an incidence of 0° , as far as it could be observed, owing to the impossibility of examining the bands where they were reflected at or near a perpendicular incidence. In order to meet this difficulty, I adopted the following mode of observation:—

* The roughness of the glass surface used in the preceding experiments is such, that a gas-flame, distant $5\frac{1}{2}$ feet, ceases to be visible at an angle of incidence of $79^\circ 50'$.

Fig. 1.



The rough or white surface MN being placed vertically, was illuminated with the flame of a gas-burner, or a moderator lamp, placed at F . The observer at E , or in any direction between F and N , observes the condition of the bands of the polariscope when they are placed parallel to MN .

If we now take a single rough surface of plate-glass *blackened* on its polished side and place it at MN , we shall observe a

neutral point about H , the polariscope-bands being *negative* or *horizontal* with a *white* centre on the M side of H , and *positive* or *vertical* with a black centre on the N side of H . This neutral point is obviously produced by the equal and opposite action of light polarized by reflexion and refraction. As these two lights, proceeding from the light incident at H , proceed from F , they cannot reach the eye at E by the ordinary law of reflexion. They are portions, therefore, of oppositely polarized rays scattered in every direction by the rough surface of the glass.

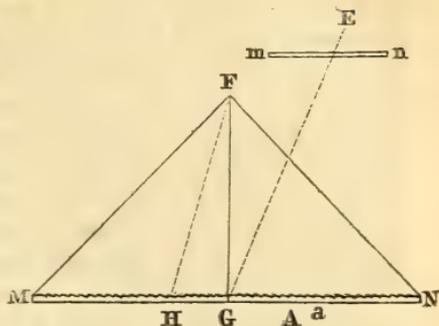
If we take the same plate of glass with its surface *not blackened* and place it at MN , we shall find the neutral point at G .

If we invert this plate, so that its polished side is uppermost, the neutral point is advanced to A , all the bands on the M side of A being, as before, *negative*, and those on the N side of A *positive*—a result proving that the intensity of the negative bands had been *increased*, and the positive ones *diminished*.

Conceiving that this effect was produced by the light scattered by the rough surface being polarized *negatively* by the refraction of the polished surface, I took the plate with the blackened side and laid upon its surface a plate of transparent glass, in order to imitate the action of the unpolished surface in the preceding experiment. The neutral point which, by the action of the single rough surface, was at H , was now advanced to A by the refractive polarization of the two surfaces of the transparent plate. When this plate was placed at mn parallel to MN , the very same effect was produced. By inclining mn , the neutral point was advanced from A to a , still further by increasing the inclination, and still further by using several plates, the plane of refraction or incidence being parallel to a plane passing through FGA .

If we turn the plate or plates at mn round 90° , keeping the same inclination to the incident rays from MN , the neutral

Fig. 2.



points will return to their respective places *a*, A, G, H, the refractive polarization of the plate or plates reducing the negative bands between M and the neutral points, and increasing the positive bands between the neutral points and N.

Hence we have a method of determining whether the polarization of the bands is positive or negative, when they are so faint or indistinct that we cannot see whether the central band is black or white. When they become *less intense*, or perhaps disappear, by viewing them through one or more plates of glass at *m n*, having their planes of reflexion parallel to the plane passing through FA, they are *positive*. When they become *more intense* they are *negative*.

The place of any neutral point may be advanced from H to G, or from G to A and *a*, by placing a sheet of *white* paper behind the rough-surfaced plate MN, or by one or more plates of glass with rough-surfaces, each plate advancing it further. A sheet of paper advances it further than *four* surfaces of ground glass. The rays scattered by the paper are negatively polarized by refraction at their emergence from the plate MN; and the increased intensity of the negative bands shifts the neutral point towards N.

We have already seen that a single rough surface of glass blackened behind has its neutral point about H, and when not blackened, about G. A plate with both sides rough carries the neutral point towards A, and two or more such plates still further. The smoothest ground plates carry the neutral point further than the roughest towards M.

In the prosecution of this subject I submitted to examination the following substances:—

Calcareous spar, ground.	Linen cloth, white.	Swan's down.
Marble, white.	Cotton cloth, white.	Snow.
Painted board.	Cashmere, white.	Pearl-oyster shell.
Ivory.	Paper of all colours.	Pounded sugar.
Ivory, artificial.	Parchment.	Pounded glass.
Bone.	White kid leather.	Rochelle salts.
Porcelain.	Pith of the sola.	Soda.
White fir wood, planed.	Rice paper.	Magnesia.
Yellow fir wood, planed.	Cotton wool.	Milk.
Silk, white.	Ermine.	White shell.
Satin, white.		

In all these substances I found a neutral point in the mixture of the positively and negatively polarized rays which they reflect. In many of them the neutral point was on the A side of G, or when the angle of incidence was less than 90° ; while in others it was on the H side, or when the angle of incidence was negative and greater than 0° , but on the other side of the perpendicular FG.

As in ground glass, one or more plates of glass inclined to the reflected ray, as previously described, brought the neutral point in almost all these substances to the A or N side of G.

When the substance was more or less glazed, the neutral point was on the A side of G, the glazed surface acting exactly like the polished surface of a plate of ground-glass when placed next the light. This effect is finely seen in milk, where the fluid surface increases the negative or refracted light as in the glass plate.

I attempted to determine for several of these substances the angle of maximum polarization, the intensity of the partial polarization, and the place of the neutral point; but I found it very difficult, owing to the magnitude of the flame which was necessary to show the bands when very faint, and to its proximity to the reflecting surface, which was necessary for the same purpose.

In white unglazed *paper*, for example, in the sun's light on the 1st of February 1841, at 11^h A.M., the polarizing angle was about 71° , and the degree of polarization, or $R = 18\frac{1}{4}^\circ$. On the 2nd of February the polarizing angle was $69\frac{1}{2}^\circ$, and $R = 18\frac{1}{4}^\circ$ at 10^h 40^m A.M.

In almost all the substances which I have examined, I have observed a neutral point only at angles of incidence below the maximum polarizing angle, and beyond G, fig. 2; but it is obvious that there must be another at some angle above the maximum polarizing angle, excepting in substances where the light polarized by refraction is too feeble to neutralize the light polarized by reflexion. I have observed this second neutral point only in one case; but with a sufficiently strong light it will doubtless be seen in many substances.

In all the preceding experiments, the substances employed have been opaque, or with such rough surfaces that objects cannot be seen through them. I was therefore desirous of ascertaining if neutral points were produced when the surfaces which polarize the light were perfectly transparent, having some analogy with the strata of the atmosphere of different densities. With this view I used a pile of transparent plates with twenty surfaces, and I found a neutral point distinctly visible, both above and below the angle of complete polarization. I obtained the same result at an angle less than that of complete polarization, with a large plate of mica split by an intense heat into so many films that it had the metallic lustre of silver.

The results now submitted to the Society could hardly have been anticipated from theoretical considerations. The laws of polarization for light normally reflected and refracted by polished surfaces, are not applicable to those which are rough, or

to bodies which reflect light from their interior; and in the case of piles of polished glass plates, the most distinguished philosophers, Arago, Young, and Sir John Herschel, would have considered the light which the plates reflected, as they did that which they transmitted, as consisting of polarized light, accompanied with a portion of common light, a combination incapable of producing neutral points.

The most important application of the preceding experiments is to the polarization of the atmosphere. Arago, Babinet, and others, in their theory of the neutral points, and of the partial polarization of the atmosphere, took no account of the rays which are polarized by refraction whenever light is polarized by reflexion, and they referred these abnormal phenomena to a horizontal secondary reflexion from the atmosphere itself, modifying, and, in three neutral points, extinguishing the light polarized by reflexion. Whether or not such a secondary reflexion exists, or is adequate, if it does, to account for these phenomena, are questions which will be considered in another paper on the polarization of the atmosphere. But however ingenious may be the hypothesis, it has no support either from experiment or observation. The reduction of complete to partial polarization, by the opposite action of light polarized by reflexion and light polarized by refraction, and the production of neutral points where these two lights are equal, whenever light is incident on surfaces which, like the atmosphere, disperse and polarize it, is next to an ocular proof of the true laws of atmospherical polarization.

It is not one of the least wonders of terrestrial physics, that the blue atmosphere which overhangs us exhibits, in the light which it polarizes, phenomena somewhat analogous to those of crystals with two axes of double refraction.

XLVIII. *On the Stereographic Projection of the Spherical Conic.*

By A. CAYLEY, Esq.*

IN order to the tolerable delineation of some figures relating to spherical geometry, I had occasion to consider the stereographic projection of the spherical conic. To fix the ideas, imagine a sphere having its centre in the plane of the paper, and through the centre three rectangular axes, that of x horizontal and that of y vertical, in the plane of the paper, and the axis of z perpendicular to and in front of the plane of the paper. The radius of the sphere is taken equal to unity (so that its intersection by the plane of the paper is the circle radius unity), and the points X , Y , and Z are taken to denote the

* Communicated by the Author.

points where the axes, drawn in the positive direction, meet the surface of the sphere ; and the opposite points are called $X', Y',$ and Z' . The eye is supposed to be at Z , and the projection to be made on the plane of the paper. This being so, and supposing that the axes of coordinates are the principal axes of the spherical conic, *the axis of x being the interior axis*, and taking ξ, η, ζ as the coordinates of a point on the spherical conic, its equations are

$$\begin{aligned} \xi^2 + \eta^2 + \zeta^2 &= 1, \\ -\xi^2 + \eta^2 \cot^2 \beta + \frac{\zeta^2}{c^2} &= 0; \end{aligned}$$

where it may be remarked that $\tan \beta, c$ are the semiaxes of the plane conic which is the gnomonic projection (*i. e.* the projection by lines through the centre of the sphere) of the spherical conic on the tangent plane at X or X' .

Taking, for a moment, x, y, z as the coordinates of a point on the projecting line (that is, the line through the eye to a point (ξ, η, ζ) on the spherical conic), the equation of this line is

$$\frac{x}{\xi} = \frac{y}{\eta} = \frac{z-1}{\zeta-1};$$

and thence putting $z=0$, x, y will be the coordinates of a point of the projection, and we have

$$\frac{x}{\xi} = \frac{y}{\eta} = \frac{1}{1-\zeta};$$

or, what is the same thing,

$$\xi = x(1-\zeta), \quad \eta = y(1-\zeta).$$

The equations of the spherical conic may be written

$$\begin{aligned} 1 - \zeta^2 &= \xi^2 + \eta^2, \\ \zeta^2 &= c^2(\xi^2 - \eta^2 \cot^2 \beta); \end{aligned}$$

and by eliminating ξ, η, ζ from the four equations, we obtain the equation of the conic.

Substituting for ξ and η their values, we find

$$\begin{aligned} 1 + \zeta &= (x^2 + y^2)(1 - \zeta), \\ \zeta^2 &= c^2(x^2 - y^2 \cot^2 \beta)(1 - \zeta)^2; \end{aligned}$$

or, observing that the first equation gives

$$\zeta = \frac{x^2 + y^2 - 1}{x^2 + y^2 + 1}$$

and that thence

$$1 - \zeta = \frac{2}{x^2 + y^2 + 1}, \quad \frac{\zeta}{1 - \zeta} = \frac{1}{2}(x^2 + y^2 - 1),$$

the equation is

$$(x^2 + y^2 - 1)^2 = 4c^2(x^2 - y^2 \cot^2 \beta).$$

It is now very easy to trace the curve. We see first that the curve is symmetrical with respect to the axes, and that it meets the axis of y in four imaginary points, but the axis of x in four real points, the coordinates whereof are

$$x = \pm (\sqrt{1 + c^2} \pm c),$$

so that the two points on the same side of the centre are the images one of the other in regard to the circle radius unity. Moreover the curve touches the lines

$$y = \pm x \tan \beta$$

at their intersections with the circle. By developing in regard to y , the equation becomes

$$y^4 + 2(x^2 - 1 + 2c^2 \cot^2 \beta)y^2 + (x^2 - 1)^2 - 4c^2x^2 = 0;$$

and putting

$$x = \pm (\sqrt{1 + c^2} \pm c),$$

the last term vanishes, and the equation gives $y^2 = 0$, or

$$\begin{aligned} y^2 &= 2(1 - x^2 - 2c^2 \cot^2 \beta) \\ &= 4(-c^2 \mp c \sqrt{1 + c^2} - c^2 \cot^2 \beta) \\ &= 4c(-c \operatorname{cosec}^2 \beta \mp \sqrt{1 + c^2}), \end{aligned}$$

the upper sign corresponding to the exterior values

$$\pm x = \sqrt{1 + c^2} + c,$$

and the lower sign to the interior values

$$\pm x = \sqrt{1 + c^2} - c.$$

In the former case the values of y are imaginary; in the latter case they are real if

$$\sqrt{1 + c^2} > c \operatorname{cosec}^2 \beta,$$

or, what is the same thing, if

$$\sin^2 \beta > \frac{c}{\sqrt{1 + c^2}};$$

that is, if, for a given value of c , β is sufficiently great, but otherwise they are imaginary.

If, as in the annexed figures, $c = \frac{5}{12}$

Fig. 1.

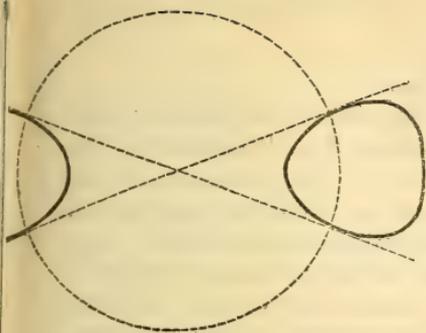
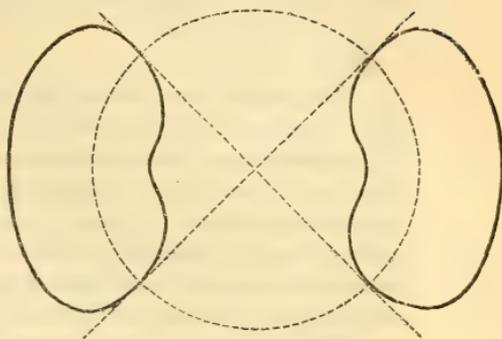


Fig. 2.



(and therefore $\sqrt{1+c^2} = \frac{13}{12}$, $\sqrt{1+c^2}+c = \frac{3}{2}$, $\sqrt{1+c^2}-c = \frac{2}{3}$),

then for the limiting value of β we have

$$\sin^2 \beta = \frac{5}{13} = \cdot 3846, \sin \beta = \cdot 62, \text{ or } \beta = 38^\circ \text{ nearly.}$$

In the first figure β is less, in the second figure greater than this value: the form for the limiting value is obvious from a comparison of the two figures.

I take the opportunity to mention the following theorem, which is perhaps known, but I have not met with it anywhere; viz. any three circles, each two of which meet, may be considered as the stereographic projections of three great circles of the sphere. In fact suppose, as above, that the projection is made on the plane of a great circle, and calling this the principal circle, the projection of any other great circle meets the principal circle at the extremities of a diameter of the principal circle. It follows that the theorem will be true, if, given any three circles each two of which meet, a circle can be drawn meeting the given circles, each of them at the extremities of a diameter of the circle so to be drawn. It is easy to see that the required circle has for its centre the radical centre (point of intersection of the radical axes) of the given circles, and that the radius is the 'Inner Potency' of the point in question in regard to each of the three given circles. In particular the three circles having for centres the vertices of an equilateral triangle, and the side for radius, may be considered as the stereographic projections of three great circles of a sphere. This is a very ready mode of delineation of a spherical figure depending on three great circles of the sphere.

2 Stone Buildings, W.C.,
March 21, 1863.

XLIX. *Reply to some remarks by G. Kirchhoff in his Paper "On the History of Spectrum Analysis."* By BALFOUR STEWART, M.A., F.R.S.

MY DEAR SIR DAVID BREWSTER,

IN the last Number of the Philosophical Magazine I have perused an article by Professor Kirchhoff, of Heidelberg, "On the History of Spectrum Analysis and of the Analysis of the Solar Atmosphere." In the course of his remarks the learned author has reviewed in a somewhat disparaging manner some researches of mine on radiant heat, in consequence of which I am forced to reply, although very unwillingly, and desiring much to avoid a scientific controversy, especially with Professor Kirchhoff as an opponent.

The experiments in confirmation of my views were made in the physical laboratory of the University of Edinburgh in the beginning of the year 1858, and my account of them was, in March of the same year, communicated to the Royal Society of Edinburgh by Principal Forbes, whose name is well known in connexion with radiant heat, and who gave me much valuable assistance and advice during the progress of these experiments.

I entertain a hope that you, as a distinguished office-bearer in both of these scientific bodies, will afford me the opportunity of briefly vindicating in your Journal the claims of these researches to a greater measure of completeness than the learned Heidelberg Professor seems disposed to allow.

It was the theoretical extension of Prevost's law of exchanges which first presented itself to my mind; nor did I omit to obtain the best possible experimental verification of my views, or to present this to men of science as the chief feature, grounding the theory upon the experiments, rather than deducing the experiments from the theory.

It was thus that dark heat came to form the subject of these investigations; for as the point to be proved was an extension of Prevost's theory, it was necessary to deal with rays which were universally acknowledged to produce heat by their absorption; and hence luminous rays were excluded, since it was still an open question whether these were in all cases capable of producing heat. I shall only add that it was attempted, as far as possible, to disengage the proof, theoretical and experimental, from the embarrassment of considering surface reflexion.

I first endeavoured to show, not only as a simple deduction from the theory of exchanges, but also as the result of experiments, that in a field of uniform temperature the absorption of a plate or particle is equal to its radiation. Now, since a thick plate absorbs more heat than a thin one, it will also radiate more,

so that here we are at once led to acknowledge a radiation proceeding from the interior of bodies as well as one from their surfaces.

Having thus shown that the equilibrium demanded by Prevost's theory extends into the substance of bodies, experiments were next made on rock-salt, mica, and glass, which resulted in proving "that every body which sifts heat in its passage through its substance is more opaque with regard to heat radiated by a thin slice of its own substance, than it is with regard to ordinary heat." This experimental result was conceived to involve the following extension of the law of equilibrium: "The absorption of a plate or particle equals its radiation, and that for every description of heat."

On this proof from experiment Kirchhoff remarks as follows: "The proof cannot be a *strict* one, because experiments which have only taught us concerning *more* and *less*, cannot strictly teach us concerning *equality*."

I presume this means that since one cannot experimentally insulate a ray of heat of a definite refrangibility, a strict proof cannot in this way be obtained. I shall only remark that the same objection holds with regard to many of our most valuable experiments on dark heat. I cannot, however, admit that the difficulty of this experimental verification is surmounted or even lessened by employing a high temperature and selecting a definite ray of light. For in this case it is necessary to compare together at the same temperature a body such as a flame, and a black body such as carbon; and here while an equality of radiation and absorption may be easily procured, it will be very difficult, if not impossible, to prove the necessary equality of temperature between the two bodies. I shall afterwards show that the best experimental proof of this law is given by the fact that coloured glasses lose their colour in the fire.

To return to the proof from dark heat. This is followed by a theoretical demonstration that the law thus rendered experimentally probable is also necessarily true. Kirchhoff remarks regarding this:—"And then he (Stewart) proceeds to more abstruse considerations which are intended to give such a rigid demonstration, and in which the meaning he attaches to the expressions *absorption* and *radiation* are more nearly defined. These considerations, however, are not sufficiently general or sufficiently precise to attain the required end; so that, after all, Stewart's proposition remains an hypothesis to which some probability is attached."

Now this proof is so far from being abstruse, that, with your permission, I shall state it simply in a few words, leaving men of science to judge for themselves of its completeness. Having

proved (with perfect right, as Kirchhoff himself allows) that radiation proceeds from the interior of bodies as well as from their surface, the question then occurs, "Are we to suppose each particle of each substance to have at a given temperature an independent radiation of its own, equal, of course, in all directions?" It might have been answered to this, that radiation is a property of the particles of a body which we cannot well conceive of as being influenced by the position of those particles in the substance of the body; so that perhaps it was hardly necessary to prove that the radiation of a particle or plate is independent of its distance from the surface. Nevertheless I have shown experimentally that the heat from two plates of rock-salt placed the one behind the other is the same as that from a single plate equal in thickness to the two.

Assuming it therefore as proved that the radiation of a particle or plate is independent of its distance from the surface, the proof of the law which asserts "that absorption is equal to radiation, and that for every description of heat," may for convenience' sake be carried into the interior of the body, by which means we are able to rid ourselves of surface reflexion. Let us therefore suppose that in the interior a stream of radiant heat is constantly flowing past a particle A in the direction of the next particle B. Now, since radiation is independent of distance from the surface, the radiation of A is equal to that of B; and since absorption is equal to radiation, the absorption of A is therefore equal to that of B. Again, as the stream of radiant heat passes A, part of it will be absorbed by A; but since the radiation of A is equal to its absorption, this stream will be as much recruited by the one as it is diminished by the other, so that when it has passed A it will be found unaltered by its passage with regard to quantity.

Of this heat it has already been shown that B absorbs as much as A; and in order that this may be the case, the *quality* as well as the *quantity* of the heat which impinges upon B must be the same as those of the heat which impinged upon A. For, suppose that the heat, by passing A, had changed its quality though not its quantity, and that it had been transformed into a description of heat scarcely absorbed at all by the substance in question; then the absorption of B would manifestly be less than that of A, and this we have already shown cannot be the case. We conclude therefore that the stream of heat, in passing A, has neither altered its quantity nor its quality, and hence we argue that radiation is equal to absorption, and that for every description of heat.

This is the whole proof; and I am quite at a loss to know in what respect it is deficient, especially since Kirchhoff has not definitely stated his objections. Having gone so far, we

are now enabled to obtain a clear conception of internal radiation. For since the stream of heat, as it proceeds, is just as much recruited as it is absorbed, we may to all intents regard this as if it were proceeding in a diathermanous medium without being absorbed at all, the absorption being virtually cancelled by the equal radiation.

I next endeavour to answer the following question: "Is the law of an equal and independent radiation of each particle of a body theoretically consistent with equilibrium of temperature? That is, suppose we have any irregularly-shaped enclosure walled round with a variety of substances, and each particle of each substance radiating into the enclosure,—from the sides of which it is reflected many times backwards and forwards before it is finally absorbed,—this being the case, will the law of equal and independent radiation, and those of reflexion and refraction so fit with one another, that every particle of the walls of the enclosure shall absorb precisely as much heat as it radiates?"

This question is answered in the affirmative; but Kirchoff makes the following remarks on the proof:—"By employing the law 'of equal and independent radiation' and the laws of reflexion and refraction, Stewart forms the equation expressing the proposition which has to be proved concerning the equality of absorption and radiation for heat of every kind. It appears that this equation contains no contradiction, but expresses a possible property of the internal radiation in a body. He argues from this that the proposition concerning the equality of absorption and radiation for every kind of ray *must* hold good. This is evidently a false conclusion. The above consideration proves that the proposition is *possibly*, but not that it is *necessarily true*."

I am quite unable to comprehend the meaning of these remarks. As far as I understand myself, I have already proved the proposition concerning the equality of absorption and radiation for heat of every kind. The object now is in some respects a superfluous work, this being to show that the law already proved fits in with the laws of refraction and reflexion. In order to accomplish this, I conceive two indefinitely extended parallel plane surfaces to be separated from one another by a small interval, one of these being a perfectly black surface, and the other the polished surface of a diathermanous uncrystallized body indefinitely extended downwards. An equation is soon obtained, one member of which expresses the amount of heat of a particular wave-length which leaves the diathermanous body in a given direction, and the other the amount of heat of the same description which enters the body in this direction.

Now the one member of this equation *must necessarily* be equal to the other. For it has been already proved that the radiation

of an interior particle is equal in all directions; and it follows that the flow of heat radiating inwards in a given direction must necessarily be equal in quality as well as in quantity to that which radiates outwards in the same direction. As might be expected, the equation implies no contradiction, but repays our trouble with additional information regarding the flow of internal radiant heat.

It is an immediate corollary from this proof, that the polished surface under consideration, if we add together radiation and reflexion, behaves precisely like a surface of lampblack; and it also easily follows that, instead of the plane black surface, we may substitute an enclosure of any substance or form. For since the heat which leaves the transparent surface is unaffected by this substitution, it is evident, from the law of equality which we have been considering, that the heat which enters this surface must be in like manner unaffected by the same substitution.

It thus appears that the equilibrium of temperature for the transparent body demands that the surrounding enclosure shall act like a surface of lampblack, and that, while this is demanded on the part of the other bodies, the transparent substance offers in itself no exception to the law.

One word with regard to the assertion that the flow of internal radiant heat is proportional to the index of refraction. It will be seen that in my first researches, when defining internal radiation, I confined myself to two dimensions of space, and arrived at a result which was quite true for this definition. I afterwards conceived that it would be a more complete view to regard the subject in three dimensions, and in my future papers I made the necessary alteration. The change from μ to μ^2 does not therefore imply a change from error to truth, but only one from a less perfect to a more perfect way of viewing the subject. While in one plane the bundle of rays opens out in the proportion of 1 to μ , it also opens out similarly in a plane perpendicular to the former; so that multiplying the one ratio by the other, and viewing the solid angle, these rays will open out in the ratio of 1 to μ^2 .

I think it will be seen from these remarks that, in order to obtain a complete solution of this problem, we must consider internal radiation, and that, as far as regards uncrystallized bodies, this solution has been already obtained. But for crystallized bodies I do not hesitate to say that the problem is still unsolved; nor is any one better able than yourself to appreciate the difficulties which attend investigations in this quarter.

I shall now briefly allude to the subject of light, to which my attention was next directed. Here I obtained experimental results precisely analogous to those which had been and

hold for dark heat. An explanation of these results requires us to admit that luminous rays shall in all cases produce heat by their absorption, and I have been informed by an eminent physicist of this country that he considers this to be one of the best proofs yet given of the identity between light and heat. Now, if we assume this identity as proved, the fact that heated coloured glasses invariably lose their colour in the fire, seems to afford a perfect experimental proof of the equality between absorption and radiation for every description of heat; for the conditions of the experiment evidently secure an equality of temperature between the dark body and the transparent one, while the eye becomes the best possible judge of the equality between radiation and absorption for every individual ray. I shall not here enter upon the question to what extent Kirchhoff in his demonstration had previously been forestalled by Provostaye; I may remark, however, that the proof of the Heidelberg Professor is so very elaborate that I fear it has found few readers either in his own country or in this. I conceive, besides, that it is an objection to the completeness of this proof that the subject of internal radiation is not investigated; and I think, moreover, that the experimental support which is here derived from light is not so unobjectionable as that afforded by those rays which are universally acknowledged to produce heat by their absorption.

I confess, however, that in new problems I do not attach the same extreme importance as the Heidelberg Professor to logical completeness of demonstration; for if the object of the investigator be to hasten on the progress of human knowledge, when such a one has matured a sufficiently good demonstration which he has well supported by experiments, it is surely neither advisable nor is it right that he should any longer defer to publish the results which he has obtained.

Although I preceded Kirchhoff nearly two years in my demonstration, I did not hesitate to acknowledge that his solution had been independently obtained; but, as a general principle, I cannot consent to admit that when a man of science has proved a new law and is followed by another who from the same premises deduces the same conclusion, the latter is justified in depreciating the labours of the former because he conceives that his own solution is more complete.

Will Kirchhoff himself willingly forego his own claims in favour of any one who shall in future ages devise (if this be possible) a simpler and more convincing demonstration than that which has been given us by the Heidelberg Professor? I feel, Sir, that, as an historian of science, you will acknowledge the justice of these remarks, and join with me in regretting that

one who has so eminently distinguished himself in original investigation should have chosen to superadd to his functions as a discoverer those of a severe and hostile critic upon the labours of those men who have worked at the same subject with himself, and by all of whom he has been treated with the utmost possible consideration.

I remain,

My dear Sir David Brewster,

Yours very truly,

5 Alva Street, Edinburgh,
April 10, 1863.

B. STEWART.

L. *On the Motion of Vapours toward the Cold.* By CHARLES TOMLINSON, *Lecturer on Physical Science, King's College School, London.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE just been arranging for repetition Dr. Woods's ingenious and beautiful experiment on the motion of vapours toward the cold, as described by him in the current Number of your Magazine.

While engaged last summer in investigating the law of the motions of camphor, &c. toward the light, I performed a number of experiments in the same direction as that by Dr. Woods. They were not described in my paper inserted in your Magazine for November last, because in it, as at the British Association Meeting in October, I wished to be as concise as possible; but you will remember that in my published paper I state (p. 361) that "I omit many subsidiary facts observed during the inquiry." The following are a few of them.

On the 21st of May, 1862, a solution of iodine in bisulphide of carbon was put into a stoppered eight-ounce phial in quantity sufficient to form a ring at the bottom and to surround a central convex island. The bottle was placed in a west window. The day was cloudy and wet. In the course of an hour a violet dew was formed a little above the solution. In another half hour (*i. e.* at noon) the dew became more copious, and extended over nearly the whole surface of the bottle, but was densest on the side nearest the light. In another half hour (12.30 P.M.) the beads of dew were smaller and paler. At 1 P.M. they had become almost colourless, and solid iodine had condensed on the convex island at the bottom of the phial. At 3 and also at 4 P.M. there was a gleam of sunshine, when the bottle became filled with a yellowish vapour and the dew again became coloured,

though faintly. At 7 P.M. the dew formed a violet arc on the side nearest the window.

On the morning of the 22nd the arc was pale, and there was a dense deposit of thick purple matter on the furthest side just above the liquid ring. At 5 P.M., the sun being on the window, the dark-coloured deposit was on the nearest side, but equally low down. These are effects of capillarity evidently similar to those so well described by Dr. Woods; but I paid no particular attention to them at the time, as my mind was occupied with other phenomena.

As the season advanced and became more settled, the phenomena varied. A portion of the bisulphide was decomposed, and another portion forced its way out of the bottle. During these events the motions of the vapour went on with the regularity of a clock. When the sun was on the window, a current apparently rose on the nearest side and descended on the furthest, where the deposit was made. When the sun was off the window, the current set in in the opposite direction; and the nearest side being the colder, the deposit was transferred to it. These motions were marked in an interesting manner at the sides of the bottle; at first by well-shaped parabolic spaces, marking the temperate regions between the hot and the cold ones; afterwards by lines as straight as if ruled, diverging above and below from a centre placed a little above the liquid. The lines above this centre were several inches in length; those below it about a third of an inch. These lines, lastly, changed into a minute sea-weed pattern which remained fixed for some weeks; but I find by my note-book that the bottle had become dry early in June.

A similar experiment was also tried with phosphorus in bisulphide of carbon, &c.

I also performed a number of experiments on the motion of the vapour of water towards the light. During many years I had often noticed, when preparing gases for the purpose of a lecture, that the moisture condensed on the sides of the gas-jars and bottles was of a different pattern with different gases. I speculated a good deal as to why this should be so, but now determined to bring the fact under the ordeal of exact experiment. Accordingly in May last I filled six quart stoppled bottles, No. 1 with oxygen, No. 2 with nitrogen, No. 3 with hydrogen, No. 4 with nitric oxide, No. 5 with carbonic oxide, and No. 6 with carbonic acid. The gases were collected over water at the pneumatic trough, and about half an ounce of water was left in each bottle, which was tied over with soft leather wetted in a solution of gum, which was also run round the stopple. The bottles were placed on a table near a west window, and a journal was kept of their proceedings, which were recorded twice

a day, together with the temperature and the state of the weather. The dew was of the most varied character in the different bottles : in some it was fine, in others coarse ; and the deposit was also frequently ploughed into furrows by the weeping tears which trickled down as the moisture on the inner side of the shoulder of the bottle became overcharged. It was beautiful to see the almost clocklike regularity with which the deposits oscillated between the front and back of the bottles. At 9 A.M., when the first observation was generally made, the deposits were on the side nearest the window, often occupying exactly one-half of the bottle ; at 3, 4, or 5 P.M., when the second observation was taken and the bottles had been exposed to the sun some time, the deposit was on the furthest side. When the afternoon was cloudless, the bottles were filled with vapour ; but as the evening advanced, the deposits were made on the furthest side, again to pass over to the nearest side as the cold of night was felt.

With respect, however, to the texture of the dew, the results were anomalous. After several weeks' observation I was not in a condition to place my finger on any one bottle and name its contents from the character of the aqueous deposit—except perhaps in the case of carbonic acid, which declared itself by its superior solubility. Suspecting that the bottles, although new from the glass-warehouse, were not clean, I washed out other similar bottles with strong sulphuric acid, rinsed them with clean water, and filled No. I. with air, No. II. with hydrogen, and No. III. with carbonic acid. On placing these near the window, the results were certainly such as to justify the conclusion that the deposits of moisture are similar in various gases if the receiving surface be chemically clean. In such case, however, we get, not dew, but a sheet of water marked by weeping tears, but without furrows ; and it is often difficult to distinguish on which side the deposit is made, so regular is the watery film. There can be no doubt, however, as to its being deposited on the colder side.

But if vapour of water in different media presents similar phenomena, different vapours in the same medium give varied phenomena. I have arranged in the window, as nearly as possible under similar circumstances, clean 8-ounce phials containing small quantities of alcohol, ether, benzole, belmontine, bisulphide of carbon, &c. The deposits varied in texture according to some law depending partly on the varying cohesive force of the liquids in question, by which they form drops of different sizes from the same aperture, and ascend to different heights in capillary tubes of the same bore.

I may here be allowed to refer to one of Dr. Draper's experiments, in which the inside of a glass bottle being marked with a

blunt point, such as that of a glass rod, the camphor vapour is afterwards deposited along the marks. A fact of this kind was pointed out more than a century ago by De Mairan*, who noticed that the hoar-frost was deposited on his window-panes in spiral lines, produced, as he supposed, by the fine sand or ashes used in cleaning the windows; and although these lines were not visible to the eye, they nevertheless formed ridges sufficiently prominent to catch the condensing vapour. To test this idea, Carena†, in the severe winter of 1814, cleaned four panes of his window with fine sand, rubbing two of them with a circular movement, a third in straight lines from top to bottom, and a fourth in diagonal lines. On the next day and on several succeeding days the hoar-frost was deposited more or less on the lines or furrows produced by the friction.

In the older memoirs and treatises on dew, the writers are very fond of ornamenting their work with pleasing figures of dew and hoar-frost as it appears on various objects; and they attribute the differences in the patterns of the watery particles to differences in the radiating powers of the objects in question. It does not seem to have been suspected that the varied patterns also depend on the want of adhesion to the surface. When this is chemically clean we get, not dew, but a sheet of water. All objects exposed to the air contract an organic film; and when the inner surface of a bottle, for example, as in Dr. Draper's experiment, is rubbed with a blunt point, the film is raised into ridges which catch the camphor vapour more readily than the other parts of the surface‡. Indeed so sensitive is this vapour in detecting prominences, that it will attach itself to, and render visible minute objects on the inner surface of the bottle which before were quite invisible. For example, I have many times washed out a bottle and wiped it dry with a clean cloth. The bottle appeared to be perfectly clean to the eye; yet after a few minutes' exposure to the sun, with its charge of crude camphor, innumerable filaments, evidently derived from the duster, would start into view from being coated with a very thin layer of camphor.

In a small volume recently published, entitled "Experimental Essays," I give some specimens of camphor-figures modified by the presence of vapour of benzole, wood-spirit, naphtha, chloroform, nitric acid, &c. One of the actions of these liquids is apparently to modify the rate of evaporation of the camphor.

* *Dissertation sur la Glace.* Paris, 1749.

† *Mémoires de l'Académie Royale des Sciences de Turin pour les années* 1813-14.

‡ Most of the phenomena of breath-figures (the *figures roriques* of the French, and the *Hauch-Figuren* of the Germans) depend on the existence of this film.

Equal quantities of camphor were placed in glass capsules before a window, and were wetted with the same number of drops from the end of a glass rod. The liquids were water, benzole, ether, chloroform, bisulphide of carbon, and alcohol. A similar weight of camphor was left untouched for comparison. The camphor disappeared by evaporation at very different rates; but the experiment requires to be repeated with more care before numerical results can be given. It illustrates probably only a case of adhesion. The liquids partly dissolve, spread out the camphor, and attach it with more or less force to the capsules. I have also observed that where camphor has been touched with bisulphide of carbon, &c. and then exposed to the light in stoppered bottles, a creeping kind of vegetation is formed; and unless the temperature be higher than that of the air in the shade, the deposits are very different from those obtained from camphor not embarrassed by the presence of another vapour.

The whole subject of the action of vapours on camphor is a curious one and might repay further inquiry; but as far as my notes at present extend I should, by quoting them, merely multiply facts without leading to any useful principle. I fear that this communication requires an apology on that ground.

I remain, Gentlemen,

Your obedient Servant,

King's College, London,
April 2, 1863.

C. TOMLINSON.

II. *On the Absorption of Gases by Charcoal.*

By JOHN HUNTER, *Scholar, Queen's College, Belfast*.*

I AM induced to publish rather sooner than I intended the following preliminary results of a series of experiments with which I have been engaged for some time on the absorption of gases by charcoal, in consequence of observing that a paper on the same subject has lately been read before the Royal Society.

In this inquiry I have followed nearly the same method which was employed by Th. de Saussure in his well-known memoirs on the absorption of gases by charcoal and other bodies. The gases, carefully dried, were collected over mercury, and after their volume had been observed the charcoal was introduced. It was heated to redness before each experiment, and while in a state of incandescence immersed under mercury. My first object was to ascertain the absorbing power of different kinds of charcoal; and

* Communicated by the Author.

to this part of the inquiry the present short paper is devoted. It will be observed that while the numbers indicating the absorptions agree remarkably well in many experiments, in others they differ considerably. When the gas is largely absorbed, as in the case of ammonia, by logwood charcoal, the absorption is extremely rapid, and terminates in a much shorter time than when the entire absorption is less. The variations in the amount of absorption are probably due in part to the slight differences which must exist in the pores of different pieces of the same charcoal, and also to unavoidable differences in the heating of the charcoals in each experiment. In order to determine the effect of long-continued heat on the absorbing power of the charcoal, I kept a piece of boxwood charcoal at a strong red heat for upwards of an hour, after which its absorption for ammonia was found to be 79, while when heated in the ordinary way it was 85. In all these experiments the charcoal was heated as nearly as possible to the same temperature and for the same length of time. The absorption generally terminated at the end of about twenty-four hours, so that it was not increased by allowing the charcoal to remain longer in contact with the gas. Charcoals do not absorb all gases in the same relative proportion: thus logwood has the greatest absorption for ammonia, fustic for carbonic acid, and ebony for cyanogen.

In the first Table are given the data from which the amount of absorption was deduced in each case.

- V = the observed volume of the gas in cubic centimetres ;
- D = the difference in level of the mercury ;
- P = barometric pressure ;
- T = temperature, in Centigrade degrees ;
- C = volume of the charcoal, in cubic centimetres.

The separate experiments are indicated by the numbers 1, 2, and 3. The upper line in each contains the volume of the gas before the introduction of the charcoal, together with the observed difference in level, pressure, and temperature; the lower line gives the volume, &c. after the absorption was completed.

Table II. contains the absorptions corresponding to the experiments in Table I. The first column under each gas gives the volume of gas, reduced to 0° C. and 760 millims., absorbed in each experiment, and the second column gives the means.

This investigation was conducted in the laboratory of the Queen's College, Belfast.

TABLE I.

	Ammonia.					Carbonic acid.					Cyanogen.						
	V.	D.	P.	T.	C.	V.	D.	P.	T.	C.	V.	D.	P.	T.	C.		
Logwood	39.84	41.0	745.0	13	.235	34.02	49.0	763.0	9.0	.187							
	9.89	35.7	742.5	13		22.56	37.0	757.0	8.9								
	40.23	46.1	741.8	12.8		35.28	51.5	771.5	9.6		.185	36.24 20.27	41.5 30.0	761.0 770.2	10.5 11.0	.162	
18.44	36.0	743.0	9.0	25.21	33.5	768.0	11.1										
	40.53	43.0	742.5	9.2	.211												
	14.32	35.0	748.0	12.5													
	40.79	39.5	767.7	13.7		.185	36.24	37.5	768.0	10.8	.256						
Ebony.....	19.09	37.0	770.0	14.7	22.13		35.0	757.5	7.7								
	42.09	38.5	766.5	14.5	35.67		36.8	762.0	9.8	.45		40.1 21.1	44.0 30.0	771.4 774.0	11.9 9.7	.1809	
	14.75	32.0	766.5	16.0	11.9.8	30.5	755.5	10.2									
	42.18	41.0	766.0	16.0	.229	34.8	41.5	753.0	12.2	.421							
	13.8	33.0	772.5	13.8		15.5	47.2	768.5	11.8								
	41.45	36.4	754.0	13.5		.214	38.36	41.0	774.5		11.1	.31					
Camwood	22.14	35.0	753.5	15.2	24.17		29.0	744.7	11.8								
	41.14	43.0	767.0	17.0	35.59		35.0	752.3	9	.253							
	14.54	40.0	762.0	18.2	21.48	26.8	745.7	10.3									
	40.36	40.0	762.0	18.7	.251	34.94	40.0	746.0	11.0	.288							
	14.97	41.0	760.5	15.7		20.83	29.0	738.8	11.5								
	42.27	47.0	753.5	14.8		.144	35.46	31.0	751.3		9.7	.276					
Green Ebony	24.39	38.0	745.0	13.5	25.82		71.0	754.0	9.9								
	42.18	47.0	760.5	16.0	15.19		31.5	756.2	8.3	.347							
	6.43	36.0	760.0	17.8	35.15	32.3	766.6	10.0									
	42.62	40.0	757.5	11.2	.169	21.05	67.0	771.5	10.2	.33							
Fustic (Cuba) ...	26.04	36.4	768.5	13.3		39.41	35.0	747.5	10.5								
	40.82	39.2	758.0	14.8		29.73	30.0	751.2	10.2		.184						
	21.39	35.0	766.0	16.4	33.98	36.0	750.2	10.3									
					.197	22.13	30.0	760.8	9.8								

TABLE II.

	Ammonia.		Carbonic acid.		Cyanogen.	
	Volume absorbed reduced to 0° C. and 760 millims.		Volume absorbed reduced to 0° C. and 760 millims.		Volume absorbed reduced to 0° C. and 760 millims.	
	Experiment.	Mean.	Experiment.	Mean.	Experiment.	Mean.
Logwood	112·4 111·3 110·1	111·3	53·1 56·2	54·6	87·3	
Ebony.....	106·6 104·4 108·7	106·7	46·6 50·3 44·0	47·0	89·6	
Camwood	93·0 89·5	91·2	44·7 51·3 44·7	45·4		
Green Ebony	96·0 84·6	90·3	37·4 44·8 41·3	40·8		
Fustic (Cuba).....	89·7 89·1	89·6	61·7 54·9	58·0		
Lignum Vitæ	88·2 89·2 83·0	89·0	47·2 47·2			
Boxwood	86·6 84·5	85·6	31·2 31·2 30·2	31·2	28·8	
Jamaica Logwood	65·0 73·8	69·5	33·3 33·3	33·3		
Sapan Wood	69·9 69·8	69·9	32·2 32·2			
Beech	54·6 61·3	58·0				
Rosewood	50·6	50·6				
Wistaria sinensis ...	44·03	44·03				
Vegetable Ivory	47·5 53·6 50·3	50·5	57·3	

March 20, 1863.

LII. *Remarks on the Dynamical Theory of Heat.*

By JOHN TYNDALL, F.R.S.

To WILLIAM THOMSON, Esq., F.R.S., Professor of Natural Philosophy in the University of Glasgow.

SIR,

THE article in 'Good Words' which drew from me the brief remonstrance published in the March Number of the Philosophical Magazine bore your name, which preceded, though

not by right of the alphabet, that of Professor Tait. From this gentleman's communication to the Philosophical Magazine of last month, I learn that it was in compliance with requests made to you that the article in 'Good Words' was written. Indeed, irrespective of these considerations, in any article of the kind where your name is associated with that of Prof. Tait, you are sure to be regarded as the 'principal.' You are the older and more famous man, and it is your behaviour in this controversy, and not that of your colleague, which will interest the scientific world. I trust, therefore, Prof. Tait will see that simple chivalry makes it my duty to decline entering into any contest with him at present; and seeing this, he will, I doubt not, have the grace and modesty to stand aside and allow you and me to settle this affair between ourselves.

At your request, we are informed, your friend has replied to my "Remarks" both for you and for himself. Doubtless his reply has passed through your hands and received your assent. I may therefore, I hope, regard it as *your* reply, and deal with it accordingly. You open thus:—"I think it right at starting to call Prof. Tyndall's attention to the fact that, in the Philosophical Magazine (1862, vol. xxiv. p. 65), he has published the following words:—"I do not think a greater disservice could be done to a man of science than to overstate his claims: such overstatement is sure to recoil to the disadvantage of him in whose interest it is made." There is surely nothing in these words of which an honest man need be ashamed. They are, I submit, sensible words; and were they not *my* words, I should on various grounds strongly recommend them to your own particular attention. But besides reviving my memory of these words, you remind me "that any unpleasant results which may follow from the course which he [Prof. Tyndall] has pursued are, by his own acknowledgment, to be laid to his charge."

I hope you will forgive me if I now remind you that, in conducting this correspondence, it is extremely desirable that each of us should endeavour to make his meaning clear. This 'reminder' of yours is by no means clear. As a deduction it would be illogical, as an assertion it would be untrue. If in such a simple matter your writing is made difficult of comprehension, how am I to seize your meaning when you come to deal with a really difficult subject? Let each of us try then to render his meaning unmistakable; but this once done, let neither of us seek to divert the words of the other from their plain and obvious sense. This course will shorten controversy, and make the world feel that we mean fairly by each other. In your last communication you write thus:—"A journal which contains in nearly every Number a scientific paper by yourself [Sir David

Brewster, to whom Prof. Tait addresses his communication], Forbes, Herschel, or Piazzi Smyth, can surely not be regarded as unsuitable for a paper on 'Energy.' Did I say it was? Did I not rather state that it was both laudable and desirable that men in your high positions should *instruct* the readers of 'Good Words;' and I may here add, what I am known to have stated a hundred times, that in my opinion it would be an incalculable boon to English society if our best men could be prevailed upon to write our scientific manuals, and to teach the public through our leading magazines. But I also said, and I think rightly, that 'Good Words' is not the place to give vent to insinuations prejudicial to the character of a scientific fellow-worker. And inasmuch as I know that the character you impugn is of a quality to defy all your attacks, if only openly made, it is only natural that I should invite you to make your charges against it in the presence of a jury competent to understand your evidence.

With regard to the publication of my lecture on Force, the course pursued was this: the abstract of the lecture was published, as all such abstracts are, in the 'Proceedings of the Royal Institution,' and it was transferred thence to the *Philosophical Magazine*. With its further publication I had nothing whatever to do. Until you informed me of the fact, I did not know that the lecture had been honoured by admission into the 'Illustrated London News,' the 'Engineer,' and 'Macmillan's Magazine.' The gentleman whom you denominate my 'pupil' is quite unknown to me. But even had I written my abstract expressly for any of these journals, there would, I submit, be nothing unworthy or inconsistent in the act; for the lecture contains imputations upon no man's character. My object in giving the lecture was not to defame anybody, but to raise a noble and a suffering man to the position which I believed his labours entitled him to occupy, and from which I am persuaded your efforts will be unavailing to remove him.

In proof of your acquaintance with Mayer, you refer me to a paper of yours in the *Edinburgh Transactions*, in which I am informed you give him "the full credit that his scientific claims can possibly be admitted to deserve." This is an assertion made without knowledge. Regarding Mayer's labours you knew, I assume, what everybody knew—the contents of his first paper, which, able and important as it is, is in reality nothing more than a preliminary note*. Everybody was acquainted with this paper,

* "To secure what I had discovered against eventualities, I put together its most important portions in a short paper, which I sent to Liebig in the spring of 1842, with a request that it might be published in the *Annalen*

because it was published in Liebig's *Annalen*; and had Mayer's subsequent memoirs been published in the same way, I do not think you would ever have ventured to provoke this controversy. The contributions from which, in connexion with his first one, Mayer's merit as a philosopher is to be inferred, were published as separate essays at Heilbronn. To these essays you never refer, and from your silence I charitably inferred your ignorance of their contents. This, however, does not satisfy you. And yet if I assume that you are acquainted with these memoirs, *how am I to reconcile your knowledge with your silence?*

You have thought it necessary to refresh my memory on another point which I should willingly have allowed to remain at rest. "Prof. Tyndall," you say, "is most unfortunate in the possession of a mental bias which often prevents him (as, for instance, in the case of Rendu and Glacier-motion) from recognizing the fact that the claims of individuals whom he supposes to have been wronged* have, before his intervention, been fully ventilated, discussed, and settled by the general award of scientific men." This language, as applied to Mayer, can only spring from ignorance; and as regards Rendu, it would be wise in you to remember that your opinions on this matter are not those of the world. Even among your own countrymen there are persons too just and too free to be warped by a misnamed patriotism, who, in common with the great majority of its readers in other lands, consider that in my work upon the Alps the claims of Rendu have been for the first time fairly set forth. No man, however, can be less persuaded of his own infallibility than I am of mine, and I clearly see the possibility of my having greatly erred in my writings upon glaciers. But I am willing at all events to bring my deeds to the light, so that, whether good or evil, they may be made manifest. I have now to request you to refer me to the passages of my writings on Rendu and Glacier-motion which establish the unfortunate mental bias you have ascribed to me.

It may shorten our correspondence, by making reflexion the precursor of expression, if I here assure you that it is my fixed intention not to permit random assertions to stand in this discussion. What you have the hardihood to affirm, you certainly must have the goodness to prove or the manliness to retract. I ask your proof, then, of the assertion you have made regarding Rendu and Glacier-motion.

der Chemie und Pharmacie. It is to be found in vol. xlii. page 233 of the *Annalen* under the title 'Bemerkungen über die Kräfte der unbelebten Natur.' (Mayer, *Bemerkungen über das mechanische Equivalent der Wärme.* Heilbronn and Leipzig, 1851.)

* I did not suppose Mayer to be wronged: I supposed simply that he had wronged himself by his method of publication.

After disposing of the matters to which I have had thus far occasion to refer, apparently much to your own satisfaction, you put the following pointed question:—"Does Professor Tyndall know that Mayer's paper has *no claims to novelty or correctness at all*, saving this, that by a lucky chance he got an approximation to a true result from *an utterly false analogy*?" I am able with the utmost sincerity of heart to avow that I did *not* know anything of the kind, and that you are the first man who has furnished me with this information. I should doubtless stand abashed and confounded at my convicted ignorance were it not that I find myself surrounded by an extremely respectable company who are actually as ill-informed as myself. Permit me to illustrate my meaning. In a discourse delivered at Königsberg on the 7th of February, 1854, Prof. Helmholtz analyses that relationship of natural forces which he, in 1847, called the Conservation of Force—a phrase to which you object as inconvenient and erroneous, but which means the same thing as the phrase 'Conservation of Energy' which you adopt. This conservation Helmholtz showed to follow directly from the negation of a perpetual motion; and it is with reference to this great principle that he expresses himself in the following words*:
 "The first man who correctly perceived and rightly enunciated the general law of nature (*das allgemeine Naturgesetz*) which we are here considering, was a German physician, J. R. Mayer, of Heilbronn, in the year 1842. A little later (in 1843) a Dane named Colding presented a memoir to the Academy of Copenhagen, in which the same law was enunciated. . . . In England Joule began about the same time to make experiments on the same subject." You say that you "never intended to hint that Prof. Tyndall could have meant to put Mayer forward as having any claims to this great generalization." Bad as I am, you would not think of charging me with this grotesque folly. And still for nine years Helmholtz, who was a master in this field before you ever entered it, has stood self-convicted of this very absurdity.

Let me now refer you to another of my companions in ignorance. On the 7th and 21st of February, 1862, two lectures on the Mechanical Theory of Heat were given by M. Verdet before the Chemical Society of Paris†. This philosopher, to whom everybody has hitherto ascribed a thorough knowledge of the literature of science, expresses himself on the subject in hand in the following words:—"Finally, I will terminate this first part of my historic review by calling to mind that M. Séguin, in

* *Ueber die Wechselwirkung der Naturkräfte*. Königsberg. Phil. Mag. vol. xi. p. 489.

† *Exposé de la Théorie Mécanique de la Chaleur*, par M. Verdet.

a work published in 1839, and devoted more to political economy than to physics, has presented considerations regarding the steam-engine which closely resemble those by which I endeavoured, in our first lecture, to make you comprehend the transformation of heat into mechanical power."

I should deem it probable that M. Verdet knows as much about the labours of M. Séguin as you do, and he certainly knows more about those of Mayer. But he does not see in the former the annihilation of the latter. He proceeds thus:—"I now come to the researches which from 1842 to 1849 definitely founded the science. These researches are the exclusive work of three men, who without concert, and even without knowing each other, arrived simultaneously in almost the same manner at the same ideas. The priority in the order of publication belongs without any doubt to the German physician Jules Robert Mayer, whose name has occurred so often in these lectures; and it is interesting to know that it was by reflecting on certain observations in his medical practice that he perceived the necessity of an equivalence between work and heat. . . . He perceived in the act of respiration the origin of the motive power of animals; and the comparison of animals with thermic engines afterwards suggested to him the important principle with which his name will be connected for ever. . . . We also find," continues M. Verdet, "in the same memoir (Liebig's *Annalen*, 1842) a first determination of the mechanical equivalent of heat, deduced from the properties of gases, which is perfectly exact in principle*." You see then that I am not the only person who has failed to arrive at your remarkable conclusion that "Mayer's paper has *no claims to novelty or correctness at all*."

After referring to M. Colding, M. Verdet passes on to Mr. Joule. "The third inventor," he says, "of whom it remains for me to speak is Mr. Joule, who perhaps has done most for the demonstration of the principle and for its final adoption. His first investigation, published only in 1843, is incontestably posterior by some months to the first publications of Mayer and Colding†. All this is certainly very much at variance with your statement in 'Good Woods.' "Curiously enough," you there say, "although similar coincidences are common, while Joule was pursuing and publishing his investigations, there appeared in Ger-

* Page 116.

† Though the history of the subject compels him to write thus, M. Verdet recognizes in the most explicit manner the merits of Mr. Joule. After giving an account of Joule's experiments, he thus addresses his audience:—"Vous ne refuserez pas à voir dans le travail aujourd'hui classique de M. Joule la preuve expérimentale de l'exactitude des nouveaux principes." (P. 20.)

many a paper by Mayer, of Heilbronn. Its title is 'Bemerkungen über die Kräfte der unbelebten Natur,' and its date is 1842." Would you have the goodness to point out a single word regarding the mechanical value of heat, or the mechanical equivalent of heat in any of Mr. Joule's writings prior to the month of December 1843? If no such word occurs, if Helmholtz and Verdet be correct in assigning this date to the earliest experiments of Mr. Joule on the 'Mechanical Value of Heat,' how am I to characterize your statement that it was while Joule was pursuing and publishing his investigations that Mayer's paper, dated May 1842, appeared? If you should urge that these investigations led Mr. Joule to his experiments on the mechanical equivalent of heat, you simply repeat what I have myself stated in the very lecture which has been the object of your unwarrantable attack.

With reference to the manner in which I have met your insinuations in my brief communication to the Philosophical Magazine, we have the following remarks:—"I am unwilling to enter upon matters of a more personal character, but it is impossible to pass over the fact that, in answer to an expostulation from Joule (having reference to this lecture), Prof. Tyndall referred (Phil. Mag. 1862, second half year, p. 173) to statements which he had made in a course of lectures which, he now tells us, were completed *before* he acquired those views of Mayer's claims to which Joule so naturally objected, and that he now replies to Prof. Thomson and me, not by showing that his lecture, to which we referred, was free from the objections which we urged against it, but by showing that these objections could not be urged against a certain statement which he quotes from a work, not published, but promised for publication." This reads smart; but my frame of mind disqualifies me from meeting it with mere smartness. In replying to it I do not forget that my aim must not be to satisfy *you* as to the uprightness of my course of action, but to enable the scientific public to judge between your actions and mine. On my return from Switzerland in 1862, I became acquainted with the "expostulation" to which you refer; and I also found waiting for me a private letter from Mr. Joule, reminding me of what he had done in connexion with this subject. In one of my morning lectures given six months previously, I had recognized in the strongest terms the claims of Mr. Joule on the very points to which, in his private note, he had directed my attention. In my answer to his "expostulation" I quoted this lecture, not expecting that Mr. Joule ought to be satisfied with a statement made six months before, but for the purpose of publicly renewing the value of that statement by resubscription to it. This was done in the following words addressed to Mr. Joule:—"Such has been my language regarding you, and to it

I still adhere: I trust you find nothing in it which indicates a desire on my part to question your claim to the honour of being the experimental demonstrator of the equivalence of heat and work." It was in the presence of this statement that you ventured to insinuate in 'Good Words' your charge of depreciation and suppression; and it is in the presence of this statement, requoted for your especial information in the March Number of the Philosophical Magazine, that you now dare to reiterate this charge. The quotation from my book will be recognized by the court before which you and I now stand, as springing from the same spirit of goodwill towards Mr. Joule, and the same high appreciation of his services which have marked all that I have said and written regarding him. It is not only 'now' that I entertain those feelings, as you intimate in your concluding paragraph, they have always been mine; and they will continue to be mine despite an advocacy on your part so unwise and so unwarranted as to be well calculated to produce a reaction against the man who is unfortunate enough to find in you the assertor of his claims.

Your article in 'Good Words' was professedly written to counteract the errors and absurdities of previous writers. "We have aimed," you say, "at preparing an article which shall at all events be *accurate* as far as human knowledge now reaches," or as you more cuttingly express it in reference to my poor lecture, you "seized the opportunity of distributing among its ['Good Words'] 120,000 readers a corrective to the erroneous information which you saw stealing upon them through the medium of *popular* journals." Well, let us inquire a little into the quality of the "corrective." In putting Mr. Joule forward as the founder of the dynamical theory of heat, you write thus:—"In 1843 he (Mr. Joule) published the results of a well-planned and executed series of experiments, by which he ascertained that a pound of water is raised one degree Fahrenheit in temperature by 772 foot-pounds of mechanical work done upon it. In other words, if a pound of water fall from a height of 772 feet, and the kinetic energy thus acquired in the form of ordinary motion be entirely transformed into the kinetic energy of heat, the water will be one degree hotter than before its fall." You here prove yourself to be as ill-informed regarding the labours of Joule as you are regarding those of Mayer. It was in 1849, and not in 1843, that Mr. Joule proved the mechanical equivalent of heat to be 772 foot-pounds. His determinations of the mechanical equivalent of heat, published in 1843, varied from 1040 to 587 foot-pounds. They were so discordant that nobody attached any value to them. It was with reference to Mr. Joule's earlier experiments that Helmholtz expressed himself thus, in

his celebrated essay 'Ueber die Erhaltung der Kraft,' published in 1847:—"Joule has examined the quantities of heat generated by the friction of water in narrow tubes, and in a vessel in which the water was set in motion by a kind of turbine. He has found in the first case that the heat which raises 1 lb. of water 1° C. would lift 452 kilogrammes to the height of 1 metre; in the second case he found the weight to be 521 kilogrammes. Nevertheless his methods of experiment correspond too little to the difficulties of the investigation to allow of these results having the least claim to accuracy"* (*als dass diese Resultate irgendwie auf Genauigkeit Anspruch machen könnten*). I translated this paper myself for the 'Scientific Memoirs†,' and was careful to append to this passage a note to the effect that Helmholtz when he wrote thus was only acquainted with the earlier experiments of Mr. Joule. Judged on broad and liberal grounds, Mr. Joule is worthy of such praise as rarely falls to the lot of a philosopher. He is, in my opinion, the true experimental demonstrator of the dynamical theory of heat. But to write as you write regarding his first experiments is simply to betray a want of acquaintance with the requirements of refined experimental inquiry.

You state in 'Good Words' that Mayer's hypothesis is at best only "*approximately*" true for air. Yes; but the approximation is so close that it cost Mr. Joule six years of labour, according to his own methods, to arrive at the degree of accuracy attainable by the method of Mayer. You coldly deny to Mayer's first paper all claim to novelty or correctness; and yet a comparison of that paper‡ with the first part of your "corrective" in 'Good Words' will show that the ideas which you have there enunciated are the ideas of Mayer. The very illustration which you employ, of stirring water in a basin to produce heat, is an experiment of Mayer's mentioned in this first paper. In the subsequent portion of the article referred to, you come to what you denominate "the grandest question of all." You put it thus:—"Whence do we immediately derive all those stores of potential energy which we employ as fuel or food? What produces the potential energy of a loaf or a beefsteak? What supplies the coal and the water-power without which our factories must stop?" These "grandest questions of all" were asked and answered by Dr. Mayer seventeen years before you wrote your article; and yet you never mention his name§. You proceed, "Whence does the sun produce the energy which he so continuously and

* See also Verdet, pp. 91 & 165.

† 1853.

‡ A translation of it is published in the *Philosophical Magazine*, vol. xxiv. Ser. 4. p. 371.

§ "Ces idées introduites pour la première fois dans la science en 1845, par Jules Robert Mayer, font faire à la physiologie générale un progrès

liberally distributes?" You then consider the various hypotheses framed to account for the permanence of solar emission, and develop the meteoric theory of the sun's heat. Mayer did the same fourteen years before you wrote this article in 'Good Words,' and six years before you published your earliest paper on the subject in the Edinburgh Transactions. There is not an idea of any originality in the whole of that paper that is not to be found in the memoirs of Mayer; and yet you do not give him an iota of credit in this article of yours in 'Good Words,' the accuracy of which you have so trumpeted forth. Mayer, moreover, went further than you did, and showed that the heat produced by tidal friction must be generated at the expense of the earth's rotation. This is but an episode in Mayer's 'Essay on Celestial Dynamics,' but it is eminently illustrative of the clear glance which this hard-working physician had obtained into the system of nature. Here was a phenomenon which had been for centuries the subject of observation, measurement, and calculation; still no astronomer, no mathematician, no natural philosopher perceived its inevitable mechanical effect. But Mayer, following out his own principles, saw and enunciated that the motion of the tides, indefinitely continued, must finally stop the earth's rotation. Name, if you can the "mathematician or naturalist, from Galileo to Davy," who ever "elaborated" these things. You cannot do so; and while science lives, the name of Mayer will be associated with these questions. In the presence of such facts, it ill becomes you to talk to me of suppression and depreciation. You may send your statements into the world labelled 'Good Words,' but the world before which you and I now stand will see that the "trade mark" is incorrect; that if to be pitiful, if to be courteous, if to cherish that charity which thinketh no evil, be the marks of goodness, these utterances of yours are *not* good words, but the reverse. Judged of by the facts, and apart from your own uninformed convictions, they are not even words of truth.

In your last communication you anticipated "unpleasant results," and, doubtless, taking it for granted that the unpleasantness was to be all on my side, you were good enough to inform me (still by anticipation) that I had only myself to blame. Certainly it is in the highest degree unpleasant to me to be compelled to write as I have here written. It wastes that commodity which to me is the most precious of all—my time, and it stirs up feelings between you and me which I would make any sacrifice consistent with self-respect to annul.

assurément égal au progrès qui est résulté, vers la fin du siècle dernier, des découvertes de Lavoisier et de Senebier sur la respiration."—Verdet, p. 101.

But the responsibility is yours. No taint of personality is to be found in anything that I have ever written regarding you; even when facts were against you I held my peace. When I wrote my brief and temperate remonstrance to the Philosophical Magazine, all that I have stated in this letter was known to me. I then knew, as well as I do now, that your treatment of Mayer could not bear the light of criticism; still my remonstrance breathed no syllable of accusation. My desire was to clear myself, and not to criminate you, trusting that the translations of Mayer's works now appearing in the Philosophical Magazine would in due time set public opinion right regarding him. I hoped that good sense and gentlemanly feeling would so far predominate as to induce you to withdraw an accusation which, though vaguely uttered, was offensive. Had you done so, this discussion would have perished in the bud, and I should never have written a sentence which could be personally disagreeable to you. But instead of withdrawing your accusation, you repeat it, and thus compel me in self-defence to lay a true statement of the case before the scientific public, and invoke its judgment between us.

I have the honour to be, Sir,

Your obedient Servant,

JOHN TYNDALL.

I have studied Mayer's first memoir attentively, and am unable to affix a definite meaning to the statement of Prof. Thomson that it was "by a lucky chance he got a true result from an utterly false analogy." The object of Mayer's paper, as he himself informs us, is to give to our ideas of "force" the same precision as to our ideas of "matter." He finds in the universe two systems of causes, from the one to the other of which there is no transition; the members of the one system possess the attributes of ponderability and impenetrability, those of the other system do not; the one consists of the different kinds of matter, the other of the different manifestations of force. The first quality of all causes he affirms to be *indestructibility* (*Unzerstörlichkeit*). A force cannot become nothing; and just as little can a force be produced from nothing. Forces, he affirms, are *indestructible and convertible*, different forms of one and the same object. When a force has wrought an effect equal to itself, it ceases by that act to have an existence. A weight resting upon the earth is not a force; it is incapable of producing motion, incompetent, for example, to lift another weight. A raised weight is a force (potential energy); a falling weight which possesses motion (dynamic energy) is a force. The first force, which consisted simply in the separation of the weight from the

earth, and where no motion is involved, is converted, when the weight is liberated, into the force of motion.

But we see, proceeds Mayer, in numberless cases motion destroyed without producing other motion, or without raising any weight: now a force once in existence cannot become *nil*; hence the question arises, under what other form does the vanished motion make its appearance? Here experiment alone can help us. We must choose materials which, while they enable us to destroy motion, will themselves undergo the least alteration possible. (The precision with which Mayer here defines the conditions of experiment is worthy of notice; he intends the motion to produce heat, and therefore he avoids changes of aggregation.) When we rub together two plates of metal, we see that motion disappears, but we also see heat make its appearance; and the question now is, is the motion the cause of the heat? In the numberless cases in which motion is destroyed and heat generated, has the motion no other effect than the production of heat? has the heat no other cause than the motion?

Thus clearly and distinctly does this original thinker set his problem before him, and he arrives at the conclusion that, without a causal connexion between motion and heat, we can neither account for the disappearance of the one nor the appearance of the other. He finds that by stirring water vigorously he can warm it. In what follows, Mayer's power of generalization comes conspicuously into play. He remarks that heat makes its appearance when the particles of a body approach each other; condensation generates heat. But what is true for the ultimate particles is also true for larger masses. The falling of a body to the earth is actually a diminution of the earth's volume, and must certainly stand in connexion with the heat generated by the fall. This heat is proportional to the mass of the body and the distance through which it has fallen. It is also proportional to the mass multiplied by the square of its velocity*. In water-mills the motion produced by the diminution of the earth's volume by the fall of the water generates continually a considerable amount of heat. Conversely, the steam-engine serves to decompose the heat into motion, or the lifting of weights. The locomotive with its train may be compared to a distilling apparatus. The heat beneath the boiler transforms itself into motion, and this motion again deposits itself as heat in the axles and the wheels. (It would be impossible to find a happier comparison than this.)

* I would invite the reader to compare this outline of Mayer's paper with the first five columns of Prof. Thomson's article in 'Good Words' (October 1862), and thus to prove for himself whether I am right or wrong when I state that these five columns deal substantially with the work of Mayer.

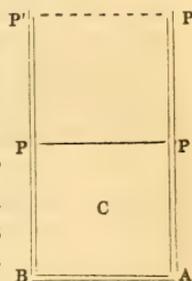
Water changes to steam in the boiler; the steam is reconverted into water in the condenser; the water and the steam are different forms of the same thing. The quantity of steam which condenses is, moreover, neither more nor less than what the boiler yields. So also the heat of the furnace passes into the visible motion of the train; this visible motion again transforms itself into heat at the wheels and axles; heat and motion, like the steam and water, are merely different forms of the same thing (*verschiedene Erscheinungsformen eines und desselben Objects*). And inasmuch as we have in the condenser neither more nor less of steam than the boiler yielded, so in the wheels and axles we have, as heat, all the motion, no more and no less, expended by the furnace to give velocity to the train.

Had Mayer paused at this illustration and not written another word, he would have proved himself, as regards the conservation of energy, in advance of every living man in 1842*. But he went further. Having thus given an outline of his principles, he concludes his paper by what he calls a "practical deduction." To express numerically the relations of force and motion in the case of gravity, it is necessary, he says, to determine experimentally the distance through which a body falls in a given time, say in the first second of its fall. So likewise as regards the relation between motion and heat, we must determine the quantity of heat which corresponds to a certain quantity of motion. We must, for example, determine how high a definite weight must be raised above the earth's surface so that its fall to the earth shall be equivalent to the warming of an equal weight of water 1° C. That such an equation is founded in nature, Mayer regards as the *résumé* of his paper. The term 'equivalent' is here used by Mayer for the first time, and his next act is to indicate how it may be determined numerically, and to give the result of the calculation. In making this calculation he employed the specific heat of air, which was at that time accepted as the most correct. Subsequently the refined experiments of M. Regnault proved the specific heat of air to be somewhat less than what the determinations prior to 1842 had made it. Substituting in Mayer's formula the corrected specific heat, we obtain a number which is almost identical with the mean of all the best determinations of Mr. Joule.

* The relative ripeness of the ideas of Dr. Mayer and Mr. Joule twenty years ago may be inferred from a comparison of the above remarks published in May 1842, with the following passage first published by Mr. Joule in December 1843:—"I shall lose no time in repeating and extending these experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, *indestructible*, and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained."

Mayer's first paper does not contain the details of the calculation; but in his second paper, published in 1845, he formally goes through it. In my 'Lectures on Heat' I have tried to put this subject into its most elementary form; and as many persons may take an interest in this discussion who are not conversant either with the details or history of the mechanical theory of heat, I will ask permission to make a quotation from the work referred to. In calculating the mechanical equivalent of heat, while using my own language I strictly adhere to the method of Mayer.

"Let C be a cylindrical vessel with a base one square foot in area. Let PP mark the upper surface of a cubic foot of air at a temperature of 32° Fahr. The height AP will then be 1 foot. Let the air be heated till its volume is doubled; to effect this it must, as before explained, be raised 273° C. or 490° F. in temperature; and when expanded, its upper surface will stand at P' P', one foot above its initial position. But in rising from PP to P' P' it has forced back the atmosphere, which exerts a pressure of 15 lbs. on every square inch of its upper surface; in other words, it has lifted a weight of $144 \times 15 = 2160$ lbs. to a height of 1 foot.



"The 'capacity' for heat of the air thus expanding is 0.24, water being unity; the weight of our cubic foot of air is 1.29 oz.; hence the quantity of heat required to raise 1.29 oz. of air 490° F. would raise a little less than one-fourth of that weight of water 490°. The exact quantity of water equivalent to our 1.29 oz. of air is $1.29 \times 0.24 = 0.31$ oz.

"But 0.31 oz. of water heated 490° is equal to 152 ozs., or $9\frac{1}{2}$ lbs. heated 1°. Thus the heat imparted to our cubic foot of air, in order to double its volume and enable it to lift a weight of 2160 lbs. 1 foot high, would be competent to raise $9\frac{1}{2}$ lbs. of water one degree in temperature.

"The air has here been heated *under a constant pressure*; and we have learned that the quantity of heat necessary to raise the temperature of a gas under constant pressure a certain number of degrees, is to that required to raise the gas to the same temperature, *when its volume is kept constant*, in the proportion of 1.42 to 1; hence we have the statement,

$$1.42 : 1 = 9.5 \text{ lbs.} : 6.7 \text{ lbs.},$$

which shows that the quantity of heat necessary to augment the temperature of our cubic foot of air at constant volume 490° would heat 6.7 lbs. of water 1°.

"Deducting 6.7 lbs. from 9.5 lbs., we find that the excess of heat imparted to the air, in the case where it is permitted to expand, is competent to raise 2.8 lbs. of water 1° F. in temperature.

"As explained already, this excess is employed to lift the weight of 2160 lbs. 1 foot high. Dividing 2160 by 2·8, we find that the quantity of heat sufficient to raise 1 lb. of water 1° Fahr. in temperature is competent to raise a weight of 771·4 lbs. a foot high.

"This method of calculating the mechanical equivalent of heat was followed by Dr. Mayer, a physician in Heilbronn, Germany, in the spring of 1842.

"Mayer's first paper contains merely an indication of the way in which he had found the equivalent, but does not contain the calculation. The paper was evidently a kind of preliminary note from which date might be taken. In it were enunciated the convertibility and indestructibility of force, and its author referred to the mechanical equivalent of heat merely in illustration of his principles. Had this first paper stood alone, Mayer's relation to the dynamical theory of heat would be very different from what it now is; but in 1845 he published an 'Essay on Organic Motion,' which, though exception might be taken to it here and there, is, on the whole, a production of extraordinary merit. This was followed in 1848 by an 'Essay on Celestial Dynamics,' in which, with remarkable boldness, sagacity, and completeness, he developed the meteoric theory of the sun. Taking him all in all, the right of Mayer to stand, as a man of true genius, in the front rank of the founders of the dynamical theory of heat, cannot be disputed."

For the sake of completeness, I will append here the *résumé* which I have given of the determinations of Mr. Joule.

"On August 21, 1843, Mr. Joule communicated a paper to the British Association, then meeting at Cork, and in the third part of this paper* he describes a series of experiments on magneto-electricity, executed with a view to determine the 'mechanical value of the heat.' The results of this elaborate investigation gave the following weights raised one foot high as equivalent to the warming of 1 lb. of water 1° Fahr. :—

1.	896 lbs.	5.	1026 lbs.
2.	1001 „	6.	587 „
3.	1040 „	7.	742 „
4.	910 „	8.	860 „

"In 1844 Mr. Joule deduced from experiments on the condensation of air, the following equivalents to 1 lb. of water heated 1° Fahr. :—

823 foot-pounds.
795 „
820 „
814 „
760 „

* Phil. Mag. 1843, vol. xxiii. p. 435.

“As the experience of the experimenter increased, we find that the coincidence of his results becomes closer. In 1845 Mr. Joule deduced from experiments with water agitated by a paddle-wheel an equivalent of

890 foot-pounds.

“Summing up his results in 1845, and taking the mean, he found the equivalent to be

817 foot-pounds.

“In 1847 he found the mean of two experiments to give as equivalent

781·8 foot-pounds.

“Finally, in 1849, applying all the precautions suggested by seven years' experience, he obtained the following numbers for the mechanical equivalent of heat:—

772·692 from friction of water, mean of 40 experiments.

774·083 ” mercury, ” 50 ”

774·987 ” cast iron, ” 20 ”

“For reasons assigned in his paper, Mr. Joule fixes the exact equivalent of heat at

772 foot-pounds.

“According to the method pursued by Mayer in 1842, the mechanical equivalent of heat is

771·4 foot-pounds.

“Such a coincidence relieves the mind of every shade of uncertainty regarding the correctness of our present mechanical equivalent of heat.”

It was in connexion with this *résumé* that I wrote the following paragraph, which has been already quoted in the *Philosophical Magazine*.

“Do I refer to these things in order to exalt Mayer at the expense of Joule? It is far from my intention to do so. The man who through long years, without encouragement, and in the face of difficulties which might well be deemed insurmountable, could work with such unswerving steadfastness of purpose to so triumphant an issue, is safe from depreciation. And it is not the experiments alone, but the spirit which they incorporate, and the applications which their author made of them, that entitle Mr. Joule to a place in the foremost rank of physical philosophers. Mayer's labours have in some measure the stamp of a profound intuition, which rose, however, to the energy of undoubting conviction in the author's mind. Joule's labours, on the contrary, are an experimental demonstration. True to the speculative instinct of his country, Mayer drew large and weighty

conclusions from slender premises ; while the Englishman aimed, above all things, at the firm establishment of facts. And he did establish them. The future historian of science will not, I think, place these men in antagonism. To each belongs a reputation which will not quickly fade, for the share he has had, not only in establishing the dynamical theory of heat, but also in leading the way towards a right appreciation of the general energies of the universe."

It will be observed that Mayer implies in his calculation that the excess of heat imparted to the gas when it expands under constant pressure is all consumed in the lifting of the weight, that there is none of it expended in overcoming the mutual attractions of the gaseous molecules. Whether Mayer, at the time he made his calculation, had really fixed his attention on the possibility of some of the heat being consumed in surmounting these attractions does not appear from his first paper ; all that appears there is, that he fixed upon a substance (atmospheric air) which was capable of giving a correct result. There were, moreover, existing at the time data which made it all but certain that atmospheric air was a body of this character. It obeyed the law of Mariotte, its density being through a large range proportional to the external pressure. If the condensation of the air had been in part due to the mutual action of its own atoms—if the external pressure and internal attractions had been thus complicated—it is all but certain that the law of Mariotte could not have been obeyed. Besides, the same law applied to hydrogen as to air ; hence if the atoms exercised any sensible influence upon each other, the hydrogen atoms must be assumed to act precisely in the same manner as the oxygen atoms. It would also have to be assumed that the action of two molecules of the same gas exert precisely the same action upon each other as two molecules of different gases*. Gay-Lussac's experiments had, moreover, proved that in the expansion of air, where no external work was performed, no heat was absorbed, which could not be the case if molecular attractions had to be overcome. In short, confining ourselves to the data known in 1842, the hypothesis that any *sensible* portion of the heat communicated to the air is applied to the overcoming of internal attractions is cumbered with difficulties and improbabilities sufficient to justify its instant rejection. How far Mayer entered into these considerations in 1842 we know not ; all that we know is, that what he did was correct. It is not a "reconstruction or destruction of thermo-dynamics" that those considerations involve, but simply a reconstruction of Professor William Thomson's ideas regarding Dr. Mayer †.

* On this point see also Verdet, p. 44.

† If my knowledge be correct, the circumstances which might affect the

One point more remains to be considered. Prof. Thomson denies everything to Mayer save a combination of a lucky chance and a false analogy; and he asks me, do I not know "that even on this point he had been anticipated by Séguin, who, three years before the appearance of Mayer's paper, had obtained and published the same numerical result from the same hypothesis?" I must frankly confess that I did not know this, nor am I yet aware of it. It would be a source of lively regret to me to find I had done injustice to M. Séguin. There cannot be a doubt that he expressed many correct ideas regarding the transformation of heat into motion in his work *Sur l'influence des chemins de fer*, published in 1839, and which ideas he affirms that he had derived from his uncle, the celebrated Montgolfier. Knowing this, I stated in the preface to my 'Lectures' that he, M. Séguin, stands "in honourable relationship to the dynamical theory of heat." I was aware that, at page 389 of the

derivation of the mechanical equivalent of heat from the properties of gases were first clearly laid down by Clausius. In connexion with this point he writes as follows (Phil. Mag. 1856, vol. xii. p. 242):—"In the absence of more accurate knowledge, it was formerly assumed, in determining the volumes of the unit of weight of saturated vapour at different temperatures, that vapour, even at its maximum density, still obeys Mariotte's and Gay-Lussac's laws. In opposition to this, I have already shown in my first memoir* on this subject, that the volumes in question can be calculated from the principles of the mechanical theory of heat, under the assumption that a permanent gas when it expands at a constant temperature only absorbs so much heat as is consumed in the external work thereby performed, and that these calculations lead to values which, at least at high temperatures, differ considerably from Mariotte's and Gay-Lussac's laws.

"Even the physicists who had occupied themselves more especially with the mechanical theory of heat did not at that time coincide with this view of the department of vapour. William Thomson in particular opposed it. In a memoir† presented to the Royal Society of Edinburgh, and published a year later, in March 1851, he only regarded this result as a proof of the improbability of the above assumption which I had employed.

"Since then, however, he and J. P. Joule have undertaken to test experimentally the accuracy of this assumption‡. By a series of well-contrived experiments, executed on a large scale, they have in fact shown that, with respect to the permanent gases, atmospheric air and hydrogen, the assumption is so nearly true, that in most calculations the deviations from exactitude may be disregarded. With carbonic acid, the non-permanent gas they investigated, the deviations were greater. This is in perfect accordance with the remark I made on first making the assumption, which was that the latter would probably be found to be accurate in the same measure as Mariotte's and Gay-Lussac's laws were applicable to the gas. In consequence of these experiments, Thomson now calculates the volumes of saturated vapours in the same manner as myself."

* Poggendorff's *Annalen*, vol. lxxix. p. 368. Phil. Mag. July 1851.

† Transactions of the Royal Society of Edinburgh, vol. xx. part 2. p. 261.

‡ Phil. Trans. vol. cxliii. part 3. p. 357; and vol. cxliv. part 2. p. 321.

work referred to, he gave a series of numbers, and that his strenuous supporter in putting his claims above those of Mr. Joule, afterwards pointed out that they contained "implicitly" the mechanical equivalent of heat. I was aware that in 1847 he had reduced these numbers so as to express the mechanical equivalent of heat in the usual form, his determinations varying from 395 to 529 grammes raised to a height of 1 metre. (That of Mayer, according to the precise calculations of M. Regnault, is 426.) I thought, moreover, that M. Séguin had operated upon *steam*, and that steam, as he used it, could not give a correct result. I further knew that the intimate friend of M. Séguin, Mr. Grove, had stated, in a lecture given at the Royal Institution on Friday, 25th January 1856, that "since the accurate and elaborate experiments of M. Regnault, M. Séguin has necessarily varied his estimate, as by those experiments it appears that, within certain limits, for elevating the temperature of compressed vapour by one degree, no more than about $\frac{3}{10}$ ths of a degree of total heat is required; consequently the equivalent multiplied in this ratio would be 1666 grammes instead of 500." This would be nearly four times Mayer's equivalent. Perhaps Prof. Thomson has access to sources of information which are unknown to me, and therefore I do not presume to deny his statement that M. Séguin obtained "*the same numerical result*" as Mayer, three years before him. Prof. Thomson will no doubt produce his authority.

But I must here say distinctly that I would not for an instant allow my estimate of Mayer to depend upon his determination of the mechanical equivalent of heat. It is the insight which he had obtained, in advance of all other men, regarding the relationship of the general energies of the universe, as illustrated in the whole of his writings, that gives him his claim to my esteem and admiration*. In 1842, when he published his first

* I am far from saying that Mayer had mastered the entire details and developments of the conservation of force, or that he is free from error; but he it was who first clearly grasped the philosophy of the subject; and what was done previous to 1842 is to what Mayer did as twilight is to the light of day. His equals—possibly his superiors—have since appeared upon the field; but the generous-minded among them, instead of looking down upon the man, will acknowledge him as a successful pioneer into a region which promises possessions richer than any hitherto granted to the intellect of man. On one point in particular, Mayer, I think, allowed his caution to cripple his philosophy. He evidently feared having anything to do with *atoms* or their motions, and hence could hardly be said to have realized the complete physical conception of the dynamical theory of heat. Probably, however, many share his caution, and adhere to the external facts without seeking to penetrate the molecular actions which underlie them. It must also be remembered that Mayer's education was that of a doctor of medicine, the power of original genius alone enabling him to break through the limitations which his culture tended to impose.

short paper in Liebig's *Annalen*, he had no equal. In 1845, when he published his 'Organic Motion in connexion with Nutrition,' he had, in this field, no equal; and in 1848, when he published his 'Celestial Dynamics,' he was without an equal in this particular domain. This I state as my profound and deliberate conviction, but I should be extremely sorry to throw mud at any man who holds a different belief. Happily the English public will soon have an opportunity of forming its own judgment on these matters from the translations of Mayer's papers which are now appearing in the *Philosophical Magazine*.

LIII. *On Celestial Dynamics*. By Dr. J. R. MAYER.

[Continued from p. 248.]

THE movements of celestial bodies in an absolute vacuum would be as uniform as those of a mathematical pendulum, whereas a resisting medium pervading all space would cause the planets to move in shorter and shorter orbits, and at last to fall into the sun.

Assuming such a resisting medium, these wandering celestial bodies must have on the periphery of the solar system their cradle, and in its centre their grave; and however long the duration, and however great the number of their revolutions may be, as many masses will on the average in a certain time arrive at the sun as formerly in a like period of time came within his sphere of attraction.

All these bodies plunge with a violent impetus into their common grave. Since no cause exists without an effect, each of these cosmical masses will, like a weight falling to the earth, produce by its percussion an amount of heat proportional to its *vis viva*.

From the idea of a sun whose attraction acts throughout space, of ponderable bodies scattered throughout the universe, and of a resisting æther, another idea necessarily follows—that, namely, of a continual and inexhaustible generation of heat on the central body of this cosmical system.

Whether such a conception be realized in our solar system—whether, in other words, the wonderful and permanent evolution of light and heat be caused by the uninterrupted fall of cosmical matter into the sun—will now be more closely examined.

The existence of matter in a primordial condition (*Urmaterie*), moving about in the universe, and assumed to follow the attraction of the nearest stellar system, will scarcely be denied by astronomers and physicists; for the richness of surrounding nature, as well as the aspect of the starry heavens, prevents the belief that the wide space which separates our solar system from

the regions governed by the other fixed stars is a vacant solitude destitute of matter. We shall leave, however, all suppositions concerning subjects so distant from us both in time and space, and confine our attention exclusively to what may be learnt from the observation of the existing state of things.

Besides the fourteen known planets with their eighteen satellites, a great many other cosmical masses move within the space of the planetary system, of which the comets deserve to be mentioned first.

Kepler's celebrated statement that "there are more comets in the heavens than fish in the ocean," is founded on the fact that, of all the comets belonging to our solar system, comparatively few can be seen by the inhabitants of the earth, and therefore the not inconsiderable number of actually observed comets obliges us, according to the rules of the calculus of probabilities, to assume the existence of a great many more beyond the sphere of our vision.

Besides planets, satellites, and comets, another class of celestial bodies exists within our solar system. These are masses which, on account of their smallness, may be considered as cosmical atoms, and which Arago has appropriately called asteroids. They, like the planets and the comets, are governed by gravity, and move in elliptical orbits round the sun. When accident brings them into the immediate neighbourhood of the earth, they produce the phenomena of shooting-stars and fireballs.

It has been shown by repeated observation, that on a bright night twenty minutes seldom elapse without a shooting-star being visible to an observer in any situation. At certain times these meteors are observed in astonishingly great numbers; during the meteoric shower at Boston, which lasted nine hours, when they were said to fall "crowded together like snow-flakes," they were estimated as at least 240,000. On the whole, the number of asteroids which come near the earth in the space of a year must be computed to be many thousands of millions. This, without doubt, is only a small fraction of the number of asteroids that move round the sun, which number, according to the rules of the calculus of probabilities, approaches the infinite.

As has been already stated, on the existence of a resisting æther it depends whether the celestial bodies, the planets, the comets, and the asteroids move at constant mean distances round the sun, or whether they are constantly approaching that central body.

Scientific men do not doubt the existence of such an æther. Littrow, amongst others, expresses himself on this point as follows:—"The assumption that the planets and the comets move in an absolute vacuum can in no way be admitted. Even if the

space between celestial bodies contained no other matter than that necessary for the existence of light (whether light be considered as emission of matter or the undulations of a universal æther), this alone is sufficient to alter the motion of the planets in the course of time and the arrangement of the whole system itself; the fall of all the planets and the comets into the sun and the destruction of the present state of the solar system must be the final result of this action."

A direct proof of the existence of such a resisting medium has been furnished by the academician Encke. He found that the comet named after him, which revolves round the sun in the short space of 1207 days, shows a regular acceleration of its motion, in consequence of which the time of each revolution is shortened by about six hours.

From the great density and magnitude of the planets, the shortening of the diameters of their orbits proceeds, as might be expected, very slowly, and is up to the present time inappreciable. The smaller the cosmical masses are, on the contrary, other circumstances remaining the same, the faster they move towards the sun; it may therefore happen that in a space of time wherein the mean distance of the earth from the sun would diminish one metre, a small asteroid would travel more than one thousand miles towards the central body.

As cosmical masses stream from all sides in immense numbers towards the sun, it follows that they must become more and more crowded together as they approach thereto. The conjecture at once suggests itself that the zodiacal light, the nebulous light of vast dimensions which surrounds the sun, owes its origin to such closely-packed asteroids. However it may be, this much is certain, that this phenomenon is caused by matter which moves according to the same laws as the planets round the sun, and it consequently follows that the whole mass which originates the zodiacal light is continually approaching the sun and falling into it.

This light does not surround the sun uniformly on all sides; that is to say, it has not the form of a sphere, but that of a thin convex lens, the greater diameter of which is in the plane of the solar equator, and accordingly it has to an observer on our globe a pyramidal form. Such lenticular distribution of the masses in the universe is repeated in a remarkable manner in the disposition of the planets and the fixed stars.

From the great number of cometary masses and asteroids and the zodiacal light on the one hand, and the existence of a resisting æther on the other, it necessarily follows that ponderable matter

must continually be arriving on the solar surface. The effect produced by these masses evidently depends on their final velocity; and, in order to determine the latter, we shall discuss some of the elements of the theory of gravitation.

The final velocity of a weight attracted by and moving towards a celestial body will become greater as the height through which the weight falls increases. This velocity, however, if it be only produced by the fall, cannot exceed a certain magnitude; it has a maximum, the value of which depends on the volume and mass of the attracting celestial body.

Let r be the radius of a spherical and solid celestial body, and g the velocity at the end of the first second of a weight falling on the surface of this body; then the greatest velocity which this weight can obtain by its fall towards the celestial body, or the velocity with which it will arrive at its surface after a fall from an infinite height, is $\sqrt{2gr}$ in one second. This number, wherein g and r are expressed in metres, we shall call G .

For our globe the value of g is 9.8164 .. and that of r 6,369,800; and consequently on our earth

$$G = \sqrt{2 \times 9.8164 \times 6,369,800} = 11,183.$$

The solar radius is 112.05 times that of the earth, and the velocity produced by gravity on the sun's surface is 28.36 times greater than the same velocity on the surface of our globe; the greatest velocity therefore which a body could obtain in consequence of the solar attraction, or

$$G = \sqrt{28.36 \times 112.05} \times 11,183 = 630,400;$$

that is, this maximum velocity is equal to 630,400 metres, or 85 geographical miles in one second.

By the help of this constant number, which may be called the *characteristic* of the solar system, the velocity of a body in central motion may easily be determined at any point of its orbit. Let a be the mean distance of the planetary body from the centre of gravity of the sun, or the greater semidiameter of its orbit (the radius of the sun being taken as unity); and let h be the distance of the same body at any point of its orbit from the centre of gravity of the sun; then the velocity, expressed in metres, of the planet at the distance h is

$$G \times \sqrt{\frac{2a-h}{2a \times h}}$$

At the moment the planet comes in contact with the solar surface, h is equal to 1, and its velocity is therefore,

$$G \times \sqrt{\frac{2a-1}{2a}}$$

It follows from this formula that the smaller $2a$ (or the major axis of the orbit of a planetary body) becomes, the less will be its velocity when it reaches the sun. This velocity, like the major axis, has a minimum; for so long as the planet moves outside the sun, its major axis cannot be shorter than the diameter of the sun, or, taking the solar radius as a unit, the quantity $2a$ can never be less than 2. The smallest velocity with which we can imagine a cosmical body to arrive on the surface of the sun is consequently

$$G \times \sqrt{\frac{1}{2}} = 445,750,$$

or a velocity of 60 geographical miles in one second.

For this smallest value the orbit of the asteroid is circular; for a larger value it becomes elliptical, until finally, with increasing excentricity, when the value of $2a$ approaches infinity, the orbit becomes a parabola. In this last case the velocity is

$$G \times \sqrt{\frac{\infty - 1}{\infty}} = G,$$

or 85 geographical miles in one second.

If the value of the major axis become negative, or the orbit assume the form of a hyperbola, the velocity may increase without end. But this could only happen when cosmical masses enter the space of the solar system with a projected velocity, or when masses, having missed the sun's surface, move into the universe and never return; hence a velocity greater than G can only be regarded as a rare exception, and we shall therefore only consider velocities comprised within the limits of 60 and 80 miles*.

The final velocity with which a weight moving in a straight line towards the centre of the sun arrives at the solar surface is expressed by the formula

$$c = G \times \sqrt{\frac{h-1}{h}},$$

wherein c expresses the final velocity in metres, and h the original distance from the centre of the sun in terms of solar radius. If this formula be compared with the foregoing, it will be seen that a mass which, after moving in central motion, arrives at the sun's surface has the same velocity as it would possess had it fallen perpendicularly into the sun from a distance† equal to the major

* The relative velocity also with which an asteroid reaches the solar surface depends in some degree on the velocity of the sun's rotation. This, however, as well as the rotatory effect of the asteroid, is without moment, and may be neglected.

† This distance is to be counted from the centre of the sun.

axis of its orbit ; whence it is apparent that a planet, on arriving at the sun, moves at least as quickly as a weight which falls freely towards the sun from a distance as great as the solar radius, or 96,000 geographical miles.

What thermal effect corresponds to such velocities? Is the effect sufficiently great to play an important part in the immense development of heat on the sun?

This crucial question may be easily answered by help of the preceding considerations. According to the formula given at the end of Chapter II., the degree of heat generated by percussion is

$$= 0.000139^{\circ} \times c^2,$$

where c denotes the velocity of the striking body expressed in metres. The velocity of an asteroid when it strikes the sun measures from 445,750 to 630,400 metres; the calorific effect of the percussion is consequently equal to from $27\frac{1}{2}$ to 55 millions of degrees of heat*.

An asteroid, therefore, by its fall into the sun develops from 4600 to 9200 times as much heat as would be generated by the combustion of an equal mass of coal.

V. *The Origin of the Sun's Heat (continuation).*

The question why the planets move in curved orbits, one of the grandest of problems, was solved by Newton in consequence, it is believed, of his reflecting on the fall of an apple. This story is not improbable, for we are on the right track for the discovery of truth when once we clearly recognize that between great and small no qualitative but only a quantitative difference exists—when we resist the suggestions of an ever active imagination, and look for the same laws in the greatest as well as in the smallest processes of nature.

This universal range is the essence of a law of nature, and the touchstone of the correctness of human theories. We observe the fall of an apple, and investigate the law which governs this phenomenon; for the earth we substitute the sun, and for the apple a planet, and thus possess ourselves of the key to the mechanics of the heavens.

As the same laws prevail in the greater as well as in the smaller processes of nature, Newton's method may be used in solving the problem of the origin of the sun's heat. We know the connexion between the space through which a body falls, the velocity, the *vis viva*, and the generation of heat on the surface

[* Throughout this memoir the degrees of heat are expressed in the Centigrade scale. Unless stated to the contrary, the measures of length are given in geographical miles. A geographical mile = 7420 metres, and an English mile = 1608 metres.—Tr.]

of this globe; if we again substitute for the earth the sun, with a mass 350,000 greater, and for a height of a few metres celestial distances, we obtain a generation of heat exceeding all terrestrial measures. And since we have sufficient reason to assume the actual existence of such mechanical processes in the heavens, we find therein the only tenable explanation of the origin of the heat of the sun.

The fact that the development of heat by mechanical means on the surface of our globe is, as a rule, not so great, and cannot be so great as the generation of the same agent by chemical means, as by combustion, follows from the laws already discussed; and this fact cannot be used as an argument against the assumption of a greater development of heat by a greater expenditure of mechanical work. It has been shown that the heat generated by a weight falling from a height of 367 metres is only $\frac{1}{6000}$ th part of the heat produced by the combustion of the same weight of coal; just as small is the amount of heat developed by a weight moving with the not inconsiderable velocity of 85 metres in one second. But, according to the laws of mechanics, the effect is proportional to the square of the velocity; if therefore the weight move 100 times faster, or with a velocity of 8500 metres in one second, it will produce a greater effect than the combustion of an equal quantity of coal.

It is true that so great a velocity cannot be obtained by human means; everyday experience, however, shows the development of high degrees of temperature by mechanical processes.

In the common flint and steel, the particles of steel which are struck off are sufficiently heated to burn in air. A few blows directed by a skilful blacksmith with a sledge-hammer against a piece of cold metal may raise the temperature of the metal at the points of collision to redness.

The new crank of a steamer, whilst being polished by friction, becomes red-hot, several buckets of water being required to cool it down to its ordinary temperature.

When a railway train passes with even less than its ordinary velocity along a very sharp curve of the line, sparks are observed in consequence of the friction against the rails.

One of the grandest constructions for the production of motion by human art is the channel in which the wood was allowed to glide down from the steep and lofty sides of Mount Pilatus into the plain below. This wooden channel, which was built about thirty years ago by the engineer Rupp, was 9 English miles in length; the largest trees were shot down it from the top to the bottom of the mountain in about two minutes and a half. The momentum possessed by the trees on their escaping at their journey's end from the channel was sufficiently great to bury

their thicker ends in the ground to the depth of from 6 to 8 metres. To prevent the wood getting too hot and taking fire, water was conducted in many places into the channel.

This stupendous mechanical process, when compared with cosmical processes on the sun, appears infinitely small. In the latter case it is the mass of the sun which attracts, and in lieu of the height of Mount Pilatus we have distances of a hundred thousand and more miles; the amount of heat generated by cosmical falls is therefore at least 9 million times greater than in our terrestrial example.

Rays of heat on passing through glass and other transparent bodies undergo partial absorption, which differs in degree, however, according to the temperature of the source from which the heat is derived. Heat radiated from sources less warm than boiling water is almost completely stopped by thin plates of glass. As the temperature of a source of heat increases, its rays pass more copiously through diathermic bodies. A plate of glass, for example, weakens the rays of a red-hot substance, even when the latter is placed very close to it, much more than it does those emanating at a much greater distance from a white-hot body. If the quality of the sun's rays be examined in this respect, their diathermic energy is found to be far superior to that of all artificial sources of heat. The temperature of the focus of a concave metallic reflector in which the sun's light has been collected is only diminished from one-seventh to one-eighth by the interposition of a screen of glass. If the same experiment be made with an artificial and luminous source of heat, it is found that, though the focus be very hot when the screen is away, the interposition of the latter cuts off nearly all the heat; moreover, the focus will not recover its former temperature when reflector and screen are placed sufficiently near to the source of heat to make the focus appear brighter than it did in the former position without the glass screen.

The empirical law, that the diathermic energy of heat increases with the temperature of the source from which the heat is radiated, teaches us that the sun's surface must be much hotter than the most powerful process of combustion could render it.

Other methods furnish the same conclusion. If we imagine the sun to be surrounded by a hollow sphere, it is clear that the inner surface of this sphere must receive all the heat radiated from the sun. At the distance of our globe from the sun, such a sphere would have a radius 215 times as great, and an area 46,000 times as large as the sun himself; those luminous and calorific rays, therefore, which meet this spherical surface at

right angles retain only $\frac{1}{46,000}$ th part of their original intensity. If it be further considered that our atmosphere absorbs a part of the solar rays, it is clear that the rays which reach the tropics of our earth at noonday can only possess from $\frac{1}{50,000}$ th to $\frac{1}{60,000}$ th of the power with which they started. These rays, when gathered from a surface of from 5 to 6 square metres, and concentrated in an area of one square centimetre, would produce about the temperature which exists on the sun, a temperature more than sufficient to vaporize platinum, rhodium, and similar metals.

The radiation calculated in Chapter III. likewise proves the enormous temperature of the solar surface. From the determination mentioned therein, it follows that each square centimetre of the sun's surface loses by radiation about 80 units of heat per minute—an immense quantity in comparison with terrestrial radiations.

A correct theory of the origin of the sun's heat must explain the cause of such enormous temperatures. This explanation can be deduced from the foregoing statements. According to Pouillet, the temperature at which bodies appear intensely white-hot is about 1500° C. The heat generated by the combustion of one kilogramme of hydrogen is, as determined by Dulong, 34,500, and according to the more recent experiments of Grassi, 34,666 units of heat. One part of hydrogen combines with eight parts of oxygen to form water; hence one kilogramme of these two gases mixed in this ratio would produce 3850° .

Let us now compare this heat with the amount of the same agent generated by the fall of an asteroid into the sun. Without taking into account the low specific heat of such masses when compared with that of water, we find the heat developed by the asteroid to be from 7000 to 14,000 times greater than that of the oxyhydrogen mixture. From data like these, the extraordinary diathermic energy of the sun's rays, the immense radiation from his surface, and the high temperature in the focus of the reflector are easily accounted for.

The facts above mentioned show that, unless we assume on the sun the existence of matter with unheard of chemical properties as a *deus ex machinâ*, no chemical process could maintain the present high radiation of the sun; it also follows from the above results, that the chemical nature of bodies which fall into the sun does not in the least affect our conclusions; the effect produced by the most inflammable substance would not differ by one-thousandth part from that resulting from the fall of matter possessing but feeble chemical affinities. As the brightest artificial light appears dark in comparison with the sun's light, so the mechanical processes of the heavens throw into the shade the most powerful chemical actions.

The quality of the sun's rays, as dependent on his temperature, is of the greatest importance to mankind. If the solar heat were originated by a chemical process, and amounted near its source to a temperature of a few thousand degrees, it would be possible for the light to reach us, whilst the greater part of the more important calorific rays would be absorbed by the higher strata of our atmosphere and then returned to the universe.

In consequence of the high temperature of the sun, however, our atmosphere is highly diathermic to his rays, so that the latter reach the surface of our earth and warm it. The comparatively low temperature of the terrestrial surface is the cause why the heat cannot easily radiate back through the atmosphere into the universe. The atmosphere acts, therefore, like an envelope, which is easily pierced by the solar rays, but which offers considerable resistance to the radiant heat escaping from our earth; its action resembles that of a valve which allows liquid to pass freely in one, but stops the flow in the opposite direction.

The action of the atmosphere is of the greatest importance as regards climate and meteorological processes. It must raise the mean temperature of the earth's surface. After the setting of the sun—in fact, in all places where his rays do not reach the surface, the temperature of the earth would soon be as low as that of the universe, if the atmosphere were removed, or if it did not exist. Even the powerful solar rays in the tropics would be unable to preserve water in its liquid state.

Between the great cold which would reign at all times and in all places, and the moderate warmth which in reality exists on our globe, intermediate temperatures may be imagined; and it is easily seen that the mean temperature would decrease if the atmosphere were to become more and more rare. Such a rarefaction of a valve-like acting atmosphere actually takes place as we ascend higher and higher above the level of the sea, and it is accordingly and necessarily accompanied by a corresponding diminution of temperature.

This well-known fact of the lower mean temperature of places of greater altitude has led to the strangest hypotheses. The sun's rays were not supposed to contain all the conditions for warming a body, but to set in motion the "substance" of heat contained in the earth. This "substance" of heat, cold when at rest, was attracted by the earth, and was therefore found in greater abundance near the centre of the globe. This view, it was thought, explained why the warming power of the sun was so much weaker at the top of a mountain than at the bottom, and why, in spite of his immense radiation, he retained his full powers.

This belief, which especially prevails amongst imperfectly in-

formed people, and which will scarcely succumb to correct views, is directly contradicted by the excellent experiments made by Pouillet at different altitudes with the pyrliometer. These experiments show that, everything else being equal, the generation of heat by the solar rays is more powerful in higher altitudes than near the surface of our globe, and that consequently a portion of these rays is absorbed on their passage through the atmosphere. Why, in spite of this partial absorption, the mean temperature of low altitudes is nevertheless higher than it is in more elevated positions, is explained by the fact that the atmosphere stops to a far greater degree the calorific rays emanating from the earth than it does those from the sun.

VI. *The Constancy of the Sun's Mass.*

Newton, as is well known, considered light to be the emission of luminous particles from the sun. In the continued emission of light this great philosopher saw a cause tending to diminish the solar mass; and he assumed, in order to make good this loss, comets and other cosmical masses to be continually falling into the central body.

If we express this view of Newton's in the language of the undulatory theory, which is now universally accepted, we obtain the results developed in the preceding pages. It is true that our theory does not accept a peculiar "substance" of light or of heat; nevertheless, according to it, the radiation of light and heat consists also in purely material processes, in a sort of motion, in the vibrations of ponderable resisting substances. Quiescence is darkness and death; motion is light and life.

An undulating motion proceeding from a point or a plane and excited in an unlimited medium, cannot be imagined apart from another simultaneous motion, a translation of the particles themselves*; it therefore follows, not only from the emission, but also from the undulatory theory, that radiation continually diminishes the mass of the sun. Why, nevertheless, the mass of the sun does not really diminish has already been stated.

The radiation of the sun is a centrifugal action equivalent to a centripetal motion.

The calorific effect of the centrifugal action of the sun can be found by direct observation; it amounts, according to Chap. III., in one minute to 12,650 millions of cubic miles of heat, or 5.17 quadrillions of units of heat. In Chapter IV. it has been shown that one kilogramme of the mass of an asteroid originates from 27.5 to 55 millions of units of heat; the quantity of cosmical

* This centrifugal motion is perhaps the cause of the repulsion of the tails on comets when in the neighbourhood of the sun, as observed by Bessel.

masses, therefore, which falls every minute into the sun amounts to from 94,000 to 188,000 billions of kilogrammes.

To obtain this remarkable result, we made use of a method which is common in physical inquiries. Observation of the moon's motion reveals to us the external form of the earth. The physicist determines with the torsion-balance the weight of a planet, just as a merchant finds the weight of a parcel of goods, whilst the pendulum has become a magic power in the hands of the geologist, enabling him to discover cavities in the bowels of the earth. Our case is similar to these. By observation and calculation of the velocity of sound in our atmosphere, we obtain the ratio of the specific heat of air under constant pressure and under constant volume, and by the help of this number we determine the quantity of heat generated by mechanical work. The heat which arrives from the sun in a given time on a small surface of our globe serves as a basis for the calculation of the whole radiating effect of the sun; and the result of a series of observations and well-founded conclusions is the quantitative determination of those cosmical masses which the sun receives from the space through which he sends forth his rays.

Measured by terrestrial standards, the ascertained number of so many billions of kilogrammes per minute appears incredible. This quantity, however, may be brought nearer to our comprehension by comparison with other cosmical magnitudes. The nearest celestial body to us (the moon) has a mass of about 90,000 trillions of kilogrammes, and it would therefore cover the expenditure of the sun for from one to two years. The mass of the earth would afford nourishment to the sun for a period of from 60 to 120 years.

To facilitate the appreciation of the masses and the distances occurring in the planetary system, Herschel draws the following picture. Let the sun be represented by a globe 1 metre in diameter. The nearest planet (Mercury) will be about as large as a pepper-corn, $3\frac{1}{2}$ millimetres in thickness, at a distance of 40 metres. 78 and 107 metres distant from the sun will move Venus and the Earth, each 9 millimetres in diameter, or a little larger than a pea. Not much more than a quarter of a metre from the Earth will be the Moon, the size of a mustard seed, $2\frac{1}{2}$ millimetres in diameter. Mars, at a distance of 160 metres, will have about half the diameter of the Earth; and the smaller planets (Vesta, Hebe, Astrea, Juno, Pallas, Ceres, &c.), at a distance of from 250 to 300 metres from the sun, will resemble particles of sand. Jupiter and Saturn, 560 and 1000 metres distant from the centre, will be represented by oranges, 10 and 9 centimetres in diameter. Uranus, of the size of a nut 4 centimetres across, will be 2000 metres; and Neptune, as large as an apple

6 centimetres in diameter, will be nearly twice as distant, or about half a geographical mile away from the sun. From Neptune to the nearest fixed star will be more than 2000 geographical miles.

To complete this picture, it is necessary to imagine finely-divided matter grouped in a diversified manner, moving slowly and gradually towards the large central globe, and on its arrival attaching itself thereto; this matter, when favourably illuminated by the sun, represents itself to us as the zodiacal light. This nebulous substance forms also an important part of a creation in which nothing is by chance, but wherein all is arranged with Divine foresight and wisdom.

The surface of the sun measures 115,000 millions of square miles, or $6\frac{1}{3}$ trillions of square metres; the mass of matter which in the shape of asteroids falls into the sun every minute is from 94,000 to 188,000 billions of kilogrammes; one square metre of solar surface, therefore, receives on an average from 15 to 30 grammes of matter per minute.

To compare this process with a terrestrial phenomenon, a gentle rain may be considered which sends down in one hour a layer of water 1 millimetre in thickness (during a thunder-storm the rainfall is often from ten to fifteen times this quantity); this amounts on a square metre to 17 grammes per minute.

The continual bombardment of the sun by these cosmical masses ought to increase its volume as well as its mass, if centrifugal* action only existed. The increase of volume could scarcely be appreciated by man; for if the specific gravity of these cosmical masses be assumed to be the same as that of the sun, the enlargement of his apparent diameter to the extent of one second, the smallest appreciable magnitude, would require from 33,000 to 66,000 years.

Not quite so inappreciable would be the increase of the mass of the sun. If this mass, or the weight of the sun, were augmented, an acceleration of the motion of the planets in their orbits would be the consequence, whereby their times of revolution round the central body would be shortened. The mass of the sun is 2.1 quintillions of kilogrammes; and the mass of cosmical matter annually arriving at the sun stands to the above as 1 to from 21

42 millions. Such an augmentation of the weight of the sun ought to shorten the sidereal year from $\frac{1}{42,000,000}$ th to $\frac{1}{85,000,000}$ th of its length, or from $\frac{3}{4}$ ths to $\frac{3}{8}$ ths of a second.

The observations of astronomers do not agree with this conclusion; we must therefore fall back on the theory mentioned at

* [Centripetal?—Tr.]

the beginning of this chapter, which assumes that the sun, like the ocean, is constantly losing and receiving equal quantities of matter. This harmonizes with the supposition that the *vis viva* of the universe is a constant quantity.

VII. *The Spots on the Sun's Disc.*

The solar disc presents, according to Sir John Herschel, the following appearance. "When the sun is observed through a powerful telescope provided with coloured glasses in order to lessen the heat and brightness which would be hurtful to the eyes, large dark spots are often seen surrounded by edges which are not quite so dark as the spots themselves, and which are called penumbrae. These spots, however, are neither permanent nor unchangeable. When observed from day to day, or even from hour to hour, their form is seen to change; they expand or contract, and finally disappear; on other parts of the solar surface new spots spring into existence where none could be discovered before. When they disappear, the darker part in the middle of the spot contracts to a point and vanishes sooner than the edges. Sometimes they break up into two or more parts that show all the signs of mobility characteristic of a liquid, and the extraordinary commotion which it seems only possible for gaseous matter to possess. The magnitude of their motion is very great. An arc of 1 second, as seen from our globe, corresponds to 465 English miles on the sun's disc; a circle of this diameter, which measures nearly 220,000 English square miles, is the smallest area that can be seen on the solar surface. Spots, however, more than 45,000 English miles in diameter, and, if we may trust some statements, of even greater dimensions, have been observed. For such a spot to disappear in the course of six weeks (and they rarely last longer), the edges, whilst approaching each other, must move through a space of more than 1000 miles per diem.

"That portion of the solar disc which is free from spots is by no means uniformly bright. Over it are scattered small dark spots or pores, which are found by careful observation to be in a state of continual change. The slow sinking of some chemical precipitates in a transparent liquid, when viewed from the upper surface and in a direction perpendicular thereto, resembles more accurately than any other phenomenon the changes which the pores undergo. The similarity is so striking, in fact, that one can scarcely resist the idea that the appearances above described are owing to a luminous medium moving about in a non-luminous atmosphere, either like the clouds in our air, or in widespread planes and flame-like columns, or in rays like the aurora borealis.

"Near large spots, or extensive groups of them, large spaces are

observed to be covered with peculiarly marked lines much brighter than the other parts of the surface; these lines are curved, or deviate in branches, and are called *faculæ*. Spots are often seen between these lines, or to originate there. These are in all probability the crests of immense waves in the luminous regions of the solar atmosphere, and bear witness to violent action in their immediate neighbourhood."

The changes on the solar surface evidently point to the action of some external disturbing force; for every moving power resident in the sun itself ought to exhaust itself by its own action. These changes, therefore, are no unimportant confirmation of the theory explained in these pages.

At the same time it must be observed that our knowledge of physical heliography is, from the nature of the subject, very limited; even the meteorological processes and other phenomena of our own planet are still in many respects enigmatical. For this reason no special information could be given about the manner in which the solar surface is affected by cosmical masses. However, I may be allowed to mention some probable conjectures which offer themselves.

The extraordinarily high temperature which exists on the sun almost precludes the possibility of its surface being solid; it doubtless consists of an uninterrupted ocean of fiery fluid matter. This gaseous envelope becomes more rarefied in those parts most distant from the sun's centre.

As most substances are able to assume the gaseous state of aggregation at high temperatures, the height of the sun's atmosphere cannot be inconsiderable. There are, however, sound reasons for believing that the relative height of the solar atmosphere is not very great.

Since gravity is 28 times greater on the sun's surface than it is on our earth, a column of air on the former must cause a pressure 28 times greater than it would on our globe. This great pressure compresses air as much as a temperature of 8000° would expand it.

In a still greater degree than this increased gravity do the qualities peculiar to gases affect the height of the solar atmosphere. In consequence of these properties, the density of our atmosphere rapidly diminishes as we ascend, and increases as we descend. Generally speaking, rarefaction increases in a geometrical progression when the heights are in an arithmetical progression. If we ascend or descend $2\frac{1}{2}$, 5, or 30 miles, we find our atmosphere 10, 100, or a billion times more rarefied or more dense.

This law, although modified by the unequal temperatures of the different layers of the photosphere, and the unknown chemical nature of the substances of which it is composed, must

also hold good in some measure for the sun. As, however, the mean temperature of the solar atmosphere must considerably exceed that of our atmosphere, the density of the former will not vary so rapidly with the height as the latter does. If we assume this increase and decrease on the sun to be ten times slower than it is on our earth, it follows that at the heights of 25, 50, and 300 miles a rarefaction of 10, 100, and a billion times respectively would be observed. The solar atmosphere, therefore, does not attain a height of 400 geographical miles, or it cannot be as much as $\frac{1}{240}$ th of the sun's radius. For if we take the density of the lowest strata of the sun's atmosphere to be 1000 times greater than that of our own near the level of the sea, a density greater than that of water, and necessarily too high, then at a height of 400 miles this atmosphere would be 10 billion times less dense than the earth's atmosphere; that is to say, to human comprehension it has ceased to exist.

This discussion shows that the solar atmosphere, in comparison with the body of the sun, has only an insignificant height; at the same time it may be remarked that on the sun's surface, in spite of the great heat, such substances as water may possibly exist in the liquid state under a pressure thousands of times greater than that of our atmosphere.

Since gases, when free from any solid particles, emit, even at very high temperatures, a pale transparent light—the so-called *lumen philosophicum*—it is probable that the intense white light of the sun has its origin in the denser parts of his surface. If such be assumed to be the case, the sun's spots and faculæ seem to be the disturbances of the fiery liquid ocean, caused by most powerful meteoric processes, for which all necessary materials are present, and partly to be caused by the direct influence of streams of asteroids. The deeper and less heated parts of this fiery ocean become thus exposed, and perhaps appear to us as spots, whereas the elevations form the so-called faculæ.

According to the experiments made by Henry, an American physicist, the rays sent forth from the spots do not produce the same heating effect as those emitted by the brighter parts.

We have to mention one more remarkable circumstance. The spots appear to be confined to a zone which extends 30° on each side of the sun's equator. The thought naturally suggests itself that some connexion exists between those solar processes which produce the spots and faculæ, the velocity of rotation of the sun, and the swarms of asteroids, and to deduce therefrom the limitation of the spots to the zone mentioned. It still remains enigmatical by what means nature contrives to bring about the uniform radiation which pertains alike to the polar and equatorial regions of the sun.

VIII. *The Tidal Wave.*

In almost every case the forces and motions on the surface of the earth may be traced back to the rays of the sun. Some processes, however, form a remarkable exception.

One of these is the tides. Beautiful, and in some respects exhaustive researches on this phenomenon have been made by Newton, Laplace, and others. The tides are caused by the attraction exercised by the sun and the moon on the moveable parts of the earth's surface, and by the axial rotation of our globe.

The alternate rising and falling of the level of the sea may be compared to the ascent and descent of a pendulum oscillating under the influence of the earth's attraction.

The continual resistance, however weak it may be, which an instrument of this nature (a physical pendulum) suffers, constantly shortens the amplitude of the oscillations which it performs; and if the pendulum be required to continue in uniform motion, it must receive a constant supply of *vis viva* corresponding to the resistance it has to overcome.

Clocks regulated by a pendulum obtain such a supply, either from a raised weight or a bent spring. The power consumed in raising the weight or in bending the spring, which power is represented by the raised weight or the bent spring, overcomes for a time the resistance, and thus secures the uniform motion of the pendulum and clock. In doing so, the weight sinks down or the spring uncoils, and therefore force must be expended in winding the clock up again, or it would stop moving.

Essentially the same holds good for the tidal wave. The moving waters rub against each other, against the shore, and against the atmosphere, and thus, meeting constantly with resistance, would soon come to rest if a *vis viva* did not exist competent to overcome these obstacles. This *vis viva* is the rotation of the earth on its axis, and the diminution and final exhaustion thereof will be a consequence of such an action.

The tidal wave causes a diminution of the velocity of the rotation of the earth.

This important conclusion can be proved in different ways.

The attraction of the sun and the moon disturbs the equilibrium of the moveable parts of the earth's surface, so as to move the waters of the sea towards the point or meridian above and below which the moon culminates. If the waters could move without resistance, the elevated parts of the tidal wave would exactly coincide with the moon's meridian, and under such conditions no consumption of *vis viva* could take place. In reality, however, the moving waters experience resistance, in

consequence of which the flow of the tidal wave is delayed, and high water occurs in the open sea on the average about $2\frac{1}{2}$ hours after the transit of the moon through the meridian of the place.

The waters of the ocean move from west and east towards the meridian of the moon, and the more elevated wave is, for the reason above stated, always to the east of the moon's meridian; hence the sea must press and flow more powerfully from east to west than from west to east. The ebb and flow of the tidal wave therefore consists not only in an alternate rising and falling of the waters, but also in a slow progressive motion from east to west. The tidal wave produces a general westerly current in the ocean.

This current is opposite in direction to the earth's rotation, and therefore its friction against and collision with the bed and shores of the ocean must offer everywhere resistance to the axial rotation of the earth, and diminish the *vis viva* of its motion. The earth here plays the part of a fly-wheel. The moveable parts of its surface adhere, so to speak, to the relatively fixed moon, and are dragged in a direction opposite to that of the earth's rotation, in consequence of which, action takes place between the solid and liquid parts of this fly-wheel, resistance is overcome, and the given rotatory effect diminished.

Water-mills have been turned by the action of the tides; the effects produced by such an arrangement are distinguished in a remarkable manner from those of a mill turned by a mountain-stream. The one obtains the *vis viva* with which it works from the earth's rotation, the other from the sun's radiation.

Various causes combine to incessantly maintain, partly in an undulatory, partly in a progressive motion, the waters of the ocean. Besides the influence of the sun and the moon on the rotating earth, mention must be made of the influence of the movement of the lower strata of the atmosphere on the surface of the ocean, and of the different temperatures of the sea in various climates; the configuration of the shores and the bed of the ocean likewise exercise a manifold influence on the velocity, direction, and extent of the oceanic currents.

The motions in our atmosphere, as well as those of the ocean, presuppose the existence and consumption of *vis viva* to overcome the continual resistances, and to prevent a state of rest or equilibrium. Generally speaking, the power necessary for the production of aerial currents may be of threefold origin. Either the radiation of the sun, the heat derived from a store in the interior of the earth, or, lastly, the rotatory effect of the earth may be the source.

As far as quantity is concerned, the sun is by far the most im-

portant of the above. According to Pouillet's measurements, a square metre of the earth's surface receives on the average 4.408 units of heat from the sun per minute. Since one unit of heat is equivalent to 367 Km, it follows that one square metre of the surface of our globe receives per minute an addition of *vis viva* equal to 1620 Km, or the whole of the earth's surface in the same time 825,000 billions of Km. A power of 75 Km per second is called a horse-power. According to this, the effect of the solar radiation in mechanical work on one square metre of the earth's surface would be equal to 0.36, and the total effect for the whole globe 180 billions of horse-powers. A not inconsiderable portion of this enormous quantity of *vis viva* is consumed in the production of atmospheric actions, in consequence of which numerous motions are set up in the earth's atmosphere.

In spite of their great variety, the atmospheric currents may be reduced to a single type. In consequence of the unequal heating of the earth in different degrees of latitude, the colder and heavier air of the polar regions passes in an under current towards the equator; whereas the heated air of the tropics ascends to the higher parts of the atmosphere, and flows from thence towards the poles. In this manner the air of each hemisphere performs a circuitous motion.

It is known that these currents are essentially modified by the motion of the earth on its axis. The polar currents, with their smaller rotatory velocity, receive a motion from east to west contrary to the earth's rotation, and the equatorial currents one from west to east in advance of the axial rotation of the earth. The former of these currents, the easterly winds, must diminish the rotatory effect of the globe, the latter, the westerly winds, must increase the same power. The final result of the action of these opposed influences is, as regards the rotation of the earth, according to well-known mechanical principles, = 0; for these currents counteract each other, and therefore cannot exert the least influence on the axial rotation of the earth. This important conclusion was proved by Laplace.

The same law holds good for every imaginable action which is caused either by the radiant heat of the sun, or by the heat which reaches the surface from the earth's interior, whether the action be in the air, in the water, or on the land. The effect of every single motion produced by these means on the rotation of the globe, is exactly compensated by the effect of another motion in an opposite direction; so that the resultant of all these motions is, as far as the axial rotation of the globe is concerned, = 0.

In those actions known as the tides, such compensation; however, does not take place; for the pressure or pull by which they are produced is always stronger from east to west than from west to east. The currents caused by this pull may ebb and flow in different directions, but their motion predominates in that which is opposed to the earth's rotation.

The velocity of the currents caused by the tide of the atmosphere amounts, according to Laplace's calculation, to not more than 75 millimetres in a second, or nearly a geographical mile in twenty-four hours; it is clear that much more powerful effects produced by the sun's heat would hide this action from observation. The influence of these air-currents, however, on the rotatory effect of the earth is, according to the laws of mechanics, exactly the same as it would be were the atmosphere undisturbed by the sun's radiant heat.

The combined motions of air and water are to be regarded from the same point of view. If we imagine the influence of the sun and that of the interior of our globe not to exist, the motion of the air and ocean from east to west is still left as an obstacle to the axial rotation of the earth.

The motion of the waters of the ocean from east to west was long ago verified by observation, and it is certain that the tides are the most effectual of the causes to which this great westerly current is to be referred.

Besides the tidal wave, the lower air-currents moving in the same direction, the trade-winds of the tropics especially, may be assigned as causes of this general movement of the waters. The westerly direction of the latter, however, is not confined to the region of easterly winds; it is met with in the region of perpetual calms, where it possesses a velocity of several miles a day; it is observed far away from the tropics both north and south, in regions where westerly winds prevail, near the Cape of Good Hope, the Straits of Magellan, the arctic regions, &c.

A third cause for the production of a general motion of translation of the waters of the ocean is the unequal heating of the sea in different zones. According to the laws of hydrostatics, the colder water of the higher degrees of latitude is compelled to flow towards the equator, and the warmer water of the tropics towards the poles, in consequence of which, similar movements are produced in the ocean to those in the atmosphere. This is the cause of the cold under current from the poles to the equator, and of the warm surface-current from the equator to the poles. The waters of the latter, by virtue of the greater velocity of rotation at the equator, assume in their onward progress a direction from west to east. It is a striking proof of the preponderating influence of the tidal wave that, in

spite of this, the motion of the ocean is on the whole in an opposite direction.

Theory and experience thus agree in the result that the influence of the moon on the rotating earth causes a motion of translation from east to west in both atmosphere and ocean. This motion must continually diminish the rotatory effect of the earth, for want of an opposite and compensating influence.

The continual pressure of the tidal wave against the axial rotation of the earth may also be deduced from statical laws.

The gravitation of the moon affects without exception all parts of the globe. Let the earth be divided by the plane of the meridian in which the moon happens to be into two hemispheres, one to the east, the other to the west of this meridian. It is clear that the moon, by its attraction of the eastern hemisphere, tends to retard the motion of the earth, and by its attraction of the western hemisphere, to accelerate the same rotation.

Under certain conditions both these tendencies compensate each other, and then the action of the moon on the earth's rotation becomes zero. This happens when both hemispheres are arranged in a certain manner symmetrically, or when no parts of the earth can change their relative position; in the latter case a sort of symmetry is produced by the rotation.

The form of the earth deviates from a perfectly symmetrical sphere on account of the three following causes:—(1) the flattening of the poles, (2) the mountains on the surface, and (3) the tidal wave. The first two causes do not change the velocity of the earth's axial rotation. In order to comprehend clearly the effect of the tidal wave, we shall imagine the earth to be a perfectly symmetrical sphere uniformly surrounded by water. The attraction of the sun and the moon disturbs the equilibrium of this mass, and two flat mountains of water are formed. The top of one of these is directed towards the moon, and the summit of the other is turned away from it. A straight line passing through the tops of these two mountains is called the major axis of this earth-spheroid.

In this state the earth may be imagined to be divided into three parts—a smaller sphere, and two spherical segments attached to the opposite sides of the latter, and representing the elevations of the tidal wave. The attraction of the moon on the small central sphere does not change the rotation, and we have therefore only to consider the influence of this attraction on the two tidal elevations. The upper elevation or mountain, the one nearest the moon, is attracted towards the west because its mass is principally situated to the east of the moon, and the opposite mountain, which is to the west of the moon, is at-

tracted towards the east. The upper tidal elevation is not only more powerfully attracted because it is nearer to the moon, but also because the angle under which it is pulled aside is more favourable for lateral deflection than in the case of the opposite protuberance. The pressure from east to west of the upper elevation preponderates therefore over the pressure from west to east of the opposite mountain; according to calculation, these quantities stand to each other nearly as 14 to 13. From the relative position of these two tidal protuberances and the moon, or the unchangeable position of the major axis of the earth-spheroid towards the centre of gravity of the moon, a pressure results, which preponderates from east to west, and offers an obstacle to the earth's rotation.

If gravitation were to be compared with magnetic attraction, the earth might be considered to be a large magnet, one pole of which, being more powerfully attracted, would represent the upper, and the other pole the lower tidal elevation. As the upper tidal wave tends to move towards the moon, the earth would act like a galvanometer, whose needle has been deflected from the magnetic meridian, and which, whilst tending to return thereto, exerts a constant lateral pressure.

The foregoing discussion may suffice to demonstrate the influence of the moon on the earth's rotation. The retarding pressure of the tidal wave may quantitatively be determined in the same manner as that employed in computing the precession of the equinoxes and the nutation of the earth's axis. The varied distribution of land and water, the unequal and unknown depth of the ocean, and the as yet imperfectly ascertained mean difference between the time of the moon's culmination and that of high water in the open sea, enter, however, as elements into such a calculation, and render the desired result an uncertain quantity.

In the mean time this retarding pressure, if imagined to act at the equator, cannot be assumed to be less than 1000 millions of kilogrammes. In order to start with a definite conception, we may be allowed to use this round number as a basis for the following calculations.

The rotatory velocity of the earth at the equator is 464 metres, and the consumption of mechanical work, therefore, for the maintenance of the tides 464,000 millions of Km, or 6000 millions of horse-powers per second. The effect of the tides may consequently be estimated at $\frac{1}{30,000}$ th of the effect received by the earth from the sun.

The rotatory effect which the earth at present possesses, may be calculated from its mass, volume, and velocity of rotation. The volume of the earth is 2,650,686,000 cubic miles, and

its specific gravity, according to Reich, =5.44. If, for the sake of simplicity, we assume the density of the earth to be uniform throughout its mass, we obtain from the above premises, and the known velocity of rotation, 25,840 quadrillions of kilogrammetres as the rotatory effect of the earth. If, during every second in 2500 years, 464,000 millions of Km of this effect were consumed by the ebb and flow of the tidal wave, it would suffer a diminution of 36,600 trillions of Km, or about $\frac{1}{700,000}$ th of its quantity.

The velocities of rotation of a sphere stand to each other in the same ratio as the square roots of the rotatory effects, when the volume of the sphere remains constant. From this it follows that, in the assumed time of 2500 years, the length of a day has increased $\frac{1}{1,000,000}$ th; or if a day be taken equal to 86,400 seconds, it has lengthened $\frac{1}{16}$ th of a second, if the volume of the earth has not changed. Whether this supposition be correct or not, depends on the temperature of our planet, and will be discussed in the next chapter.

The tides also react on the motion of the moon. The stronger attraction of the elevation nearest to, and to the east of the moon, increases with the tangential velocity of our satellite; the mean distance of the earth and the moon, and the time of revolution of the latter, are consequently augmented. The effect of this action, however, is insignificant, and, according to calculation, does not amount to more than a fraction of a second in the course of centuries.

[To be continued.]

LIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 238.]

February 18, 1863.—Prof. A. C. Ramsay, President, in the Chair.

THE following communication was read:—

“On the Middle and Upper Lias of the Dorsetshire Coast.”

By E. C. H. Day, Esq.

The subdivisions of the Lias instituted by Sir Henry De la Beche and Dr. Buckland from stratigraphical considerations, and the subsequent modifications and further subdivision established by recent careful comparisons of the fossils, having been briefly noticed, the author proceeded to define the two portions of the Lias which were treated of in this paper. The Middle Lias was stated to comprise all the beds between the zone of *Ammonites communis* above and that of *A. varicostatus* below, and the Upper Lias to include the beds commencing with the zone of *A. communis* and ending with

that of *A. Jurensis*, or all those resting on the zone of *A. spinatus* and superposed by that of *A. Murchisonæ*.—the beds formerly termed 'the Sands of the Inferior Oolite' being referred to the Upper Lias. The sections exposed at Black Venn, Westhay Cliff, Golden Cap, and Down Cliffs were described in succession, the fossils found in each bed being given, as well as the vertical range of the Ammonites.

The occurrence of a new genus of *Belemnitidæ* in the Belemnite-beds of the Middle Lias was noticed, and a description of its general features given, with a list of the associated fossils.

Mr. Day then exhibited, in the form of a generalized section, the different Ammonite-zones into which the Middle and Upper Lias of the Dorsetshire coast could be divided, and gave lists of the fossils peculiar to each.

LV. *Intelligence and Miscellaneous Articles.*

ON A METHOD OF VARYING THE TENSION OF THE DISCHARGE
OF AN ELECTRIC BATTERY, AND OF A RUHKORFF'S COIL.
BY M. A. CAZIN.

IT is known that the elements of a battery may be associated in two ways, according to the nature of the effects which it is desired to produce. I have thought that analogous arrangements might be used with condensers of statical electricity. Hitherto the discharge of Leyden jars has only been used by uniting the armatures of the same name, so as to increase the quantity, and association in series has only been used to charge several batteries at once. I am not aware that any one has observed the properties of the spark obtained in discharging the series by its extreme coatings, although the analogy of a series of condensers with the pile has long been remarked. Thus the illustrious Biot in his *Traité de Physique* describes experiments in which he has measured, by means of a proof plane, the tension on the different plates of an insulated series; and he points out the increase of the tension from the middle to the extremities of the series, although it cannot be concluded from his observations that the length of the spark obtained by these means is much larger than with a single condenser: this is what I have had occasion to observe.

The first experiments were made by M. Ruhmkorff with the aid of his powerful induction-coil. He himself arranged everything with his known ability, and with a disinterestedness which I here desire to thank publicly. The poles of the coil being connected on the one hand with the extreme armatures of a series of insulated Leyden jars, and on the other hand with a discharge as in the arrangement adopted for a single condenser, the length of the spark which passes between the arms of the discharger increases as the number of jars increases, while its magnitude, its lustre, and the report which accompanies it scarcely appear to diminish. Without condenser the spark of the induced discharge was from 300 to 360

millims.; with a single middle-sized Leyden jar it was about 30 millims.; with eight like jars arranged in series the discharge attained 130 millims. We then used jars of almost double size; with a single one the striking distance was about 17 millims., eight gave a spark of 82 millims. The addition of a ninth increased the spark by 8 millims. In this mode of working, the successive condensers discharged themselves immediately after having been charged. To retain the charge, one of the poles of the coil must be joined to the final external armature, and the induction spark taken between the other pole and the first internal armature. The series becomes charged very rapidly, and it can be discharged with the ordinary discharger; the elongation of the striking distance is observed as in the first case.

The same phenomena are reproduced with the ordinary electrical machine; I have repeated the experiment in the laboratory of the Lycée at Versailles, and the general result agrees with the preceding.

M. Ruhmkorff and myself think that this new mode of discharging condensers may be useful in a great number of cases. With a certain number of Leyden jars, associated either in battery or in series, discharges will be obtained, suited by the tension and by the quantity of electricity expended, for the most varied effect. M. Ruhmkorff has already witnessed the application of this method to the beautiful researches of MM. Plücker and Hittorf. The satisfactory result which we obtained by passing the discharge in a capillary tube arranged like theirs, but containing gas at the ordinary pressure, leads us to hope that the arrangement in series might be useful to them.—*Comptes Rendus*, February 16, 1863.

ON SOME REMARKABLE NUMERICAL APPROXIMATIONS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The very close approximation which Mélius obtained by using the fraction $\frac{355}{113}$ as the proportion existing between the *diameter* and the *circumference* of a circle, induced me to seek for other fractions which should approximately represent the decimal values of various other problems.

The result of my investigations I have embodied in a Table, which M. Babinet has done me the honour to lay before the Académie des Sciences at their meeting on the 6th instant. I beg to annex a copy. My intention is to extend the Table as soon as I can find sufficient leisure.

I remain, Gentlemen,

Your most obedient Servant,

April 20, 1863.

CHARLES M. WILlich.

Willich's Table of Approximations.

(Métius)	$\pi = \frac{355}{113}$	-0.000 000 2	=	3.141 592 7
	$2\pi = \frac{710}{113}$	-0.000 000 5	=	6.283 185 3
	$\frac{\pi}{4} = \frac{355}{452}$	-0.000 000 07	=	0.785 398 16
	$\frac{\pi}{6} = \frac{355}{678}$	-0.000 000 07	=	0.523 598 75
	$\frac{1}{3}\pi = \frac{355}{339}$	-0.000 000 09	=	1.047 197 55
	$\frac{2}{3}\pi = \frac{710}{339}$	-0.000 000 1	=	2.094 395 1
	$\frac{4}{3}\pi = \frac{1420}{339}$	-0.000 000 4	=	4.188 790 2
	$\frac{\pi}{360} = \frac{7}{802}$	-0.000 001 6	=	0.008 726 6
	$\frac{1}{\pi} = \frac{113}{355}$	+0.000 000 03	=	0.318 309 88
	$\frac{360}{\pi} = \frac{8136}{71}$	+0.000 010	=	114.591 559
	$\pi^2 = \frac{227}{23}$	+0.000 039	=	9.869 604
	$\pi^3 = \frac{23200}{763}$	-0.000 022	=	30.406 276
	$\frac{1}{\pi^2} = \frac{23}{227}$	-0.000 000 4	=	0.101 321 2
	$\sqrt{\pi} = \frac{296}{167}$	+0.000 001	=	1.772 454
	$\sqrt[3]{\pi} = \frac{331}{226}$	-0.000 010	=	1.464 592
	$\sqrt{\frac{1}{\pi}} = \frac{145}{257}$	-0.000 012	=	0.564 190
	$\sqrt{\frac{4}{\pi}} = \frac{167}{148}$	+0.000 000 8	=	1.128 379 2
	$\sqrt[3]{\frac{\pi}{6}} = \frac{457}{567}$	-0.000 000 5	=	0.805 996 0
	$\sqrt[3]{\frac{6}{\pi}} = \frac{567}{457}$	+0.000 000 8	=	1.240 701 0
Hyper. log of	$\pi = \frac{87}{76}$	-0.000 007	=	1.144 730
Modulus of common log ..	$= \frac{195}{449}$	+0.000 004	=	0.434 294
Reciprocal of same number	$= \frac{449}{195}$	+0.000 021	=	2.302 585
Base of hyper. log.	$= e = \frac{1264}{465}$	+0.000 002	=	2.718 282
Its reciprocal	$= \frac{1}{e} = \frac{465}{1264}$	-0.000 000 002 3	=	0.367 879 744
Side of square equivalent to circle when diameter is unity	$\left. \begin{array}{l} \\ \\ \end{array} \right\} = \frac{148}{167}$	-0.000 000 6	=	0.886 226 9

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JUNE 1863.

LVI. *On an Induction Coil of great power, and on the effects of connecting Plates with the ends of the Secondary Coil.* By the Rev. N. J. CALLAN, D.D., Professor of Natural Philosophy in St. Patrick's College, Maynooth*.

ABOUT three years and a half ago I made an induction coil of considerable power. The secondary coil, which was made of No. 34 iron wire, consisted of three parts, two of which were each about $2\frac{1}{2}$ inches, and the third 3 inches long. The entire length of the secondary coil was about 8 inches, and the length of the secondary wire about 150,000 feet. The mode of insulation is nearly the same as that of the coil described in the Philosophical Magazine for May 1859, except that thin sheet gutta percha is used for insulating the spirals of one layer from those of the adjoining layers. With three cells of the Maynooth battery, in each of which the zinc plate was 4 inches square, the coil gave sparks about 10 inches long between two pointed wires, one connected with each end of the coil. With five cells of the same size, the sparks were only $10\frac{1}{4}$ inches, but were much louder than the sparks produced by three cells. Within the last four months I made a new primary coil nearly 3 feet long with a core about 3 feet 6 inches in length, and improved the insulation between the secondary coil and the primary, and also the insulation of the three parts of the secondary from each other.

Since these changes were made, this coil (the negative end being connected with a plate containing a square foot of surface, and the positive end with a pointed wire) gave sparks $7\frac{1}{2}$ inches long with a single 4-inch cell of the Maynooth battery. With two 4-inch cells the sparks were $12\frac{1}{2}$ inches, and with three cells of the same size they were 15 inches in length.

* Communicated by the Author.

Phil. Mag. S. 4. Vol. 25. No. 170. June 1863.

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When the sparks were taken between two pointed wires, they were only 6 inches with one, and a little more than $13\frac{1}{4}$ inches with three 4-inch cells. Hence, when the negative end is connected with a plate a foot square, the sparks are $1\frac{1}{2}$ inch longer with one cell and $1\frac{3}{4}$ inch longer with three cells than when each end of the coil is connected with a pointed wire.

When the negative end was connected with a circular brass plate 4 inches in diameter, and the positive with a pointed wire, the sparks with one cell were $6\frac{1}{4}$ inches, and with three cells they were $13\frac{1}{2}$ inches. Therefore when the negative end is connected with a plate a foot square, the sparks are $1\frac{1}{4}$ inch longer than when it is connected with a 4-inch circular plate.

Since I discovered the great increase produced in the length of the spark by connecting a large plate of any metal with the negative end of the coil, I made a series of experiments in order to ascertain the best size of the plate, and the effects of connecting plates in various ways with the ends of the coil. Some of the results of these experiments appear to be anomalous, and not in accordance with the commonly received theory regarding the distribution of electricity over the surface of bodies, nor with the rule for determining the height to which a lightning conductor should project above the roof of a building to which it is attached.

First, with regard to the size of the plate necessary to produce the longest spark, I have found that a circular metallic plate 7 inches in diameter is sufficiently large when the spark does not exceed 10 inches, and that a piece of wood 9 inches square produces the same effect as the 7-inch plate. I commonly use a circular plate of tin about $12\frac{1}{2}$ inches in diameter. With this plate the coil gives a spark an eighth of an inch longer than with a plate containing four square feet of surface. In order to get the longest spark from a coil, the outer end of the secondary coil should be positive, and the inner end negative; the plate should be connected with the negative end, and the pointed wire with the positive end, and the central part of the plate should face the point of the wire. When the plate faces the point, and its circumference is opposite the point, the sparks are a little shorter than when the point is opposite the middle of the plate.

Secondly, with regard to the effects of plates connected with the ends of the coil, I have found that the effects of a plate in connexion with the negative end differ very much and in various ways from those that are produced by a plate connected with the positive end.

In most of my experiments on this subject the greatest length of spark the coil was capable of giving with the three cells em-

ployed varied from $13\frac{1}{2}$ to 15 inches. When I used a weaker battery I mention the length of the spark. The plate was about $12\frac{1}{2}$ inches, and the point was opposite to the middle of the plate, except when the contrary is expressed.

First, when a pointed wire is connected with the positive end of the coil, a plate connected with the negative end lengthens the spark considerably; but when the pointed wire is connected with the negative end, a plate connected with the positive one shortens the spark in a greater proportion. Sparks which were 15 inches long in the first arrangement were reduced to less than 11 inches by the second; they did not pass at all between the positive plate and negative point until the plate was brought within $8\frac{1}{2}$ inches from the point.

Secondly, sparks from a positive point to a negative plate never went to the circumference of the plate, and scarcely ever struck the plate at a greater distance from the centre than 3 inches. But sparks between a negative point and positive plate always went to the circumference until the plate was brought within $2\frac{1}{2}$ or 3 inches from the point: even when I used a rectangular plate 20 inches broad and 28 inches long, the sparks went to the edge of the plate.

Thirdly, the sparks from a positive point to a negative plate never moved in a straight line, even when the point was less than an inch from the plate. But when a negative point is brought within $2\frac{1}{2}$ or 3 inches, or even less than an inch from a positive plate, the sparks pass in a straight line between the point and the nearest part of the plate.

Fourthly, the sparks from a positive point to a negative plate grow weaker and less loud as the point is made to approach the plate. But when a negative point is brought within two or three inches from a positive plate, the sparks become as loud as if the plate were charged, or as if they were produced by a small Leyden jar whose opposite coatings were connected with the ends of the coil. With a plate containing four square feet of surface, the sparks were louder than with the 12-inch plate. A hollow ball connected with the positive end of the coil also gives very loud sparks. Hence a plate or ball connected with the positive end becomes charged, but a plate connected with the negative end does not.

Fifthly, when a pointed wire nearly $\frac{1}{4}$ of an inch thick projected $6\frac{3}{8}$ inches from the middle of the $12\frac{1}{2}$ -inch circular plate connected with the negative end of the coil, and nearly at right angles to the plate, and another plate of the same size was connected with the positive end, the sparks passed not between the point and positive plate, but from the circumference of one to the circumference of the other, although the distance of the point

from the plate was less than 4 inches, and the distance between the circumference of one plate and that of the other was 10 inches. But when a pointed wire projected more than three-fourths of an inch from the middle of a plate connected with the positive end of the coil, and the other plate was connected with the negative end, the sparks passed from the point to the negative plate, and never from the circumference of one to that of the other.

Sixthly, when a pointed wire projected from the negative plate at about half an inch from its circumference, and $3\frac{1}{2}$ inches from the plate, the sparks passed even then far less frequently between the point and the opposite circumference of the positive plate than between the circumference of one and that of the other plate. But no such effect will take place when the pointed wire projects from the positive plate.

When a pointed wire projected $3\frac{1}{2}$ inches from the middle of a wet slate about 9 inches square, or $2\frac{1}{2}$ inches from the middle of a wet piece of wood of the same size, connected with the negative end of the coil, and the $12\frac{1}{2}$ -inch plate was connected with the positive end, the sparks passed between the circumference of the plate and the edges of the slate or wood, rather than between the point and plate. When the slate or wood is dry, the sparks passed from the pointed wire to the positive plate. When the wood or slate is merely damp but not very wet, the sparks from the wire frequently run to the edge of the slate or wood and then pass to the plate. If the wire project from the wood or slate near the edges, the sparks will pass between the point and positive plate, unless when the pointed wire projects to a small distance from the wood or slate.

A ball 3 inches in diameter connected with the positive end shortens the spark as much as a 12-inch plate.

When a pointed wire is opposite the edge of a plate connected with the negative end, the spark is longer than between two points, but shorter than between a positive point and the middle of a negative plate.

When two plates are connected with the ends of the coil and face each other, the spark is reduced from 15 inches to about 11 inches. When their edges are opposite to each other, the spark is also shortened.

I have repeated most of the above-mentioned experiments with a weak battery which gave sparks 7 or 8 inches long, and obtained the same results. When the sparks are about 8 inches, the plate connected with the end plate should not be more than 7 inches in diameter. When I connected the $12\frac{1}{2}$ -inch plate, or a 3-inch hollow ball, with the positive end (the longest spark being about 8 inches), no spark passed between the plate or ball and a negative point until the point was brought within about 2

inches from the plate, or until the sparks resembled the discharges of a small Leyden jar. The sparks never passed between the point and circumference of the plate. It was otherwise when I used a 7-inch plate. Hence the size of the plate connected with the positive end of the coil must depend on the power of the coil employed. The pointed wire used in all the experiments was nearly $\frac{1}{4}$ of an inch thick.

I have not as yet had time to give a fair trial to the coil with a battery of greater power than that of three 4-inch cells. With one cell in which the zinc plate was 4 inches by 8, I got sparks $8\frac{1}{4}$ inches long when the plate connected with the negative end was only 4 inches in diameter. Had I known at the time that with a 12-inch plate the spark is $1\frac{1}{4}$ inch longer than with a 4-inch one, the sparks with the single cell 4×8 would have been $9\frac{1}{2}$ inches long. I intend to try the coil as soon as convenient with a battery of twelve 6-inch cells, and six cells 4 inches by 8.

St. Patrick's College, Maynooth,
May 13, 1863.

LVII. *On Celestial Dynamics.* By Dr. J. R. MAYER.†

[Concluded from p. 409.]

IX. *The Heat of the Interior of the Earth.*

WITHOUT doubt there was once a time when our globe had not assumed its present magnitude. According to this, by aid of this simple assumption, the origin of our planet may be reduced to the union of once separated masses.

To the mechanical combinations of masses of the second order, with masses of the second and third order, &c., the same laws as those enunciated for the sun apply. The collision of such masses must always generate an amount of heat proportional to the squares of their velocities, or to their mechanical effect.

Although we are not in a position to affirm anything certain respecting the primordial conditions under which the constituent parts of the earth existed, it is nevertheless of the greatest interest to estimate the quantities of heat generated by the collision and combination of these parts by a standard based on the simplest assumptions.

Accordingly we shall consider for the present the earth to have been formed by the union of two parts, which obtained their relative motions by their mutual attraction only. Let the whole mass of the present earth, expressed in kilogrammes, be T , and the masses of the two portions $T-x$ and x . The ratio of these two quantities may be imagined to assume various

values. The two extreme cases are, when x is considered infinitely small in comparison with T , and when $x = T - x = \frac{1}{2} T$. These form the limits of all imaginable ratios of the parts $T - x$ and x , and will now be more closely examined.

Terrestrial heights are of course excluded from the following consideration. In the first place let x , in comparison with $T - x$, be infinitely small. The final velocity with which x arrives on the surface of the large mass, after having passed through a great space in a straight line, or after previous central motion round it, is, according to the laws developed in relation to the sun in Chapter IV., confined within the limits of 7908 and 11,183 metres. The heat generated by this process may amount to from $8685 \times x$ to $17,370 \times x$ units, according to the value of the major axis of the orbit of x . This heat, however, vanishes by its distribution through the greater mass, because x is, according to supposition, infinitely small in comparison with T .

The quantity of heat generated increases with x , and amounts in the second case, when $x = \frac{1}{2} T$, to from $6000 \times T$ to $8685 \times T$ units.

If we assume the earth to possess a very great capacity for heat, equal in fact to that of its volume of water, which when calculated for equal weights = 0.184, the above discussion leads to the conclusion that the difference of temperature of the constituent parts, and of the earth after their union, or, in other words, the heat generated by the collision of these parts, may range, according to their relative magnitude, from 0° to $32,000^\circ$, or even to $47,000^\circ$!

With the number of parts which thus mechanically combine, the quantity of heat developed increases. Far greater still would have been the generation of heat if the constituent parts had moved in separate orbits round the sun before their union, and had accidentally approached and met each other. For various reasons, however, this latter supposition is not very probable.

Several facts indicate that our earth was once a fiery liquid mass, which has since cooled gradually, down to a comparatively inconsiderable depth from the surface, to its present temperature. The first proof of this is the form of the earth. "The form of the earth is its history." According to the most careful measurements, the flattening at the poles is exactly such as a liquid mass rotating on its axis with the velocity of the earth would possess; from this we may conclude that the earth at the time it received its rotatory motion was in a liquid state; and, after much controversy, it may be considered as settled that this liquid condition was not that of an aqueous solution, but of a mass melted by a high temperature.

The temperature of the crust of the globe likewise furnishes proof of the existence of a store of heat in its interior. Many exact experiments and measurements show that the temperature of the earth increases with the depth to which we penetrate. In boring the artesian well at Grenelle, which is 546 metres deep, it was observed that the temperature augmented at the rate of 1° for every 30 metres. The same result was obtained by observations in the artesian well at Mondorf in Luxembourg: this well is 671 metres in depth, and its water 34° warm.

Thermal springs furnish a striking proof of the high temperature existing in the interior of the earth. Scientific men are agreed that the aqueous deposits from the atmosphere, rain, hail, dew, and snow, are the sole causes of the formation of springs. The water obeying the laws of gravity, percolates through the earth wherever it can, and reappears at the surface in places of a lower situation. When water sinks to considerable depths through vertical crevices in the rocks, it acquires the temperature of the surrounding strata, and returns as a thermal spring to the surface.

Such waters are frequently distinguished from the water of ordinary springs merely by their possessing a higher temperature. If, however, the water in its course meets with mineral or organic substances which it can dissolve and retain, it then reappears as a mineral spring. Examples of such are met with at Aachen, Carlsbad, &c.

In a far more decided manner than by the high temperature of the water of certain springs, the interior heat of our globe is made manifest by those fiery fluid masses which sometimes rise from considerable depths. The temperature of the earth's crust increases at the rate of 1° for every 30 metres we descend from the surface towards the centre. Although it is incredible that this augmentation can continue at the same rate till the centre be reached, we may nevertheless assume with certainty that it does continue to a considerable depth. Calculation based on this assumption shows that at a depth of a few miles a temperature must exist sufficiently powerful to fuse most substances. Such molten masses penetrate the cold crust of the globe in many places, and make their appearance as lava.

A distinguished scientific man has lately expressed himself on the origin of the interior heat of the earth as follows:—"No one of course can explain the final causes of things. This much, however, is clear to every thinking man, that there is just as much reason that a body, like the earth for example, should be warm, warmer than ice or human blood, as there is that it should be cold or colder than the latter. A particular cause for this absolute heat is as little necessary as a cause for

motion or rest. Change—that is to say, transition from one state of things to another—alone requires and admits of explanation.”

It is evident that this reflection is not fitted to suppress the desire for an explanation of the phenomenon in question. As all matter has the tendency to assume the same temperature as that possessed by the substances by which it happens to be surrounded, and to remain in a quiescent state as soon as equilibrium has been established, we must conclude that, whenever we meet with a body warmer than its neighbours, such body must have received at a (relatively speaking) not far distant time, a certain degree of heat,—a process which certainly allows of, and requires explanation.

Newton's theory of gravitation, whilst it enables us to determine, from its present form, the earth's state of aggregation in ages past, at the same time points out to us a source of heat powerful enough to produce such a state of aggregation, powerful enough to melt worlds; it teaches us to consider the molten state of a planet as the result of the mechanical union of cosmical masses, and thus to derive the radiation of the sun and the heat in the bowels of the earth from a common origin.

The rotatory effect of the earth also may be readily explained by the collision of its constituent parts; and we must accordingly subtract the *vis viva* of the axial rotation from the whole effect of the collision and mechanical combination, in order to obtain the quantity of heat generated. The rotatory effect, however, is only a small quantity in comparison with the interior heat of the earth. It amounts to about $4400 \times T$ kilogrammetres, T being the weight of the earth in kilogrammes, which is equivalent to $12 \times T$ units of heat, if we assume the density of the earth to be uniform throughout.

If we imagine the moon in the course of time, either in consequence of the action of a resisting medium or from some other cause, to unite herself with our earth, two principal effects are to be discerned. A result of the collision would be, that the whole mass of the moon and the cold crust of the earth would be raised some thousands of degrees in temperature, and consequently the surface of the earth would be converted into a fiery ocean. At the same time the velocity of the earth's axial rotation would be somewhat accelerated, and the position of its axis with regard to the heavens, and to its own surface, slightly altered. If the earth had been a cold body without axial rotation, the process of its combining with the moon would have imparted to it both heat and rotation.

It is probable that such processes of combination between different parts of our globe may have repeatedly happened

before the earth attained its present magnitude, and that luxuriant vegetation may have at different times been buried under the fiery débris resulting from the conflict of these masses.

As long as the surface of our globe was in an incandescent state, it must have lost heat at a very rapid rate; gradually this process became slower; and although it has not yet entirely ceased, the rate of cooling must have diminished to a comparatively small magnitude.

Two phenomena are caused by the cooling of the earth, which, on account of their common origin, are intimately related. The decrease of temperature, and consequent contraction of the earth's crust, must have caused frequent disturbances and revolutions on its surface, accompanied by the ejection of molten masses and the formation of protuberances; on the other hand, according to the laws of mechanics, the velocity of rotation must have increased with the diminution of the volume of the sphere, or, in other words, the cooling of the earth must have shortened the length of the day.

As the intensity of such disturbances and the velocity of rotation are closely connected, it is clear that the youth of our planet must have been distinguished by continual violent transformations of its crust, and a perceptible acceleration of the velocity of its axial rotation; whilst in the present time the metamorphoses of its surface are much slower, and the acceleration of its axial revolution diminished to a very small amount.

If we imagine the times when the Alps, the chain of the Andes, and the Peak of Teneriffe were upheaved from the deep, and compare with such changes the earthquakes and volcanic eruptions of historic times, we perceive in these modern transformations but weak images of the analogous processes of bygone ages.

Whilst we are surrounded on every side by the monuments of violent volcanic convulsions, we possess no record of the velocity of the axial rotation of our planet in antediluvian times. It is of the greatest importance that we should have an exact knowledge of a change in this velocity, or in the length of the day during historic times. The investigation of this subject by the great Laplace forms a bright monument in the department of exact science.

These calculations are essentially conducted in the following manner:—In the first place, the time between two eclipses of the sun, widely apart from each other, is as accurately as possible expressed in days, and from this the ratio of the time of the earth's rotation to the mean time of the moon's revolution determined. If, now, the observations of ancient

astronomers be compared with those of our present time, the least alteration in the absolute length of a day may be detected by a change in this ratio, or in a disturbance in the lunar revolution. The most perfect agreement of ancient records on the movements of the moon and the planets, on the eclipses of the sun, &c., revealed to Laplace the remarkable fact that in the course of 25 centuries, the time in which our earth revolves on its axis has not altered $\frac{1}{300}$ th part of a sexagesimal second; and the length of a day therefore may be considered to have been constant during historic times.

This result, as important as it was convenient for astronomy, was nevertheless of a nature to create some difficulties for the physicist. With apparently good reason it was concluded that, if the velocity of rotation had remained constant, the volume of the earth could have undergone no change. The earth completes one revolution on its axis in 86,400 sidereal seconds; it consequently appears, if this time has not altered during 2500 years to the extent of $\frac{1}{300}$ th of a second, or $\frac{1}{43,000,000}$ th part of a day, that during this long space of time the radius of the earth also cannot have altered more than this fraction of its length. The earth's radius measures 6,369,800 metres, and therefore its length ought not to have diminished more than 15 centimetres in 25 centuries.

The diminution in volume, as a result of the cooling-process, is, however, closely connected with the changes on the earth's surface. When we consider that scarcely a day passes without the occurrence of an earthquake or shock in one place or another, and that of the 300 active volcanos some are always in action, it would appear that such a lively reaction of the interior of the earth against the crust is incompatible with the constancy of its volume.

This apparent discrepancy between Cordier's theory of the connexion between the cooling of the earth and the reaction of the interior on the exterior parts, and Laplace's calculation showing the constancy of the length of the day, a calculation which is undoubtedly correct, has induced most scientific men to abandon Cordier's theory, and thus to deprive themselves of any tenable explanation of volcanic activity.

The continued cooling of the earth cannot be denied, for it takes place according to the laws of nature; in this respect the earth cannot comport itself differently from any other mass, however small it be. In spite of the heat which it receives from the sun, the earth will have a tendency to cool so long as the temperature of its interior is higher than the mean temperature of its surface. Between the tropics the mean temperature produced by the sun is about 28° , and the sun therefore

is as little able to stop the cooling-tendency of the earth as the moderate warmth of the air can prevent the cooling of a red-hot ball suspended in a room.

Many phenomena, for instance the melting of the glaciers near the bed on which they rest, show the uninterrupted emission of heat from the interior towards the exterior of the earth; and the question is, Has the earth in 25 centuries actually lost no more heat than that which is requisite to shorten a radius of more than 6 millions of metres only 15 centimetres?

In answering this question, three points enter into our calculation:—(1) the absolute amount of heat lost by the earth in a certain time, say one day; (2) the earth's capacity for heat; and (3) the coefficient of expansion of the mass of the earth.

As none of these quantities can be determined by direct measurements, we are obliged to content ourselves with probable estimates; these estimates will carry the more weight the less they are formed in favour of some preconceived opinion.

Considering what is known about the expansion and contraction of solids and liquids by heat and cold, we arrive at the conclusion that for a diminution of 1° in temperature, the linear contraction of the earth cannot well be less than $\frac{1}{100,000}$ th part, a number which we all the more readily adopt because it has been used by Laplace, Arago, and others.

If we compare the capacity for heat of all solid and liquid bodies which have been examined, we find that, both as regards volume and weight, the capacity of water is the greatest. Even the gases come under this rule; hydrogen, however, forms an exception, it having the greatest capacity for heat of all bodies when compared with an equal weight of water. In order not to take the capacity for heat of the mass of the earth too small, we shall consider it to be equal to that of its volume of water, which, when calculated for equal weights, amounts to 0.184*.

* The capacity for heat, as well as the coefficient of expansion of matter, as a rule, increases at higher temperatures. As, however, these two quantities act in opposite ways in our calculations, we may be allowed to dispense with the influence which the high temperature of the interior of the earth must exercise on these numbers. Even if, in consequence of the high temperature of the interior, the earth's mass could have a capacity two or three times as great as that which it has from 0° to 100° , it is to be considered, on the other hand, that the coefficient of expansion, $\frac{1}{100,000}$, only holds good for solids, and is even small for them, whilst in the case of liquids we have to assume a much greater coefficient: for mercury between 0° and 100° , it is about six times as great. Especially great is the contraction and expansion of bodies when they change their state of aggregation; and this should be taken into account when considering the formation of the earth's crust.

If we accept Laplace's result, that the length of a day has remained constant during the last 2500 years, and conclude that the earth's radius has not diminished $1\frac{1}{2}$ decimetre in consequence of cooling, we are obliged to assume, according to the premises stated, that the mean temperature of our planet cannot have decreased $\frac{1}{430}^{\circ}$ in the same period of time.

The volume of the earth amounts to 2650 millions of cubic miles. A loss of heat sufficient to cool this mass $\frac{1}{430}^{\circ}$ would be equal to the heat given off when the temperature of 6,150,000 cubic miles of water decreases 1° ; hence the loss for one day would be equal to 6.74 cubic miles of heat.

Fourier has investigated the loss of heat sustained by the earth. Taking the observation that the temperature of the earth increases at the rate of 1° for every 30 metres as the basis of his calculations, this celebrated mathematician finds the heat which the globe loses by conduction through its crust in the space of 100 years to be capable of melting a layer of ice 3 metres in thickness and covering the whole surface of the globe; this corresponds in one day to 7.7 cubic miles of heat, and in 2500 years to a decrease of 17 centimetres in the length of the radius.

According to this, the cooling of the globe would be sufficiently great to require attention when the earth's velocity of rotation is considered.

At the same time it is clear that the method employed by Fourier can only bring to our knowledge one part of the heat which is annually lost by the earth; for simple conduction through *terra firma* is not the only way by which heat escapes from our globe.

In the first place, we may make mention of the aqueous deposits of our atmosphere, which, as far as they penetrate our earth, wash away, so to speak, a portion of the heat, and thus accelerate the cooling of the globe. The whole quantity of water which falls from the atmosphere upon the land in one day, however, cannot be assumed to be much more than half a cubic mile in volume, hence the cooling effect produced by this water may be neglected in our calculation. The heat carried off by all the thermal springs in the world is very small in comparison with the quantities which we have to consider here.

Much more important is the effect produced by active volcanos. As the heat which accompanies the molten matter to the surface is derived from the store in the interior of the earth, their action must influence considerably the diminution of the earth's heat. And we have not only to consider here actual eruptions which take place in succession or simultaneously at different parts of the earth's surface, but also volcanos in a

quiescent state, which continually radiate large quantities of heat abstracted from the interior of the globe. If we compare the earth to an animal body, we may regard each volcano as a place where the epidermis has been torn off, leaving the interior exposed, and thus opening a door for the escape of heat.

Of the whole of the heat which passes away through these numerous outlets, too low an estimate must not be made. To have some basis for the estimation of this loss, we have to recollect that in 1783 Skaptar-Jokul, a volcano in Iceland, emitted sufficient lava in the space of six weeks to cover 60 square miles of country to an average depth of 200 metres, or, in other words, about $1\frac{1}{2}$ cubic mile of lava. The amount of heat lost by this one eruption of one volcano must, when the high temperature of the lava is considered, be estimated to be more than 1000 cubic miles of heat; and the whole loss resulting from the action of all the volcanos amounts, therefore, in all probability, to thousands of cubic miles of heat per annum. This latter number, when added to Fourier's result, produces a sum which evidently does not agree with the assumption that the volume of our earth has remained unchanged.

In the investigation of the cooling of our globe, the influence of the water of the ocean has to be taken into account. Fourier's calculations are based on the observations of the increase of the temperature of the crust of our earth, from the surface towards the centre. But two-thirds of the surface of our globe are covered with water, and we cannot assume *à priori* that this large area loses heat at the same rate as the solid parts; on the contrary, various circumstances indicate that the cooling of our globe proceeds more quickly through the waters of the ocean resting on it than from the solid parts merely in contact with the atmosphere.

In the first place, we have to remark that the bottom of the ocean is, generally speaking, nearer to the store of heat in the interior of the earth than the dry land is, and hence that the temperature increases most probably in a greater ratio from the bottom of the sea towards the interior of the globe, than it does in our observations on the land. Secondly, we have to consider that the whole bottom of the sea is covered by a layer of ice-cold water, which moves constantly from the poles to the equator, and which, in its passage over sand-banks, causes, as Humboldt aptly remarks, the low temperatures which are generally observed in shallow places. That the water near the bottom of the sea, on account of its great specific heat and its low temperature, is better fitted than the atmosphere to withdraw the heat from the earth, is a point which requires no further discussion.

We have plenty of observations which prove that the earth suffers a great loss of heat through the waters of the ocean. Many investigations have demonstrated the existence of a large expanse of sea, much visited by whalers, situated between Iceland, Greenland, Norway, and Spitzbergen, and extending from lat. 76° to 80° N., and from long. 15° E. to 15° W. of Greenwich, where the temperature was observed to be higher in the deeper water than near the surface—an experience which neither accords with the general rule, nor agrees with the laws of hydrostatics. Franklin observed, in lat. 77° N. and long. 12° E., that the temperature of the sea near the surface was $-\frac{1}{2}^{\circ}$, and at a depth of 700 fathoms $+6^{\circ}$. Fisher, in lat. 80° N. and long. 11° E., noticed that the surface-water had a temperature of 0° , whilst at a depth of 140 fathoms it stood at $+8^{\circ}$.

As sea-water, unlike pure water, does not possess a point of greatest density at some distance above the freezing-point, and as the water in lat. 80° N. is found at some depth to be warmer than water at the same depth 10° southward, we can only explain this remarkable phenomenon of an increase of temperature with an increase of depth by the existence of a source of heat at the bottom of the sea. The heat, however, which is required to warm the water at the bottom of an expanse of ocean more than 1000 square miles in extent to a sensible degree, must amount, according to the lowest estimate, to some cubic miles of heat a day.

The same phenomenon has been observed in other parts of the world, such as the west coast of Australia, the Adriatic, the Lago Maggiore, &c. Especial mention should here be made of an observation by Horner, according to whom the lead, when hauled up from a depth varying from 80 to 100 fathoms in the mighty Gulf-stream off the coast of America, used to be hotter than boiling water.

The facts above mentioned, and some others which might be added, clearly show that the loss of heat suffered by our globe during the last 2500 years is far too great to have been without sensible effect on the velocity of the earth's rotation. The reason why, in spite of this accelerating cause, the length of a day has nevertheless remained constant since the most ancient times, must be attributed to an opposite retarding action. This consists in the attraction of the sun and moon on the liquid parts of the earth's surface, as explained in the last chapter.

According to the calculations of the last chapter, the retarding pressure of the tides against the earth's rotation would cause, during the lapse of 2500 years, a sidereal day to be lengthened to the extent of $\frac{1}{16}$ th of a second; as the length of

a day, however, has remained constant, the cooling effect of the earth during the same period of time must have shortened the day $\frac{1}{16}$ th of a second. A diminution of the earth's radius to the amount of $4\frac{1}{2}$ metres in 2500 years, and a daily loss of 200 cubic miles of heat, correspond to this effect. Hence, in the course of the last 25 centuries, the temperature of the whole mass of the earth must have decreased $\frac{1}{14}^{\circ}$.

The not inconsiderable contraction of the earth resulting from such a loss of heat, agrees with the continual transformations of the earth's surface by earthquakes and volcanic eruptions; and we agree with Cordier, the industrious observer of volcanic processes, in considering these phenomena a necessary consequence of the continual cooling of an earth which is still in a molten state in its interior.

When our earth was in its youth, its velocity of rotation must have increased to a very sensible degree, on account of the rapid cooling of its then very hot mass. This accelerating cause gradually diminished, and as the retarding pressure of the tidal wave remains nearly constant, the latter must finally preponderate, and the velocity of rotation therefore continually decrease. Between these two states we have a period of equilibrium, a period when the influence of the cooling and that of the tidal pressure counterbalance each other; the whole life of the earth therefore may be divided into three periods—youth with increasing, middle age with uniform, and old age with decreasing velocity of rotation.

The time during which the two opposed influences on the rotation of the earth are in equilibrium can, strictly speaking, only be very short, inasmuch as in one moment the cooling, and in the next moment the pressure of the tides must prevail. In a physical sense, however, when measured by human standards, the influence of the cooling, and still more so that of the tidal wave, may for ages be considered constant, and there must consequently exist a period of many thousand years' duration during which these counteracting influences will appear to be equal. Within this period a sidereal day attains its shortest length, and the velocity of the earth's rotation its maximum—circumstances which, according to mathematical analysis, would tend to lengthen the duration of this period of the earth's existence.

The historical times of mankind are, according to Laplace's calculation, to be placed in this period. Whether we are at the present moment still near its commencement, its middle, or are approaching its conclusion, is a question which cannot be solved by our present data, and must be left to future generations.

The continual cooling of the earth cannot be without an influence on the temperature of its surface, and consequently on the climate; scientific men, led by Buffon, in fact, have advanced the supposition that the loss of heat sustained by our globe must at some time render it an unfit habitation for organic life. Such an apprehension has evidently no foundation, for the warmth of the earth's surface is even now much more dependent on the rays of the sun than on the heat which reaches us from the interior. According to Pouillet's measurements, mentioned in Chapter III., the earth receives 8000 cubic miles of heat a day from the sun, whereas the heat which reaches the surface from the earth's interior may be estimated at 200 cubic miles per diem. The heat therefore obtained from the latter source every day is but small in comparison to the diurnal heat received from the sun.

If we imagine the solar radiation to be constant, and the heat we receive from the store in the interior of the earth to be cut off, we should have as a consequence various changes in the physical constitution of the surface of our globe. The temperature of hot springs would gradually sink down to the mean temperature of the earth's crust, volcanic eruptions would cease, earthquakes would no longer be felt, and the temperature of the water of the ocean would be sensibly altered in many places—circumstances which would doubtless affect the climate in many parts of the world. Especially it may be presumed that Western Europe, with its present favourable climate, would become colder, and thus *perhaps* the seat of the power and culture of our race transferred to the milder parts of North America.

Be this as it may, for thousands of years to come we can predict no diminution of the temperature of the surface of our globe as a consequence of the cooling of its interior mass; and, as far as historic records teach, the climates, the temperatures of thermal springs, and the intensity and frequency of volcanic eruptions are now the same as they were in the far past.

It was different in prehistoric times, when for centuries the earth's surface was heated by internal fire, when mammoths lived in the now uninhabitable polar regions, and when the tree-ferns and the tropical shell-fish whose fossil remains are now especially preserved in the coal-formation were at home in all parts of the world.

LVIII. *Note on Professor Tyndall's "Remarks on the Dynamical Theory of Heat."*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Glasgow College,
May 4, 1863.

THE Philosophical Magazine just published contains a letter addressed to me, and bearing Dr. Tyndall's signature. As I never received the original, I am surprised to see this use made of my name.

Allow me to say that I consider it a great injury to myself that I should be made even apparently the medium of the statements which Dr. Tyndall addresses to me regarding my friend Professor Tait, whose distinguished position in science, as one of the first mathematicians of our time, as an author, and as an experimenter, should have preserved him from such liberties.

The tone adopted by Dr. Tyndall in addressing myself is of a character, I believe, unprecedented in scientific discussion. It is such that I decline to take part personally in any controversy with him, or to notice his letter further than to say that on all the scientific questions touched upon in it I am ready to support the correctness of the opinions, and information, in the article in 'Good Words' by Professor Tait and myself, and that I shall do so when I see proper occasion.

I remain, Gentlemen,

Your obedient Servant,

WILLIAM THOMSON.

LIX. *On the Conservation of Energy.* By P. G. TAIT, M.A.,
Professor of Natural Philosophy in the University of Edinburgh.

MY DEAR SIR DAVID BREWSTER,

I FEAR I must trouble you with a second letter on the history of the "Conservation of Energy;" but it shall be as brief as possible. And, as Professor Tyndall has demanded, though not from me, proofs of some assertions made in my former letter, I shall take this opportunity of giving them.

The conservation of energy as regards ordinary mechanics was completely stated by Newton, in its differential form, as a corollary to his third law of motion, in the remarkable words, "*Si æstimetur agentis actio ex ejus vi et velocitate conjunctim, et similiter resistentis reactio æstimetur conjunctim ex ejus partium singularum velocitatibus et viribus resistendi ab earum attritione, co-hæsione, pondere, et acceleratione oriundis; erunt actio et reactio sibi invicem semper æquales.*" In an integral form it

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occurs, completely proved for a single moving particle, in Sect. VIII. Prop. XL. of the *Principia*.

So that when Davy experimentally showed that "the immediate cause of the phenomenon of heat is motion, and the laws of its communication are precisely the same as the laws of the communication of motion," the dynamical theory of heat was so far completed that it required only a quantitative determination of the mechanical equivalent. The immense additions it received from Carnot, and subsequently from Clausius, Rankine, Thomson, &c., are really only the *development* of the theory; and much of them must rest on hypotheses until we know the intimate structure of matter, and the kind of motion to which heat belongs.

But the general principle of conservation of energy is founded on the *experimental* determination of relations of equivalence between the various forms of energy, and especially between heat on the one hand, and each of the others in succession. This was begun by Joule in 1840, and had already produced a vast amount of important results, *before* the appearance of Mayer's first paper.

That paper, about which so much has lately been written, contains in its fundamental statements an *essentially false* analogy between the falling together of material masses by gravitation and the condensation of a gas by the external application of mechanical force. The idea that heat developed by compression is an equivalent for the work spent (radically fallacious as it is) is so important a part of Mayer's theoretical speculations, that he has repeated it in his 'Celestial Dynamics' (1848), and his 'Remarks on the Mechanical Equivalent of Heat' (1851).

So far from being a step in advance, the method suggested by Mayer for the deduction of the mechanical equivalent of heat was a retrograde step, and tended only to introduce confusion.

Joule saw that experiment, not hypothesis, was required to arrive at knowledge regarding the heat evolved by compression. He thus arrived experimentally in 1844 or 1845 at the knowledge that when air is compressed the heat evolved is very nearly the true equivalent of the work spent. In later investigations he verified Thomson's deductions from Carnot's theory as to the heat evolved by the compression of liquids (mercury and water), and application of pressure or tension to solids. Thus while, as a general principle, Mayer's hypothesis is essentially false and misleading, and is signally at variance with the truth when applied to liquids, it is, as Joule's experiments have shown, approximately verified in air and gases. Having obtained this result, Joule, finding no such agreement between Mayer's equivalent and his own as might have been expected, was led to investigate the specific heat of air; and this important datum, without which Mayer's method (even supposing it had been founded on correct

principles) could never have given even an approximation, was furnished by Joule in 1851 and 1852, by means of his own value of the mechanical equivalent as determined from the friction of liquids, and by actual experiments on the specific heat of air made for the purpose of verifying this deduction.

In the application of energy to organic processes, Joule preceded Mayer (at all events as far as publication is concerned) by two years; and in its application to shooting-stars and some other points of celestial dynamics he had at least one year's priority.

Mayer's later papers are extremely remarkable and excessively interesting, and certainly deserve high credit; but, as we have just seen, they are, though greatly superior in development to the earliest cosmical speculations of Joule, certainly subsequent to them in publication.

My remark about Séguin was already virtually made by Joule in his published letter of August last. He there gives Séguin's equivalent as 363 kilogrammetres, and Mayer's as 365; and quotes from Séguin the following sentence which embodies the assumption of Mayer (but without his false analogy):—"La force mécanique qui apparaît pendant l'abaissement de température d'un gaz comme de tout autre corps qui se dilate, est la mesure et la représentation de cette diminution de chaleur." I am not aware that Prof. Tyndall has pointed out any inaccuracies in that letter.

That Joule gave in 1843 the value 772 foot-pounds (or 770, which, as Prof. Thomson and I remarked in our article in 'Good Words,' is within about $\frac{1}{300}$ th of difference) is denied by Prof. Tyndall. Prof. Tyndall strangely enough quotes *all* Joule's published values for that year *with this remarkable exception*; and this is the more strange that in a foot-note to his last paper he quotes Joule's very next sentence. In 'Good Words' we distinctly said "the actual method which he [Joule] first employed was to force water through small tubes." The determinations which Prof. Tyndall has quoted were made by means of the magneto-electric machine, and, considering the excessive experimental difficulties which this process involves, it is wonderful that they were not much more widely discordant.

With many apologies for thus trespassing on your valuable space, believe me,

My dear Sir David Brewster,

Yours very truly,

P. GUTHRIE TAIT.

6 Greenhill Gardens, Edinburgh,
May 8, 1863.

LX. *Mineralogical Notes.* By PROFESSOR MASKELYNE and
Dr. VIKTOR VON LANG, of the British Museum.

[Continued from p. 58.]

[With Four Plates].

7. *On the Crystalline Form and the Optical Properties of Malachite.*
By Viktor von Lang.

MALACHITE crystallizes in the oblique system ; but hitherto the whole of its completely crystallographic elements were not capable of being determined, as on the crystals of this mineral there had only been found the planes 110, 100, 010, 101, which do not yield a sufficient number of independent angular elements for the purpose. But there are in the mineral collection of the British Museum crystallized specimens from several localities, on which several other planes besides these occur. The angles of these new planes could not be determined with great accuracy, partly because the crystals themselves are very small, partly also on account of the imperfection of these faces. But as the final results are in tolerably good accordance, and as some of the crystals investigated differ in their form from those hitherto known, I will give here the description of them, adding some remarks on the optical properties of malachite.

The following planes have been observed (Plate VI. fig. 10) :—
100, 010, 110, 101, $\bar{1}02$, $\bar{1}03$, $\bar{1}04$, $\bar{1}12$, $\bar{1}24$, 134 ;
and from the measurements the crystallographic elements

$$a : b : c = 0.8173 : 1 : 0.4231$$

$$ac = 91^\circ 30'$$

were deduced. With them are calculated the angles of the following Table :—

	100.	001.	010.	110.	$\bar{1}10.$
101	61° 27'	27° 3'	90° 0'	68° 17'	111° 43'
$\bar{1}02$	103 6	18 36	90 0	100 7	79 53
$\bar{1}03$	98 19	9 49	90 0	96 26	83 34
$\bar{1}04$	95 53	7 23	90 0	94 33	85 27
$\bar{1}12$	102 50	18 28	78 21	92 32	72 34
$\bar{1}24$	95 45	13 58	78 7	86 58	78 1
$\bar{1}34$	95 37	18 58	72 29	83 25	74 34

For the twin crystals* we have the angles

$$101(100)101 = 57^{\circ} 6'$$

$$\bar{1}04(100)\bar{1}04 = 11 46$$

$$\bar{1}03(100)\bar{1}03 = 16 38$$

$$\bar{1}02(100)\bar{1}02 = 26 12$$

The combinations observed belong to two types. The crystals of the first type, as represented in figs. 1 to 4, are elongated parallel to the axis of the prism (110); the planes at their lower end, by which they rest on the mass, could not be observed, and the crystals appear therefore in the drawings terminated only by the very perfect cleavage-plane 101. The crystals of this group are all twinned, the twin face being 100: the two individuals penetrate one another sometimes in a very complex manner, which may often cause the unevenness of the prism-planes. I succeeded several times in splitting out of such a twin crystal a form of an apparently prismatic character, such as is represented in a section parallel to the plane of symmetry in fig. 5, where the perfect cleavage-planes are marked with the letter *c*. But a slide parallel to this section showed in polarized light the true structure, as the parts marked with the same number become dark at the same azimuth between two crossed Nicol prisms. Similar penetrations of two twinned crystals are found also on other oblique minerals, as for instance in gypsum. Such were also lately described by G. vom Rath on crystals of epidote. The forms in figs. 1, 3, 4 were observed on specimens occurring in a kind of sandstone from Wallaroo and Burra Burra in South Australia. These specimens are either very small crystals of malachite of a light green colour, disseminated on chrysocolla, or the crystals on them are larger and of a darker colour, and occur in the hollows of denser accumulations of crystallized malachite together with blue carbonate of copper. Fig. 2 gives the form of small deep green crystals of malachite from Grimberg near Siegen, on limonite.

Fig. 1.—110, 11 $\bar{2}$, 10 $\bar{2}$, 101. The following angles were observed:—

$$10\bar{1}.10\bar{2} = 42^{\circ} \quad (41^{\circ} 39' \text{ calc.})$$

$$11\bar{2}.10\bar{2} = 12 \quad (11 39 \text{ ,, })$$

$$110.11\bar{2} = 73 \quad (72 34 \text{ ,, })$$

$$11\bar{2}.1\bar{1}0 = 86\frac{1}{2} \quad (87 28 \text{ ,, })$$

* It seems to me more convenient to give in this form the angles between planes of two twin individuals, putting the plane (or zone-axis) on which they are twinned in the middle, than, as has been usual hitherto, to write the plane of the second individual with inverted letters.

Fig. 2.—110, $\bar{1}03$, 101

$$\bar{1}03(100)\bar{1}03 = 16^\circ 24' - 40' (16^\circ 38').$$

Fig. 3.—110, $\bar{1}04$, $\bar{1}24$

$$\bar{1}04(100)\bar{1}04 = 11\ 40 (11\ 46)$$

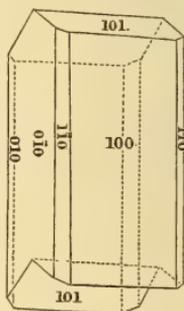
$$\bar{1}24.\bar{1}\bar{2}4 = 22\ 30 (23\ 46)$$

$$\bar{1}04.101 = 34\ 26 (34\ 45)$$

Fig. 4.—110. $\bar{1}24$.101

$$\bar{1}34.\bar{1}\bar{3}4 = 34\ \text{appr.} (35^\circ 2').$$

After the engravings of Plate VI. were done, I found on a specimen of melaconite from Joachimsthal in Bohemia small crystals of malachite showing only the forms 110, 100, 010, 101, but remarkable for the unusual predominance of the plane 100, as is represented in the accompanying woodcut. On some of the crystals the prism planes were not to be found at all.



The second type of crystallization was found on specimens which came from the same locality in Australia, the malachite coating the sandstone in a uniform layer. The crystals are shortened in the direction of the axis of the zone of the prism 100; they adhere to the stone by their posterior part, and seem to occur sometimes also not twinned like fig. 6, which is a combination of 110.100.010. $\bar{1}34$. I found on such a crystal

$$\bar{1}34.\bar{1}\bar{3}4 = 34\ 42 (35\ 2\ \text{calc.})$$

$$\bar{1}34.110 = 83\ 30 (83\ 25\ \text{,,})$$

$$\bar{1}34.1\bar{1}0 = 106\ \text{ap.} (105\ 26\ \text{,,})$$

But other crystals, as in fig. 7, are twinned, the two individuals penetrating each other in the same manner as described before. The prism-planes of the two individuals are very seldom in one plane, but form reentering angles of from 0 to 7 degrees. The crystal fig. 6, a section of which parallel to the plane of symmetry is given in fig. 8, is a combination of 110 and $\bar{1}24$, and yielded the angle

$$\bar{1}24.\bar{1}\bar{2}4 = 23^\circ 22' (23^\circ 46').$$

In this group, however, the planes $\bar{1}24$, $\bar{1}34$, $\bar{1}04$ are striated parallel to their mutual intersection, and therefore cannot be measured with accuracy.

With regard to the optical properties of malachite I have the

following remarks to make. A slide parallel to 101, which is easily obtained by cleavage, shows in the polarizing apparatus one of the optic axes; and it is possible by inclining the slide a little to get also the second axis into the field of view. One perceives in this way that the plane of the optic axes is parallel to the plane of symmetry, and that the optical character of the first mean line is negative. As the crystals are also cleavable parallel to the plane (010), although not so perfectly, it is possible to obtain a slide parallel to the plane of symmetry. Such a slide out of a twin crystal gave, with the aid of a polarizing microscope, for the angle which the axes of greatest elasticity of the two individuals make with one another, the approximate value of $47^{\circ} 40'$, which by the notation before introduced may be expressed by the equation

$$n(100)n = 47^{\circ} 40'.$$

Hence we find

$$n(101) = 4^{\circ} 43'.$$

Let N in fig. 9 represent the normal to the plane (101); A, B the direction of the optic axes in the crystal; A', B' their direction in the air after their refraction at the plane 101; then we find easily

$$\cos AN = \frac{\sin NB' - \sin NA' \cos 2nN}{\sin NA' \sin 2nN},$$

$$\beta = \frac{\sin A'N}{\sin AN},$$

β being the mean coefficient of refraction for malachite. The observed numbers for NA' and NB', and the values of AB and β which follow from these observations, are, assuming for all colours $nN = 4^{\circ} 43'$,

	Red.	Yellow.	Blue.
NA' . . .	29 50	33 30	36 30
NB' . . .	51 0	56 0	60 30
AB . . .	41 54	46 26	48 8
β . . .	1.7886	1.7943	1.8028

These numbers can only be considered as a first approximation to their correct values.

The optical orientation of malachite may be expressed by the symbol*

$$100 \text{ } \alpha = 66^{\circ} 10'.$$

8. On some Artificial Crystals of Gold. By Viktor von Lang.

For the crystals of gold, of which the following is an account, I am indebted to Dr. Percy, who received them from Australia.

* Vide Murmann and Rotter, *Sitzungsber. der Wiener Akad.* vol. xxxiv.

They are the result of an accidental crystallization occurring after the expulsion of the mercury from a mass of the gold amalgam obtained in the ordinary way.

The crystals, which are tolerably large, had a pyramidal appearance from the want at certain planes of the cube and the dodecahedron, as is shown in Plate VII. figs. 1 and 2. Only the planes which are represented in the figures were well developed; no planes could be observed on the posterior side. Several small planes were found to belong to the two forms (1 1 3) and (1 2 3), the latter of which has, I believe, not yet been observed on crystals of gold. The data for the calculation of the indices of this form were (fig. 2),—

	Obs.	Calc.
0 1 2 . 3 2 1 =	60° 30'	61° 27'
0 1 2 . 3 1 $\bar{1}$ =	97	97 46
0 1 2 . 1 0 $\bar{1}$ =	129	129 15
0 2 1 . 3 2 1 =	52 50	53 18

9. On some Combinations of Eudialyte. By Viktor von Lang.

There are in the handbooks of mineralogy only two or three combinations of eudialyte represented by drawings. I have undertaken therefore to add a few more, the forms of which were observed on crystals from Kangersluardsuk, South Greenland, in the mineral collection at the British Museum. In studying these crystals also three new planes were found, so that the planes now known on this mineral are

$$o(111), a(01\bar{1}), b(2\bar{1}\bar{1}), r(100), t(211), y(611), \\ e(011), s(\bar{1}11), h(122), t(20\bar{1}), n(210),$$

y, h, n being the new planes. Fig. 7, Pl. VII. gives the principal zones formed by these planes.

Prof. Miller* found for this mineral, from very careful measurements, the crystallographic element

$$ro = 67^\circ 42',$$

by the aid of which the following calculated angles are deduced.

The observed combinations and the measurements made to determine the position of the new planes are:—

Fig. 3.— $o(111), a(01\bar{1}), r(100), e(011)$.

Fig. 4.— $o(111), a(01\bar{1}), b(2\bar{1}\bar{1}), r(100), y(611), \\ z(211), e(011), s(\bar{1}11),$

$$yo = 56^\circ 44' \text{ calc.}, \quad 56^\circ 48' \text{ obs.}$$

* Phil. Mag. vol. xvi. (1840) p. 477.

Fig. 5.— $o(111)$, $a(01\bar{1})$, $b(21\bar{1})$, $r(100)$, $e(011)$,
 $t(20\bar{1})$.

Fig. 6.— $o(111)$, $a(01\bar{1})$, $b(2\bar{1}\bar{1})$, $r(100)$, $z(211)$,
 $s(\bar{1}11)$, $e(011)$, $h(122)$, $n(210)$.

$eo = 50^{\circ} 38'$	calc.,	$50^{\circ} 18'$	obs.
$ho = 26 0$	„	$25 45$	„
$re'' = 53 5$	„	$52 55$	„
$n'e = 24 3$	„	$23 42$	„

On a crystal of eudialyte with the planes $o(111)$, $a(01\bar{1})$,
 $t(211)$, $e(011)$, $n(210)$ belonging to Dr. Percy, the follow-
ing angles were observed:

$re'' = 53^{\circ} 5'$	calc.,	$53^{\circ} 15'$	obs.
$en' = 24 3$	„	$24 3$	„

Notices of Aërolites. By Nevil Story Maskelyne.

10. *Perth.*

A small stone fell on the field known as the North Inch at Perth on May 17, 1830. The only record of its fall that I have as yet been able to trace is that which accompanied two small specimens of the aërolite, and is in the handwriting of the late Dr. Thomson of Glasgow, in whose collection it was preserved. Mr. Nevill became the owner of the specimens, and presented one of them to the Museum. He very liberally also let me have a microscopic section cut from his own specimen.

The note in Dr. Thomson's writing is as follows:—"Part of a meteorolite that fell on the North Inch of Perth during a thunder-storm on the 17th of May, 1830, at half-past 12 o'clock noon. The mass of which this is a portion was about 7 inches in diameter."

The section of this little stone exhibits a beautiful structure, placing it high in the series of chondritic aërolites. The spherules in it are rather numerous and pretty distinct, and exhibit great variety. The fanned kind of spherule, and some very sharp crystals of a mineral with a diagonal cleavage, are set in a small amount of a granular ground-mass. The general aspect of the stone presents a bluish-grey hue.

The iron in it is present in small particles very sparingly scattered, and also in some amount as fine microscopic (intro-crystalline) dust. The particles of meteoric pyrites are in considerable excess over the iron particles, and there are seen here and there on a section small isolated spots of rust.

Its specific gravity is = 3.494.

11. *Parnallee*.

The larger of the two stones which fell near the village of Parnallee, sixteen miles south of Madura in the Carnatic (lat. $9^{\circ} 14'$, long. $78^{\circ} 21'$), on February 28, 1857, was preserved in the central museum at Madras. It weighs 127 lbs., and has been presented to the British Museum by His Excellency Sir William Denison, K.C.B., Governor of the Madras Presidency.

The smaller stone, weighing about 37 lbs., was presented by the Government of Madras to the gentleman to whom we are indebted for the account of the fall and the preservation of the specimens, the Rev. H. S. Taylor, an American missionary clergyman. He transmitted it to his native country, where it is now preserved in the museum of the Western Reserve College of Hudson, Ohio. This smaller stone has been described in general terms and figured by Professor Cassels in *Silliman's Journal* for November 1861, page 401. Hofrath Haidinger* has also noticed it, and has drawn attention to the beauty of its polished surface.

The description of the circumstances attending the fall of the aërolites was forwarded, mainly through the instrumentality of Mr. Taylor, to the Madras Government, and may be epitomized as follows. The two stones fell two or three miles apart, the larger one to the north of the smaller one and a few seconds before it. How the priority in time of the fall of the larger stone came to be observed is not recorded; nor is it very easy to see how it could be established, as neither stone was seen during its passage through the air.

Judging from the inclination of the hole made in the ground by the larger mass, and which measured 2 feet 5 inches in depth (whether inclusive or exclusive of the huge stone is omitted from the account), it was estimated that it came from a direction about 10° west of north, and at an angle with the perpendicular of some 15° or 20° . Mr. Taylor, however, speaks of the small stone, like the large one, falling "about perpendicularly." It would be a curious fact if the smaller stone flew further than the larger; but the difference in the resistance offered to their passage through the atmosphere by their very different forms may explain this if the statement be correct. The reports were like two claps of thunder, and were heard as far off as Tuticorin on the Gulf of Manar, a distance of 40 miles nearly due south. This last fact, coupled with the idea some of the natives had formed that the stones had been shot by guns on ships from Tuticorin, or had been brought by a Brahman by his *muntrums* from the sea, incline me to believe an E. by S. to a W. by N. direction to have been that of the aërolites rather than the opposite direction, as

* Sessional Proceedings of the Vienna Academy, 1861.

supposed by Mr. Taylor. The stones, however, as before observed, were not seen in the act of falling; the dust only was seen which their concussion with the earth raised into the air; and Mr. Taylor's observations seem to have been made on the whole with so much intelligence as to give weight to his opinion on a subject upon which a person on the spot ought to have the best means of forming a judgment.

The large stone now preserved in the British Museum is certainly one of the most interesting, as it is one of the largest of existing *aërolites*.

Its form, as seen from two opposite points, is represented in Plate VIII. It is difficult to define the form of so irregularly shaped a mass. Two nearly opposite sides of it are comparatively smooth; the remainder is one mass of pit-marks, sharp, deep, and irregular, and in two or three cases the pit-marks are deep holes or fissures penetrating to some depth into the stone. The crust is of a grey brown, tolerably thick, but on one of the much-pitted sides presenting the aspect of a less thick coating than is seen elsewhere on the *aërolite*, and probably indicating a surface of disruption—perhaps the one from which the smaller Parnallee stone may have been broken while the whole *aërolite* was in its transit through the atmosphere.

The material of the stone takes a good polish, and presents a beautiful mottled surface. It is in fact a mass of spherules, and though not so varied as the really marbled and veined stone of Akbarpúr, or the coarse breccias of Assam, Aigle, &c., it is not less striking than any of these, from the peculiar character imparted to its polished surface by its highly spherular structure and compact nature.

In the microscope it forms the most beautiful and instructive object I have yet seen among the sections of *aërolites*. Like Borkut in the number of its spherules, it differs from that singularly pisolitic stone in the compactness with which its spherules are compacted into a solid whole; indeed it is probably to this compact solidity of structure that we are indebted for its falling in so large an unbroken mass.

The spherules are well defined in form, and are cemented together in a great degree by meteoric iron and by a considerable amount of meteoric pyrites, each of which, but especially the pyrites, is often, as it were, moulded round and between the closely packed spherules. They are mixed, and their interstices filled with a dark mineral (*Hyalosiderite*? or *Fayalite*?) in small amount. The marvellous variety revealed by the microscope in the structure and contents of these spherules makes a slide of the Parnallee stone an *aërolitic* microcosm in itself. Some are seen to consist of porphyritic assemblages of crystals held in a

compact homogeneous granular mass; others exhibit in their sharpest features either the barred, the fanned, or the mottled minerals, of which I shall give descriptions in a future article*. The iron, again, may be seen in every condition as irregularly shaped grains and particles, or as microscopic dust; or it may be found entangled in or entangling sponge-like masses of meteoric pyrites; while either of these bodies may be seen in turn penetrating the spherules, or containing a spherule completely enclosed in them. / Or, again, we may find one of those curious veins of metal or of meteoric pyrites, or of these associated with other minerals highly charged with oxide of iron, which betray a stage in the history of the aërolitic rock subsequent to that in which the mass had become compacted as we now see it, a stage during which it underwent a subsequent splitting along a direction now indicated by the narrow vein that fills the fissure, and which, running through the spherules, has divided them and "heaved" them. Such a vein brings evidence to prove, by the non-continuity of the outlines of the spherules on either side of it, that there have been stages in the progress of the slag-like mass from the first origin of the spherule—in perhaps a seething lake of mixed and molten metals on which a rare oxygenous atmosphere was acting and fermenting out as it were the more oxidizable elements—to the final state of compact continuity in which the spherules are found agglutinated together or imbedded in a magma of mineral. /

Parnallee is in these respects more singular, and it presents also a more beautiful object in the microscope than do even Bor-kut, Mezö-Madaras, Bremevörde, Tabor, or Seres.

Its specific gravity = 3.41.

12. *Durala*. Plate IX.

The aërolite which fell at Durala on February 18, 1815, was presented, in the state in which it fell, to the East India Company. After remaining in the library of the East India House till the change of government took place, it was presented in 1861 to the British Museum by the Secretary of State for India in Council. The following circumstances attending its fall are taken from the Report of the British Association for 1850.

Extracts from a letter from Captain G. Bird, first assistant in the Political Department, to Major-General Sir D. Ochterlony, Bart., K.G.C.B., to Major Penington:—

* In a subsequent Number I propose giving a more systematic and detailed account of the appearances presented generally by the different varieties of spherule, and of the magma that contains them, in chondritic aërolites.

“Loodianah, April 5, 1815.

“On the 18th of February last some people who were at work in a field about half a mile distant from the village of Dooralla, were suddenly alarmed by the explosion of what they conceived to be a large cannon, ‘the report being louder than that of any other gun they had ever heard,’ which report was succeeded by a rushing noise like that of a cannon-ball in its greatest force. When looking towards the quarter from whence the noise proceeded, they perceived a large black body in the air, apparently moving directly towards them, which, passing with inconceivable velocity, buried itself in the earth at the distance of about sixty paces from the spot where they stood. The Brahmins of the village hearing of it, proceeded to the spot with tools for digging it up.

“They found the surface broken, and the fresh earth and sand thrown about to a considerable distance; and at the depth of rather more than 5 feet, in a soil of mingled sand and loam, they found the stone which they cannot doubt was what actually fell, being altogether unlike anything known in that part of the country. The Brahmins conveyed it to the village, covered it with wreaths of flowers, and started a subscription for the purpose of raising a small temple over it. It fell on the 18th of February about midday, in a field near the village of Dooralla, which lies about lat. $30^{\circ} 20'$, long. $76^{\circ} 41'$, within the territory belonging to the Pattialah Rajah, 16 or 17 miles from Umballa, and 80 from Loodianah. The day was very clear and serene, and, as usual at that season of the year, not a cloud was to be seen, nor was there in the temperature of the air anything to engage their attention; the thermometer therefore may be stated at about 68° in the shade. The report was heard in all the circumjacent towns and villages to the distance of 20 coss, or 25 miles from Dooralla. The Rajah, having been led to consider it as a messenger of ill-omen, according to my wish gave immediate orders for its conveyance to Loodianah, but with positive injunctions that it should not approach his place of residence. It weighs rather more than 25 lbs., and is covered with a pellicle, thinner than a wafer, of a black sulphurous crust, though it emits no smell of sulphur that I can discover. It is an ill-shapen triangle; and from one of the corners a piece has been broken off, either in its fall or by the instruments when taking it out of the ground. This fracture discloses a view of the interior, in which iron pyrites and nickel are distinctly visible. No Hindoo ventures to approach it but with closed hands in apparent devotion, so awful a matter is it in their eyes.”

“This aërolite was brought from India by Lieut.-Colonel

Penington, and presented to the Hon. East India Company, who have deposited it in their museum."

With the exception of a small portion broken off at what may be termed the apex of the aërolite, the entire stone was sent to Europe. Its form is rudely pyramidal, of which the edges, especially where they culminate at the broken apex, are rounded so as to give a convex character to that part of the mass. Ridges and valley-like depressions converge towards the apex, which, while they grow deeper and more marked the further they recede from it, become mere linear markings or cracks in the crust where they traverse the convex surfaces that would culminate on the summit. On the base of the aërolite, on the underside, namely opposite to the apex, the crust is accumulated in pitch-like prominences; in other parts it presents a smooth dull aspect, and has a dark rich brown colour. It exhibits fissures and lines, resulting no doubt from contraction as it cooled. It has also small shining dark points which correspond apparently with spherules.

The structure of the Durala stone is that of a rather loosely aggregated chondritic aërolite. The ground-mass is of a pale grey hue, exhibiting a whitish granular mineral interspersed among waxy nodules. Like Nellore, it does not take a polish. It differs from Nellore in the distribution of the iron, which is scattered in somewhat larger particles, and less evenly than in that aërolite. Some of the iron granules are as much as $\frac{1}{12}$ th of an inch in diameter. On the other hand, in no aërolite that I have seen is this metal diffused in a more minute form of dust than in Durala. The ground-mass exhibits under the microscope a larger amount of the granular mineral than Nellore; and spherules and nodules, some smooth, some angular in outline, are distributed through this mass, and most frequently present under the microscope the usual characteristics of the waxy (probably olivine) spherule. Occasionally, too, we see a barred structure sometimes fanned out and radiated, or a mineral with a mottled aspect and without much colour in polarized light, or very definite planes of polarization. Other spherules, again, exhibit a number of crystals within them enclosed in a mass with sometimes a hexagonal outline, but not with an orientation exhibiting any evident morphological relation to that outline.

The individual minerals are very much like those in Nellore; and a very few crystals of a felspar-like mineral may be also detected in Durala.

The cut face has two metallic veins running across it. Its specific gravity = 3.53.

13. *Nellore*.

This fine mass, which fell at Yatoor, Nellore, lat. $14^{\circ} 18'$, long. $79^{\circ} 46'$, at half-past 4 P.M. on January 23, 1852, was presented to the Museum by His Excellency Sir William Denison, K.C.B., Governor of Madras. The history of its fall is recorded in the following deposition:—

“Deposition of Chella Vencataramoodoo, an inhabitant of Yatoor in the Talook of Toomalatalpoor, dated the 25th of January, 1852.

“Q.—The circumstance of a sound being heard and a ball falling on the lands of Yatoor Village was communicated by the village moonsiff. State where you were then, whence and how the ball fell, and all you know about it.

“A.—At about half-past 4 o'clock P.M. the day before yesterday, I and Cherukoor Soobadoo were grazing cattle near the Chouta Vagoo, south of Yatoor; and just about five or six ghadies, before it was dark, a noise as of the firing of a musket was first heard, and as we were looking towards the west we heard a rumbling noise for a moment, and espied at the distance of about forty yards dust rising to the height of a man. We three then went to the spot, and found a hole made in the ground 2 spans deep and 2 spans wide, and a white stone in it; and as it was not possible to take it out, we took a small piece of that stone that was on the ground and informed the circumstance to the village moonsiff, who went to the spot yesterday morning and dug up the stone. The soil of the place where the ball fell is composed of clay and soda.

“Q.—You stated that you were at the distance of forty yards when the ball fell; did you see then any lightning?

“A.—I did not observe any lightning; the sound was indeed an extraordinary one; the sky was clear and calm; the cattle ceased grazing, and lifting up their heads, ran away in amazement. We were struck with fear and looked all round.

“This mark of Chella Vencataramoodoo taken before me on the aforesaid date. (Signed) T. Marcaudaloo, *Tahsildar*.”

The evidence of another witness was to precisely the same effect.

The foregoing facts have been quoted by Hofrath Haidinger from the same source as that from which I have drawn them.

The weight of the mass as it arrived in the British Museum was 30 lbs. Its specific gravity = 3.63.

Its form was rudely prismatic, one end and two sides and the truncated edge of a third coated with crust, the whole of which is much pitted with hollows. The colour of the crust is a deep reddish brown (coloured apparently by ferric oxide). It presents cracks (as if from cooling), and in some parts it has the pitch-

like appearance seen when the crust of an aërolite has become accumulated. This is most the case on the end of the meteorite. It is thick and of a dull lustre, and there is no evidence of successive formation in different parts of it.

In structure the Nellore aërolite may be described as composed of a friable and porous, pale bluish-grey ground-mass, very much stained in patches by ferric oxide, and containing many small semiopake, occasionally grey, but often greenish black irregularly formed spherules, and also much granular iron distributed pretty evenly in medium-sized particles, with a few larger ones scattered at greater intervals. Meteoric pyrites is also seen associated with the iron to the amount of about one-third its bulk, and occasionally also minute particles of a yellow mineral, which is probably iron pyrites. There is also a little of a black mineral which appears to be chromite. Examined more closely by the microscope, the magma of this aërolite is found to be formed of distinct crystals and crystalline aggregates of minerals, the latter forming often a fine grey sort of network in which the former are set. This aggregated granular but crystalline mineral is like one which forms an important feature in Bachmut and the aërolites of its class, in which it is seen surrounding and entangling as in a network the other minerals. It owes its grey hue sometimes to microscopic particles of iron and often to minute fissures, among which what appear to be hollow bubbles without liquid in them are in Nellore occasionally to be discerned.

The crystals which are suspended in this ground-mass are of different kinds. The most frequent is a mineral with but little cleavage, and that parallel to the planes of polarization, which doubtless consists of olivine. There are also present minerals similar in general appearance and characters to the last, but exhibiting a greater amount of cleavage, and that in directions diagonal to the prismatic outlines of the crystal and oblique to the planes of polarization, though the angles vary with the direction of the section. The directions of the planes of polarization in these minerals usually indicate the prismatic system as being probably that of this form of mineral. But a few cases occur in which a mineral is seen the system of which does not seem to be prismatic, as both the symmetry of the crystal (the section of which generally affects the form of a rhomboid) and the directions of the planes of polarization suggest an oblique or anorthic form. This latter mineral is possibly a felspar, as its crystal generally presents itself in juxtaposition with another similar crystal, evidently, from the different directions of its planes of polarization, twinned with it. The former mineral will perhaps be found to consist of Enstatite, the *prismatic* magnesian augite (if it may so be termed), for our acquaintance with the true cha-

acters of which we are indebted to the refined optical investigations of M. Des Cloiseaux.

The spherules appear in different aspects under the microscope: greenish-brown wax-like nodules with a laminar structure are frequent among them, while others exhibit a more decidedly crystalline appearance. In section these nodules present the characters (and the probably prismatic system) of what for the present we may call the fanned mineral—grey in tint from an infinitesimal dust of iron and other foreign matter chiefly accumulated between the laminae; and they seem to pass by different stages into a mottled mineral (or minerals) which, from a confusedly crystalline structure, acts on polarized light as agate does. This mottled form of mineral in particular cases may be seen to consist of the fanned or the barred mineral seen in a section cut across the fibre, so to say, of the prisms, and presenting therefore a vast number of sections of these in confused arrangement. It would be an arduous task, and take a large amount of material, to pick out enough of the several varieties of these nodules from the bruised fragments of an aërolite to furnish a sufficient quantity for a trustworthy analysis. But the fanned and mottled minerals are among the very few the spherules of which might perhaps be so treated in this class of aërolites. Some of the spherules exhibit a quasi-crystalline exterior, and these are generally such as consist of crystals, and in Nellore generally of one imperfect individual crystal, usually surrounded by a fringe of irregularly disposed and minute crystalline fragments; while in other aërolites (as in Aussun for instance) we find such spherules often to contain perhaps four or five largish and well-defined crystals with a number of minuter ones interspersed between them. But the planes of polarization, and therefore the orientation, of each crystal seem quite independent of those of the rest, and the apparent crystalline outline of the spherules seem seldom in these cases significant of an internal crystallographic structure.

The singular observation of the natives, that the Nellore stone was white when it fell, may be accounted for on the supposition that it has acquired its rusty stains since. This rusty character, so obvious a feature of the aërolites of Mainz, Charwallas, and Segowlee, and in a less degree of Nellore among others, is certainly the result of oxidation since their fall. The difficulty of taking the specific gravity of an aërolite illustrates its porosity. Even the most compact aërolites absorb much water in that process. In the microscope the stains of ferric oxide are seen surrounding the larger granules of metallic iron, curiously distinguishing them from the meteoric pyrites; but

they are confined to the fissures that penetrate the crystalline magma in which the iron particles are enclosed.

Thus, of Segowlee the Asiatic Society of Calcutta sent to London a very small complete stone with a minute fracture at one end. That fracture seemed bluer than the large fissured mass of Segowlee, of which pieces had before come to Europe. I obtained Dr. Oldham's assent on behalf of the Society to a section being made of it; and the result was a surface exactly like in structure and material to the original Segowlee, presenting the same irregular mixture of crystalline granules of all sizes interspersed with rather large olivine-like crystals ill defined in their outline, and here and there with spherules remarkable for the illustrations they afford of the fanned and radiated mineral rich in iron spangles. But, unlike the brown Segowlee stone, it was as free from the iron-stains which give its brown hue to that *aërolite*, as are the curious stones of Klein-Wenden and Erleben, with their thin crystalline spangles of iron fixed in their clear, colourless, and transparent crystal setting.

The specific gravity of Nellore = 3.63.

14. *Dhenagur*.

An *aërolite* somewhat similar to, but less chondritic than those of Durala and Nellore fell at a place called on the label Kheragur, on March 28, 1860. This *aërolite* was presented by Major Bouverie, the Governor-General's agent at Bhurtpúr, to the Asiatic Society of Bengal, and is stated in their 'Proceedings' for 1860, p. 212, to have fallen at a village about 15 miles south of that place. There is also in the same volume a notice in a letter from R. F. Saunders, Esq., Deputy Commissioner at Kangra, in which he describes the Dhurmnala fall, and incidentally states that an *aërial meteor* or waterspout (!) had taken place in the neighbourhood of Bhurtpúr, "where an *aërolite* is said also to have fallen." There is no place of the name of Kheragur to be found on the great Survey Map of India; but S.W. of Bhurtpúr, at some 10 or 12 miles distance, is a village called Dhenagur. I have little doubt that the name Kheragur is a mistaken reading of this name, the *D* and the *n* being readily mistaken for *K* and *r*. There is a Kyreegur in Oude west of a little place called Bhurtpúr, but it is not likely to be the place, as the Bhurtpúr known all over India is that in the district which takes its name from it. Pending a greater certainty as to the position reaching me from India, I shall call this *aërolite* Dhenagur, and I hope on a future occasion to give an account of the circumstances attending its fall.

The specimen in the British Museum was the large half of

the fragment preserved in the collection of the Asiatic Society of Bengal, and was included among the specimens presented to the Museum by that Society.

The crust is very black and dull in lustre; it is thick and somewhat foliated. The portion retained by the Society exhibits this crust on three of its sides.

The ground-mass of this aërolite consists of a nearly white mineral highly charged with spherules of greenish-yellow olivine (the waxy mineral), in some cases very dark in hue, and presenting under the microscope the characteristics of that mineral as exhibited in other aërolites.

It is not sufficiently compact to take a polish.

There is less iron apparent than is the case in Nellore, though more than in Durala; and it is in rather large grains.

The meteorite is somewhat stained by patches of rust round the iron granules; the result, no doubt, of its porous and loosely aggregated character.

There is a good deal of meteoric pyrites, somewhat crystalline and generally in association with the iron.

By reflected light in the microscope a white opaque flocculent mineral is seen in some little quantity, rarely clouding the other minerals present as it does in Bishopville, Mallaigaum, &c., but usually in fragments and small nodules (as in Borkut, Seres, and some other aërolites) intermixed with the other materials of the stone.

Specific gravity = 3.391.

15. *Mhow*.

The village of Mhow (or Mow) stands on the right bank of the Tons, an arm of the Gogra. It is about 37 miles north of Ghazepúr, and 30 miles south-east of Azimqur, and 57 miles south-east of Goruckpúr, lat. 25° 54', long. 83° 37'. Five miles from this village, at 3 o'clock in the day, on February 27, 1827, an aërolitic shower fell from a serene sky. The largest stone recorded weighed 3 lbs. Fragments were picked up four or five miles apart, one of which broke a tree, and another wounded a man. The fall was accompanied by noises like the roar of ordnance.

The fragment preserved in the collection of the Asiatic Society at Calcutta weighed 12½ ounces, and for a portion of that specimen the National Museum is indebted to the liberality of that important Society.

The specimen exhibited in the Museum weighs 5¼ ozs. The original specimen, when entire, was of a peculiar form. Rudely four-sided and somewhat pyramidal, it presented a curved shape, so as to look like a fragment splintered from a shell or a hollow

mass. It was coated with crust on all its sides, but its ends were not incrustated. The crust was dark brown, in parts black, in some places with a foliated appearance. It was rather lustrous, but not approaching in brilliancy to the enamelled coating of the *eukritic* varieties of *aërolites*. Its thickness was equal over the whole surface, and was considerable. Here and there darker and more lustrous spots were seen, and on the convex and concave sides there were shallow pittings.

Under the microscope, in texture and material the Mhow *aërolite* resembles very closely the *aërolite* of Bachmut, and approaches it in specific gravity—that of Bachmut being 3·596, while that of Mhow is 3·521.

The structure of this meteorite is that of a tolerably compact bluish-grey mass containing spherules disseminated sparsely through it, very variable in size, and but little more compact than the magma that contains them. The larger iron particles are generally surrounded by stains of ferric oxide. The mottled bluish-grey colour seems due to the admixture of a white or pale brownish-yellow with an iron-grey mineral.

The iron in it is rather abundant, partly as largish irregular granules, partly as small particles, and also as an infinitesimally minute dust disseminated especially through the iron-grey mineral.

Meteoric pyrites is present in the proportion of perhaps one-fourth or one-fifth of the bulk of the iron.

A black mineral (*chromite*?) is also present in some quantity in parts of the *aërolite*, as also several others.

A microscopic inspection of the materials forming the Mhow stone exhibits in it a good deal of a granular mineral, forming a magma in which the spherules and many distinct crystals are set. This magma is a congeries of (probably for the most part olivinous) granules aggregated into a mass which is not at all compact, and is similar to the ground-mass of Bachmut and the *aërolites* analogous to it, but of rather larger grain than is the case in Bachmut. In the spherules, which are by no means a marked feature of this *aërolite*, we find the grey mineral which I have before described as occurring in prisms and sometimes with a fanned or radiated structure. There are also large spherules consisting of a mottled mineral, and containing iron in small flakes dispersed as if spurted through it.

Besides these there is a substance in very clear, little, colourless crystals possessing a diagonal cleavage, and apparently belonging to the prismatic system. These form groups that, with some of the granular mineral intervening, build up spherules; but they are sometimes isolated and outside the spherules. In the latter state some transparent crystals are also to be seen with a pale

flesh-tint like the olivine in Lherzolite when seen by transmitted light.

It seems not to be dichroic; it has a cleavage parallel to a plane of polarization, and is probably olivine.

16. *Moradabad.*

An aërolite fell in the densely peopled district of Moradabad (in Rohileund) in the year 1808. The precise date and the circumstances attending its fall do not seem to be known; but a small portion was preserved at Calcutta in the Asiatic Society's Museum, and the British Museum owes to the liberality of that Society the fragments it possesses.

Its place in a classification of aërolites would be near Château-Renard and Bachmut. Coarser-grained in the granular ground-mass than the latter of these stones, and therefore much more so than the former, it exhibits fewer isolated crystals than Bachmut, and in this respect is nearer to Château-Renard.

The chondritic character of the stone is barely asserted by a very few grey spherules, of which about two may be seen on a section of half an inch square. The iron is present chiefly in very small particles, very little in microscopic dust, and is associated with a considerable proportion, perhaps an equal bulk, of meteoric pyrites. Rust-stains are seen in small isolated patches on the section. The crust is rather thick and beautifully black. From the smallness of the fragments it was difficult to obtain a specific gravity, even of the imperfect value usually obtainable with aërolites. The result it gave was 3.143.

17. *Paulograd.*

My friend Mr. Greg, in an article in the *Philosophical Magazine* for January (Suppl. to vol. xxiv.), has given an account of some very interesting fragments of aërolites, for the acquisition of which the National Collection is directly or indirectly indebted to him. One of these is the stone of Ekatharinoslav. The doubt about this stone was first raised, I believe, as some other doubts regarding specimens in the British Museum have been, by gentlemen at Vienna; and Hofrath Haidinger has published a notice about the aërolite in question. A specimen of this fall does not exist in the Vienna Collection; but the authorities there are making efforts to obtain one from Odessa, which I trust may be crowned with success. The grounds of the doubt thus raised may be stated to be, that the Paulograd stone in the British Museum is one of the many stones very similar to Bachmut, and that Bachmut is in the same government as Paulograd—both being in that of Ekatharinoslav, and distant from each other some 60 or 70 miles. These arguments are

certainly in favour of the *à priori* assumption that the stones are fragments of the same fall. To these may be added the statement in the Allan Catalogue, that the fall took place in the year 1825, whereas the Paulograd fall is well authenticated as having occurred on May 19, 1826—that at Bachmut having occurred on February 3, 1814.

Mr. Greg has, however, so effectually disposed of these rather plausible objections that I will not repeat his arguments. I content myself with stating the terms in which Mr. Allan described his acquisition when it was fresh in his collection, and with making an observation or two of my own. Mr. Allan's note, in his own handwriting, is:—

“1828. No. 15. Fragment of a stone weighing about 85 lbs. fell in 1825 in the government of Ekatharinosloff; the principal mass is deposited in the Museum at Odessa: given me by Dr. Dowler; the texture of this stone is remarkably distinct. Polish Ukrain.”

The question arises, Was Dr. Dowler likely to have made a blunder between the years 1814 and 1826, rather than that the 1826 should have been mistaken for 1825, a very probable error in a copyist? He was physician to Count Woronzow, then recently appointed governor of that part of Russia, and resident at Odessa. As the entry is made in 1828, the present must have been given by Dr. Dowler within two years of the Paulograd fall, which happened within the jurisdiction of the illustrious Russian in whose service he was, and which was far more likely to have excited Dr. Dowler's interest than that of 1814.

The fall of 1814 at Bachmut consisted, I believe, of two masses, severally of 40 lbs. and 20 lbs. in weight, the history of which is known. This mass of 85 lbs. could hardly be confounded with either of them.

Under the microscope, the stone from the Allan Collection exhibits all the characters of the sparsely chondritic class of aërolites, of which Bachmut is an example. It closely resembles this latter stone in the granular material that forms the ground-mass of both, though perhaps that of the Paulograd stone is the finer in texture, while the isolated crystals in both stones are very similar in character, but the Bachmut stone seems somewhat the less rich in them.

Their specific gravities (that of Paulograd = 3.584, and that of Bachmut = 3.596) present too small a difference to support any argument against their identity. I have no hesitation in retaining the name and date of the Paulograd stone as the true designation for the Allan aërolite, as I have entire confidence in its pedigree; and I sincerely hope soon to hear that the Vienna Collection has obtained a fine mass of it from the original at

Odessa, and that their anxiety on behalf of the authenticity of the specimen in the British Museum is set at rest.

18. *Pleskowitz Aërolite.*

Mr. Greg does not confine himself in his paper of January last to his programme, which included only specimens "with which he has happened to have had to do at various times." He enters also on the discussion of two stones with which he had but a slight acquaintance. One of these was the stone of Pleskowitz.

The British Museum has the good fortune to possess a specimen of this fall. The indefatigable researches of M. Kesselmeyer have brought to light the fact that specimens of it were preserved at Prague, as he has also traced the history of a specimen of the Tabor fall into the collection of Colonel Greville, a collection that became the property of the British Museum in 1810. But the original specimens of the Pleskowitz and Liboschitz fall appear to have disappeared from notice at Prague; and it is difficult at present to ascertain how Heuland, from whom this specimen was purchased, became possessed of it.

That he attached a very high value to it and appreciated its true character is evident from the terms in which he describes it in the original invoice, and indeed also from the price paid for it; these terms are as follows:—

"Meteoric stone (weight $1\frac{1}{4}$ oz.) from Reichstadt, Bohemia; fell the 2nd of June, 1823, and is the only specimen seen by Mr. Heuland from that locality. It is surrounded by its crust, and both surfaces show an unusually compact character; it is mixed with brown oxide of iron. £4."

Were there any cause to doubt the honesty or the accuracy of this description of Mr. Heuland's, it would probably be founded upon the external similarity of the falls of Pleskowitz and Tabor.

It was on this similarity that Mr. Greg founded his own doubt. But in point of fact this comparison confirms in a twofold manner the genuineness of the Pleskowitz specimen: first, by the fact of their *apparent* external similarity, and secondly, by the great differences the two stones exhibit in their internal structure. I am greatly indebted to M. Kesselmeyer for the courtesy with which he has communicated to me everything he has as yet ascertained in his laborious researches among old writings about these aërolites. That gentleman informs me of a passage in Dr. John Mayer's work, *Beytrag zur Geschichte der meteorischen Steine in Böhmen*, Dresden, 1805, wherein Mayer quotes from a most scarce treatise (not in the British Museum) by Stepling, *de Pluviâ lapideâ anni 1753, ad Strkow et ejus causis Meditatio*, Pragæ, 1754, in which he describes no less than three stonefalls in Bohemia, viz.:—

in 1723 on the 22nd of June, at Liboschitz and Pleskowitz ;
 in 1743 —————, likewise at Liboschitz ;
 and 1753 on the 3rd of July, at Plan and Strkow near Tabor ;

and this, says he, is exceedingly wonderful that the *stone he* (Stepling) *has preserved from the stonefall of 1723 at Liboschitz and Pleskowitz is exactly like the stones of 1753 from Tabor!*

Mr. Greg wrote to ask me whether the Pleskowitz stone was like that of Tabor, and whether I did not think the two stones might be in fact identical. My reply was, that the two were externally curiously alike, but that a closer inspection showed the more rusty appearance of the surface of Pleskowitz to be accompanied by a greener tint in the ground-mass. I have since, by order of the Trustees, had both stones cut and polished, and microscopic sections made of them. The answer is decisive. The two stones are entirely distinct, and present no point of similarity. The Pleskowitz belongs rather to the granular than to the more emphatically spherular varieties of chondritic stones, of which latter Tabor is a characteristic illustration. It contains far less iron than the latter aërolite, and the metallic grains are more clustered and less evenly distributed than is the case in Tabor. In Tabor, the rust-stains are dotted somewhat dendritically over the whole face of a section; in Pleskowitz they form patches rather bright in colour, much seen at the outer part of the section, even where the crust occurs, and far less in amount in the interior, large parts of the area of which are quite free from them. The proportion of meteoric pyrites to meteoric iron is rather higher in the Pleskowitz stone, and it is disseminated in more minute particles. This stone also contains the microscopic kind of iron particles in rather greater abundance than Tabor. When I add that the specific gravity of Tabor is 3.693, and that of Pleskowitz is 3.491, as determined with much care from specimens in every respect similar, and that I know as yet, out of fifty of the most important falls that I have examined in section, not one that is like Pleskowitz, I think the original accuracy of Heuland's description will be allowed to have been vindicated.

19. *Wiborg*.

The second of the two stones regarding the authenticity of which Mr. Greg has raised doubt, though himself not connected with their history, is that called in my catalogue Lontalux (Loutolax or Luotalaks).

I hope soon to obtain a specimen of the true Loutolax for comparison; but I feel convinced that the stone so named in our collection cannot be correctly designated. The description given by Rose of the true Loutolax, which he places in his

"Howardite" class, and the specific gravity as recorded by other observers (3·07), are quite inconsistent with our very highly chondritic stone called by this name being a true specimen of the Loutolax fall. Its specific gravity is 3·671-3·674. Mr. Greg imagined it might be a specimen of Timochin erroneously labelled. But it differs from that *aërolite* very widely in the character of its spherules, which are singularly round, numerous, and distinct, —and also in the rust-stains—a very good characteristic *where aërolites exhibit them*, as the mode of distribution of the stain depends on the nature of the porosity of the stone and on the mode of dissemination of the iron, which are both important and constant features.

The iron in both is very similarly distributed; but the stains are more patchy in the Timochin stone, and the spherules are not so sharply relieved by their colourless character from the rust around them. Their specific gravities are also very nearly the same, that of Timochin being 3·636.

I cannot as yet find any meteorite quite like the stone in the British Museum, though, until I had sections made, I believed Barbotan to be so. It is, however, a very different stone. I should make one remark which I do not think is without importance. This stone was obtained from that sound mineralogist and generally accurate observer Heuland. His original description of it, preserved in the archives of the Museum, described it as a stone that fell at *Wiborg* in *March* 1814; whereas the stone known as Loutolax fell on December 13, 1813. Heuland does not mention the name Loutolax. It is not impossible that *aërolitic* falls took place in the neighbourhood of *Wiborg* on these two several dates; but until some further facts are ascertained on the subject, either confirming or condemnatory of this hypothesis, I shall place the "*Wiborg*" stone of the Museum among the many specimens of problematical *aërolites*.

LXI. *Sequel to the Theorems relating to "Canonic Roots" given in the last March Number of this Magazine.* By J. J. SYLVESTER, F.R.S.*

THE theorems kindly communicated from me by Mr. Cayley in the March Number of this Magazine were originally designed to appear as a note or *excursus* to a memoir in preparation on the extension of Gauss's method of approximation from single to multiple integrals by a method which invariably leads to the construction of a *canonizant* whose roots are all real. To establish this reality, recourse may advantageously be had to a

* Communicated by the Author.

theorem of Jacobi, given at the end of his well known memoir "De Eliminatione Variabilis e duabus æquationibus," *Crelle*, vol. xv. p. 101, a very slight inspection of which at once leads to the further and interesting inference that the resultant of the canonizant of an odd-degreed function of x and *unity*, and of the canonizant of the second differential coefficient of that function in respect to x , is an exact power of the *catalecticant* of the first differential coefficient of x in respect to the same. This is the essence of the matter communicated by Mr. Cayley; but subsequent successive generalizations of the theorem have led me on, step by step, to the discovery of a vast general theory of double determinants, *i. e.* resultants of bipartite lineo-linear equations, constituting, I venture to predict, the dawn of a new epoch in the history of modern algebra and the science of pure tactic.

I will begin this note upon a note, by reproducing in brief the first of my two demonstrations of the simple theorem in question*. Let us write

$$X_0=1, \quad X_1 = \begin{vmatrix} 1 & x \\ a & b \end{vmatrix}, \quad X_2 = \begin{vmatrix} 1 & x & x^2 \\ a & b & c \\ b & c & d \end{vmatrix}, \quad X_3 = \begin{vmatrix} 1 & x & x^2 & x^3 \\ a & b & c & d \\ b & c & d & e \\ c & d & e & f \end{vmatrix}$$

and so on. And again, let

$$\lambda_1 = a, \quad \lambda_2 = \begin{vmatrix} a & b \\ b & c \end{vmatrix}, \quad \lambda_3 = \begin{vmatrix} a & b & c \\ b & c & d \\ c & d & e \end{vmatrix}$$

and so on. The theorem in effect to be proved is simply this, that the resultant of X_i and X_{i-1} is an exact power of λ_i , which (as will at once be seen) is the coefficient of x^i in X_i . In what follows, I shall use $R(P, Q)$ or $R(Q, P)$ to denote indifferently the positive or negative resultant of any two functions P and Q , ignoring for greater simplicity all considerations as to the proper algebraical sign to be affixed to a resultant of two functions taken in an assigned order.

Jacobi's theorem above referred to, stated so far as necessary for the purpose in hand, is as follows:

$$X_n = (Ax + B)X_{n-1} - \frac{\lambda_n^2}{\lambda_{n-1}^2} X_{n-2}.$$

Hence, by virtue of a general theorem of elimination †,

$$R(X_n, X_{n-1}) = \lambda_{n-1}^2 R\left(-\frac{\lambda_n^2}{\lambda_{n-1}^2} X_{n-2}, X_{n-1}\right);$$

* The second has been communicated by Mr. Cayley in the March Number of this Magazine.

† This theorem is best seen by dealing in the first instance with U, V , any two homogeneous functions of x, y of degrees $n, n-1$ respectively

or, neglecting as premised all considerations of algebraical sign,

$$= (\lambda_{n-1})^2 \left(\frac{\lambda_n}{\lambda_{n-1}} \right)^{2(n-1)} \cdot R(X_{n-1}, X_{n-2}),$$

i e.

$$\begin{aligned} \frac{R(X_n, X_{n-1})}{\lambda_n^{2(n-1)}} &= \frac{R(X_{n-1}, X_{n-2})}{\lambda_{n-1}^{2(n-2)}} = \frac{R(X_{n-2}, X_{n-3})}{\lambda_{n-2}^{2(n-3)}} \\ &= \&c. = \frac{R(X_1, X_0)}{\lambda_1} = 1; \end{aligned}$$

or if any one of my readers finds a difficulty in admitting that $R(ax-b, 1) = a$, he can stop short at $\frac{R(X_2, X_1)}{\lambda_2^2}$, which may easily be verified to be equal to unity. Hence

$$R(X_n, X_{n-1}) = \lambda_n^{2n-2}. \quad \text{Q. E. D.}$$

Thus we see that if X_n, X_{n-1} have one root in common, λ_n must vanish; but then, by the cited theorem of Jacobi, it follows that X_n completely contains X_{n-1} ; from this it was easy to infer the necessity of the function* of which X_n is the canonizant, having infinity for one of its "canonic roots"—or, in other words, of its being reducible to the form

$$k_1(x+h_1)^{2n-1} + k_2(x+h_2)^{2n-1} + \dots + k_{n-1}(x+h_{n-1})^{2n-1} + k_n.$$

And so it became natural to establish *à priori* the existence of this condition, and thus to obtain the proof virtually reproduced by Mr. Cayley in the article referred to.

In what precedes, X_{n-1} was a *first principal minor* of X_n ; and it occurred to me to institute an inquiry into the form of the resultant of two functions related to each other as X_{n-1} is to X_n , with the sole but important difference that the constants in X_n are not to be contained in a concatenated order from one line to another, but to be taken perfectly independent as in the example

$$X_3 = \begin{vmatrix} 1 & x & x^2 & x^3 \\ a & b & c & d \\ a' & b' & c' & d' \\ a'' & b'' & c'' & d'' \end{vmatrix}$$

satisfying the identity $U = (Ax + By)V + y^2W$; we have then

$$R(U, V) = R(V, y^2W) = R(V, W) \times (R(V, y))^2,$$

where evidently $R(V, y)$ is the coefficient of x^{n-1} in V ; let y become unity, then on calling $U, V, R(V, y)$ $X_n, X_{n-1}, \lambda_{n-1}$ respectively, and giving to W its corresponding value, we have the theorem as it is used in the text.

* For in general if X_n be the canonizant to F , X_{n-1} will be the canonizant to $\frac{d^2F}{dx^2}$.

and

$$X_4 = \begin{vmatrix} 1 & x & x^2 & x^3 & x^4 \\ a & b & c & d & e \\ a' & b' & c' & d' & e' \\ a'' & b'' & c'' & d'' & e'' \\ a''' & b''' & c''' & d''' & e''' \end{vmatrix}$$

Or, according to a suggestion of Mr. Cayley, putting the question into a more general and simple form, we may inquire into the resultant of any two complete determinants, functions of x of the n th degree, which belong to the rectangular matrix

$$\begin{matrix} 1, & x, & x^2 & \dots & x^{n-1}, \\ a_{1,1}, & a_{1,2}, & a_{1,3} & \dots & a_{1,n}, \\ a_{2,1}, & a_{2,2}, & a_{2,3} & \dots & a_{2,n}, \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ a_{n-1,1}, & a_{n-1,2}, & a_{n-1,3} & \dots & a_{n-1,n}, \\ a_{n,1}, & a_{n,2}, & a_{n,3} & \dots & a_{n,n}; \end{matrix}$$

as, for instance, the resultant of the two determinants which may be obtained by suppressing successively the last and last but one line in the matrix above written: and by aid of the most elementary principles of the calculus of determinants the instructed reader will find no difficulty in proving that this resultant will resolve itself into two distinct parts—one a power of the determinant obtained by suppressing the uppermost (or x) line in the above matrix, the other the Resultant of the matrix obtained by suppressing simultaneously the two lowermost lines*. This last suppression leaves a rectangular matrix which, written in a homogeneous form, becomes

$$\begin{matrix} y^{n-1}, & y^{n-2} \cdot x, & y^{n-3} \cdot x^2 & \dots & x^{n-1}, \\ a_{1,1}, & a_{1,2}, & a_{1,3} & \dots & a_{1,n}, \\ a_{2,1}, & a_{2,2}, & a_{2,3} & \dots & a_{2,n}, \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot \\ a_{n-2,1}, & a_{n-2,2}, & a_{n-2,3} & \dots & a_{n-2,n}, \end{matrix}$$

consisting of n columns and $(n-1)$ lines†.

* For, on making the last-named determinant referred to in the text zero, it may easily be shown, by aid of a familiar theorem in compound determinants, that the two determinants whose resultant is under investigation have all the coefficients of the one in the same ratio to each other as the corresponding coefficients of the other.

† The reader may notice that the real interest of the subject under consideration commences with the independent inquiry into the form of the Resultant of the above matrix—the original question, as to the quasi-canonical, being important only as leading up to the appearance of this Resultant.

The Resultant of this matrix means the quantity R which, equated to zero, will indicate the possibility of the simultaneous nullity of *all* its first minors, so that R will be the factor common to the resultants of every couple of these minors. If we name the columns of the matrix taken in any arbitrary order $C_1, C_2 \dots C_n$, and call R' the resultant of $C_1 C_3 \dots C_{n-1} C_n, C_2, C_3 \dots C_{n-1} C_n$, it may readily be made out that $\frac{R'}{R}$ is equal to a power of the determinant obtained by suppressing the uppermost (or x) line of the rectangular matrix $C_3 \dots C_{n-1} C_n$.

To find R , we may proceed in the general case in the manner indicated in the example following, where $n-1$ is made 4. Taking the two extreme first minors and dividing them respectively by y and x , we have two equations of the following form for determining R , viz.

$$\begin{vmatrix} y^3 & y^2x & yx^2 & x^3 \\ a & b & c & d \\ a' & b' & c' & d' \\ a'' & b'' & c'' & d'' \end{vmatrix} = 0, \quad \begin{vmatrix} y^3 & y^2x & yx^2 & x^3 \\ b & c & d & e \\ b' & c' & d' & e' \\ b'' & c'' & d'' & e'' \end{vmatrix} = 0.$$

By rejecting, as we have done, the factors x and y from the above equations, certain factors, it is true, are lost to their resultant (R'); but it will easily be seen that these factors are each of them powers of one and the same determinant, viz. the determinant

$$\begin{vmatrix} b & c & d \\ b' & c' & d' \\ b'' & c'' & d'' \end{vmatrix}$$

and that their product is contained in the irrelevant factor $\frac{R'}{R}$, itself a power of that determinant, as above explained. To find R , we may write down the oblong matrix

$$\begin{matrix} y^2 & yx & x^2 \\ b & c & d \\ b' & c' & d' \\ b'' & c'' & d'' \end{matrix}$$

and make its three first minors respectively equal to u, v, w , i. e.

$$\begin{vmatrix} y^2 & yx & x^2 \\ b' & c' & d' \\ b'' & c'' & d'' \end{vmatrix} = u, \quad \begin{vmatrix} y^2 & yx & x^2 \\ b'' & c'' & d'' \\ b & c & d \end{vmatrix} = v, \quad \begin{vmatrix} y^2 & yx & x^2 \\ b & c & d \\ b' & c' & d' \end{vmatrix} = w;$$

then we shall obtain the equations following, of which the intermediate ones result solely from the equations last assumed, but the first and last from those combined with the original two

given ones, viz.

$$\begin{aligned}(bu + b'v + b''w)y - (au + a'v + a''w)x &= 0, \\(cu + c'v + c''w)y - (bu + b'v + b''w)x &= 0, \\(du + d'v + d''w)y - (cu + c'v + c''w)x &= 0, \\(eu + e'v + e''w)y - (du + d'v + d''w)x &= 0.\end{aligned}$$

These equations may be satisfied by making simultaneously

$$u=0, \quad v=0, \quad w=0,$$

all of which (since u, v, w are minors of the same rectangular matrix) may exist simultaneously, provided

$$\begin{vmatrix} b & c & d \\ b' & c' & d' \\ b'' & c'' & d'' \end{vmatrix} = 0.$$

Rejecting (as before) this irrelevant factor, it remains to find the resultant of the system of equations in $x, y; u, v, w$, above written, defined as the characteristic of the possibility of their coexistence for some particular system of values of $x, y; u, v, w$, but with joint and *several* exclusion of the system $x=0, y=0$, and of the system $u=0, v=0, w=0$.

So, in like manner, in the general case we shall obtain a similar system of $(m+1)$ homogeneous equations linear in x, y , and also in u_1, u_2, \dots, u_m ; and R will be the resultant of this system, subject to the same condition as to the exclusion of zero systems of x, y , and of u_1, u_2, \dots, u_m as in the particular instance above treated. Such a resultant, as hinted at the outset, is entitled to the name of a double determinant. In general a double determinant will refer to two systems of variables, one p , the other q in number, and to $(p+q-1)$ equations between them.

In the particular instance before us, one of these quantities, say q , is the number 2. There is, moreover, a further particularity (but which as it happens does not at all influence the form of the solution), consisting in the fact that the equations are of the *recurring* form

$$\begin{aligned}L_1 y - L_0 x &= 0, \\L_2 y - L_1 x &= 0, \\L_3 y - L_2 x &= 0, \\&\cdot \quad \cdot \quad \cdot \\L_{p+1} y - L_p x &= 0,\end{aligned}$$

where L_0, L_1, \dots, L_{p+1} are each of them linear homogeneous functions of u_1, u_2, \dots, u_p . This gives rise to an identification of the resultants of two matrices of very different appearance—one

that is to say, it becomes a power of the determinant

$$\begin{vmatrix} a & b & c & d \\ b & c & d & e \\ c & d & e & f \\ d & e & f & g \end{vmatrix}$$

as we know *à priori* it ought to do, by virtue of the theorem originating out of Jacobi's theorem stated at the beginning of this paper: in fact the two factors of the resultant of X_3, X_4 each of them becomes equal to λ_4^3 ; and so in general we shall find, if we use n instead of 4, each factor of the corresponding resultant becomes λ_n^{n-1} , giving λ_n^{2n-2} as the complete resultant for that singular case, as previously determined.

The author is conscious that some apology may appear due for the cursory mode of elucidation pursued in the preceding extended note, and for the absence as regards certain points of the appropriate proofs; but to have gone into all the details of demonstration would have swollen the paper to a length out of proportion to its importance. Let him be permitted also in all humility to add (as can be vouched by more than one contributor to this Magazine), that in consequence of the large arrears of algebraical and arithmetical speculations waiting in his mind their turn to be called into outward existence, he is driven to the alternative of leaving the fruits of his meditations to perish (as has been the fate of too many foregone theories, the still-born progeny of his brain, now for ever resolved back again into the primordial matter of thought), or venturing to produce from time to time such imperfect sketches as the present, calculated to evoke the mental cooperation of his readers, in whom the algebraical instinct has been to some extent developed, rather than to satisfy the strict demands of rigorously systematic exposition.

LXII. *On the Source and Maintenance of the Sun's Heat.*

By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

THE most recent speculations to which the question of the maintenance of the sun's heat has given rise are chiefly these which follow. In a paper contained in the Transactions of the Royal Society of Edinburgh (vol. xxi. part 1. p. 63) and in the Philosophical Magazine for December 1854, Professor William Thomson has concluded that "the source of energy from which solar heat is derived is undoubtedly meteoric." He contends that the waste of heat by radiation can be supplied in no other way than by the heating effect of the impact of small

* Communicated by the Author.

bodies descending in spiral courses to the sun's surface, and supposes that these bodies constitute by their aggregate visibility the phenomenon of the zodiacal light. This theory appears, however, to have been subsequently abandoned for the most part, if we may judge from statements made by Professors Thomson and Tait in the article "On Energy" (in the Number of 'Good Words' for October 1862), which on another account is now attracting the attention of men of science. The authors there admit that an explanation which "supposes the loss by radiation at present not to be compensated by fresh influx of meteoric matter" is probably the true one. According to this explanation, the sun "must be at present in the condition of a heated body cooling. But being certainly liquid for a great depth all round his surface, if not throughout, the superficial parts must sink by becoming heavier as they contract through cooling. The currents thus produced, bringing fresh portions from below to the surface, and keeping all the liquid thoroughly stirred up, must distribute the loss of heat very equably throughout the whole liquid mass, and so prevent the surface from cooling quickly, as it certainly would do if the superficial stratum were solid." This new theory has apparently not satisfied Sir John Herschel, who, in an article "On the Sun" contained in the Number of the same publication for April 1863, has suggested that the permanence of the sun's light and heat might possibly be accounted for by attributing a species of vitality to Nasmyth's 'willow-leaves.'

I will not disguise that I have cited the above views because in my opinion they give evidence of themselves, without the necessity of any argument, that the problem of the sun's heat has not yet been solved, and justify the course I am about to take of attempting the solution of it on entirely different principles. The investigation which I now proceed to enter upon will rest, first, on the hypothetical constitution of matter which I have already so frequently enunciated, viz. that all visible and tangible substances consist of discrete inert atoms of spherical form and constant magnitude; and secondly, on the mathematical theory of heat which I have expounded in the *Philosophical Magazine* for March 1859. This theory assumes that the force of heat is due to the dynamical action of the direct vibrations of the æther, the laws of the vibrations being inferred by mathematical reasoning applied to the medium on hydrodynamical principles, and on the supposition that it is a continuous and perfect fluid varying in pressure proportionally to variations of its density.

Before applying the above principles to the present problem, an instance of the dynamical action of aerial vibrations will be

adduced, which will serve to illustrate the subsequent reasoning. It is a well-known fact that if vibrations are excited at one end of a cylindrical tube open at both ends, by blowing across it, or other means, there will be formed in the tube equidistant positions of minimum velocity and intermediate positions of minimum change of density. These facts are accounted for by supposing that two series of vibrations are propagated along the tube in opposite directions. If the series are exactly alike and equal, the velocity will be zero at the former positions, and at the other there will be no condensation. In actual instances the vibrations are not generally exactly equal when they originate in the manner above mentioned. But for our present purpose we may assume them to be so, taking no account of a slight excess of the vibrations of one set above those of the other. Now the vibrations propagated from the end where the disturbance is made are clearly due to the disturbance itself. But what is the origin of the returning vibrations? The answer to this question is, that the tube is at the other end *relatively* a cause of disturbance by suddenly *ceasing* to confine the vibrations as they reach it. At both ends the condition must be satisfied that the density is permanently very nearly the same as in the surrounding air. At the further end this condition is fulfilled by the relative disturbance producing reflected waves, of which the condensation in that position is just equal and opposite in sign to the condensation due to the direct waves. At the other end the same condition is fulfilled by the condensation due to the reflected waves being equal and opposite in sign to that impressed by the disturbance. Under the circumstances supposed, the motion is evidently alike on the two sides of a plane dividing the tube transversely into two equal parts, and in the position of this plane the velocity is zero. Conceive now the column of fluid in the tube to be divided by an indefinitely thin partition placed in that position. That half of the column which is furthest from the disturbance will then become quiescent, and the vibrations of the other half will remain just as before. Consequently the total movement of the fluid will be only half as much in the case of the closed tube as in that of the open tube, although the original disturbance is the same in the two cases. In fact, there is, in the latter case, an additional *relative* disturbance, due to the rigidity, or constancy of form, of the tube. We have thus been conducted to a remarkable principle, peculiar to the motion of a fluid, viz. that the effect of a disturbance impressed actively may be subsequently increased by a relative disturbance due to a passive rigid body. The above considerations are not merely illustrative of the principle, but amount to a proof that it holds good in aërial vibrations, and by consequence in ætherial vibra-

tions, the æther, according to our hypothesis, being constituted like air of given temperature. But the mode of its application is in some respects different, as we shall presently see, in the theory of the sun's heat.

Reverting now to the mathematical theory of heat above referred to, it is first to be noticed that, according to that theory, the constituent atoms of substances are kept apart by a repulsion due to the dynamical action of vibrations reflected from their spherical surfaces. It is there shown mathematically that such vibrations propagated in the directions of the produced radii of an atom, and impinging on a neighbouring atom, may cause a permanent motion of translation of the latter, repelling it from the other. It is not, however, this effect that we are at present concerned with. We have rather to consider what modifications the impinging undulations are subject to in consequence of the inertia and constant spherical form of the atom on which they are incident. We will suppose that the undulations propagated from the first atom (without at present considering how they may have originated) are alike in all directions from its centre; in which case the velocity V and condensation σ at the time t and at the distance r from the centre are given by the known equations,

$$V = \frac{f'(r - \kappa at + c)}{r} - \frac{f(r - \kappa at + c)}{r^2},$$

$$\kappa a \sigma = \frac{f'(r - \kappa at + c)}{r}.$$

Assuming generally that

$$\frac{f(r - \kappa at + c)}{r^2} = \frac{1}{r^2} \cdot \Sigma \left[m \sin \frac{2\pi}{\lambda} (r - \kappa at + c) \right],$$

we have also

$$\frac{f'(r - \kappa at + c)}{r} = \frac{1}{r^2} \cdot \Sigma \left[\frac{2\pi m r}{\lambda} \cos \frac{2\pi}{\lambda} (r - \kappa at + c) \right].$$

Since it may be inferred from experimental facts that the radius of an atom must be very small compared to the mean interval between adjacent atoms, and also that this mean interval is very small compared to the values of λ for waves of light and heat, the above expressions prove that the first term of the value of V has an extremely small ratio to the other at the surface of the atom from which the waves radiate, and also a very small ratio at the surface of the atom on which they impinge.

This being premised, let us now consider the modifications which the waves defined by the above equations undergo on encountering the adjacent atom, with especial reference to the secondary waves which the presence of the atom gives rise to.

Respecting the condensation represented by σ , we may at once assert that it is neither increased nor diminished by the incidence of the waves on the second atom, on the general principle that condensations and rarefactions once generated are never destroyed, but are dissipated by propagation into unlimited space. They may be diverted, expanded, and contracted; but the total quantity of condensation and rarefaction remains the same.

Next we have to investigate the effect of the incidence of that part of the velocity V which is represented by the second term, and is unaccompanied by condensation. Neglecting, for the sake of simplicity, the motion of the atom, and supposing the velocity of impact at its surface to be W , and the angle of incidence at any point to be θ , the velocity due to the resistance of the surface will be $W \cos \theta$. Now, although the incident velocity was not accompanied by condensation, it is evident that the surface of the atom cannot impress this velocity without producing pressure, and therefore condensation, since action and reaction must be equal. Consequently there will be, apart from the original condensations of the incident waves, *new condensations due to the inertia, and constancy of form, of the atom*. This is a very important result as regards the theory of the internal heat of the sun, or of any other body, on the undulatory hypothesis of heat.

Any vibratory motion which the atom might have, whether caused by extraneous waves, or by the dynamic effect of those under consideration, would produce condensations by impressing the fluid, which would coexist with those due to the atom's reaction, but would not alter the above general inference.

The part of the value of V which is equal to $\kappa\alpha\sigma$, regarded independently of the accompanying condensation, will operate in the same manner as the other part, and will equally give rise to new condensations by the reaction of the atom. These, however, will generally be of much smaller magnitude than the others.

Such secondary undulations at the surface of the second atom as those which we found to result, by the reaction of its surface, from undulations emanating from the first, will also result from undulations emanating from *all* the surrounding atoms. And since we may suppose the atoms to be distributed equally in all directions from any one of them, the secondary undulations of that one will, in the aggregate, be equal at all points of the surface, as was assumed to be the case, without assigning a reason, at an early stage of this argument. Accordingly we are led to the conclusion that the undulations which emanate equally in all directions from each atom, and constitute by their dynamic

action the repulsion of heat, are mainly produced by the reaction of the atoms, due to their inertia and impenetrability. It is plain, however, from this reasoning, that there must be an original and independent source of undulations. Now obviously such a supply may be conceived to be furnished to the sun *by undulations emanating from the stars*. We have ocular evidence that stars transmit light-undulations, and it is quite possible that they originate others not sensible to the sight. Also, according to our preliminary hypothesis respecting the constitution of bodies, the sun must be regarded as atomically constituted throughout. Hence the stellar-undulations, of whatever kind, entering his vast body, have their effects multiplied to an incalculable amount by the reaction of the immense number of atoms. This is the theoretical explanation of the *sun's internal heat*. The continual generation of new undulations both maintains the heat, and supplies the loss resulting from the continual propagation of undulations from the body of the sun. If the external supply be constant, the sun's internal heat must eventually be constant also. For if at any time the loss of undulations by emanation exceeded the supply by internal generation, the heat would decline, and the atoms of the sun being brought closer together by the consequent contraction, the generation of undulations would increase till it became adequate to the supply of the loss. So if at any time the generation of undulations and the internal heat were in excess, the supply would be diminished by the expansion of the mass. Hence the sun's heat will be constant so long as the calorific action of the universe is constant.

In a previous communication entitled a "Theory of Molecular Forces," contained in the Philosophical Magazine for February 1860, I have shown how the waves propagated from the individual atoms of a body constitute by their aggregation compound waves of different orders, the highest of which is the order of gravity-undulations. It is under this form of compound undulations that the above-mentioned loss by emanation takes place.

This theory is not complete unless an explanation be given of the origin of the light-undulations which come from stars. Inasmuch as our sun is a star, the inquiry is the same as that of ascertaining the origin and generation of the sun's light. Solar light is always accompanied by heat—the latter being due to direct vibrations, and the other to transverse. But this heat is to be regarded in its origin and effects as quite distinct from the internal heat above treated of. Observations of the sun's disc seem to indicate that the solar rays which are at once light-bearing and heat-bearing, come chiefly from the upper superficial boundary of a stratum of cloudy matter, which, with occasional interruptions in the form of spots, perpetually surrounds the

body of the sun. The phenomena of the spots show that the stratum is of great thickness. The movements it undergoes justify the assumption that it is in composition *cloud-like*—that is to say, that it consists of a congeries of very minute masses, as terrestrial cloud consists of extremely minute globules of water. Now the flow of undulations from the sun, spoken of in the foregoing explanations, must traverse this stratum of *molecules*; and though the undulations will pass through the molecules, for the same reason that they pass through the substance of the sun, yet in the case of such insulated bodies the passage cannot take place without some degree of disturbance and the generation of secondary undulations. These views being understood, the theory I now propose of the generation of the sun's sensible light and heat is, that it is owing to the collision of the secondary undulations originating in the cloud-stratum with the primary large undulations emanating from the sun's body. On a like principle I account for the zodiacal light by the collision of the same large undulations with steady gyrations about the sun. (See the articles on the Zodiacal Light in the Philosophical Magazine for February and March 1863.) What seems to confirm this explanation is, that the solar light comes principally from the superior boundary of the stratum, up to which limit the supposed collision must be accumulative, and at this limit also there is a sudden break of continuity of the disturbances. The amount of collision between the primary and the resulting secondary waves will depend on the number and combination of the latter, and will clearly be greatest where the boundary of the stratum cuts at right angles the directions of the propagation of the primary waves. According to this view the slanting surfaces of a depression, constituting the penumbra of a spot, are less bright than the general surface, because they are inclined to the directions of the sun's radii, and on this account the resultants of the secondary waves come less directly into collision with the primary waves. For the contrary reason, the faculæ, or bright streaks, are due to crests and elevations of the cloud-matter. On the same principle the general mottled appearance of the sun's face is readily explained—the brighter parts corresponding to the rounded tops of cumuli, and the shaded parts to their slanting sides.

In this manner the theory accounts for the sensible light and heat of the sun; and on the hypothesis that the same operations are going on in the stars as in the sun, the theory at the same time accounts for the sensible light and heat, and for the internal heat, of every body of the universe.

It may be remarked that in giving the above explanations I have not deviated in the least particular from the physical prin-

principles I have so often asserted in this Magazine, nor from the views of the ultimate qualities of matter, and the nature of physical forces, which Newton has left on record. The readiness with which those principles apply in the solution of the difficult problem of the sun's heat, may be taken as evidence of their being founded in truth. If the tendency of the explanations is to show that all the forces of nature are either active pressures of the æther, or passive resistances of atoms, I do not see that this conclusion should not be accepted because it reduces physical forces to such conceptions as can be perfectly reached by experiment and common observation.

Cambridge, May 21, 1863.

LXIII. *Historic Notice of the Mechanical Theory of Heat.*
By M. VERDET*.

THE importance to be attached to this new theory (the dynamical theory of heat) makes it a duty on my part to finish this exposition by a short historic review, in which I shall attempt to render justice to the principal discoverers. This is all the more necessary as I have hitherto constantly followed the logic of the ideas without any regard to the historic order of the discoveries.

Two periods may be distinguished in this history. During one of these, which extends to the year 1842, ideas similar to that embodied in the mechanical theory of heat have been expressed by various authors, while in some cases the same phenomena that this theory explains have been regarded in accordance with other principles, and useful attempts have been made to refer them to general laws. But the true principle not being found, these efforts remained isolated, sterile, and without sensible influence on the general progress of science. All this labour, however, finished by bearing its fruit, and towards the year 1842 the new idea, as is often the case with great discoveries, clearly revealed itself to several minds almost at the same moment. Soon afterwards began that period of rapid progress which always follows the discovery of a true principle, and a few years sufficed to establish the magnificent assemblage of results which I have attempted to lay before you.

The first name on the list of those who may be regarded as the precursors of the mechanical theory of heat is that of the illustrious Daniel Bernoulli. The 'Hydrodynamics' of this great geometer and physicist contained—for a century for-

* Translated from the *Exposé de la Théorie Mécanique de la Chaleur*, p. 109-118.

gotten and neglected by all the world—the theory of the constitution of gases to which I have referred towards the close of our first meeting. His contemporaries saw probably in this theory only a portion of the débris of ancient Cartesian hypotheses; and until quite recently nobody would suppose that in them would be found the germ of a new science.

In 1780, a little more than forty years after the publication of the 'Hydrodynamics,' Lavoisier and Laplace, discussing in their memoir on heat the two hypotheses which could be entertained regarding the nature of this agent, express themselves in the following manner:—

“Other physicists think that heat is only the result of the insensible vibrations of matter. . . . In the system which we are examining heat is the living force which results from the insensible motions of a body, it is the sum of the products of the mass of each molecule by the square of its velocity. . . . We will not decide between the two preceding hypotheses; several phenomena appear favourable to the latter, such, for example, as the heat produced by the friction of two solid bodies; but there are others which are more simply explained by the first hypothesis; perhaps both of them occur at the same time.”

But after this assertion, so clear and so precise, in no part of the memoir do we meet the idea of comparing the living force of heat with the ordinary living force which is sensible in the motion of a body's centre of gravity or in the motion of rotation. Never did Lavoisier and Laplace compare heat except with itself; and hence it mattered little for the fertility of their reasonings, whether they considered heat as an indestructible body, or as a quantity of living force.

More than this, in a subsequent part of their work they regard as evident a proposition which is directly opposed to the principle of the conversion of heat into work. *All variations of heat, they say, whether real or apparent, experienced by a system of bodies, in changing their state, are produced in the inverse order when the system returns to its first condition.* If they had added that this equality occurs only when the changes of condition are accompanied by no exterior work, the mechanical theory of heat would have been founded; but without this complement the assertion of Lavoisier and Laplace is an error, disproved every day by the action of a steam-engine or an electro-magnetic machine.

No one knows the extent to which the views of Lavoisier on this subject might have been modified had he lived. It can only be presumed, from the reading of his treatise on chemistry, that in 1789 he had not entirely abandoned the theory which refers heat to molecular motions. It is true that, yielding per-

haps to the influence of the current opinions of his time, he speaks of gases as if they resulted from the combination of certain bases with *caloric*. But he always showed a reserve, no trace of which is to be found in the writings of his disciples; it was not without scruple that he placed light and caloric at the head of the list of elementary bodies.

As to Laplace, his ideas changed very quickly, and in all that he wrote after the period of his association with Lavoisier he has shown himself the convinced defender of the theory of the materiality of caloric. His imposing authority even produced partisans of that theory long after it had been shown to be without the least foundation.

Towards the end of the eighteenth century, in 1798 and 1799, two experiments were made which were sufficient to demonstrate the inanity of the theory adopted by the author of the *Mécanique Céleste*. These were the celebrated experiments of Rumford and Davy on the heat disengaged by friction. Rumford had measured in a precise manner the heat produced in the boring of a cannon in the royal foundry at Munich; and to leave no doubt as to the origin of this heat, he determined the specific heat of the solid bronze and of the rubbed-off portions of this metal. No sensible difference appeared to exist between the two specific heats. The only reasonable explanation that could be given of the phenomenon according to the material theory of heat was thus peremptorily refuted. It had indeed been admitted that in pulverized bodies the specific heat was much less than in the same bodies in a compact condition, and it would certainly follow from this hypothesis that the pulverization of a body by friction ought to liberate heat. But it was forgotten that friction liberates heat even when there is no alteration of the rubbing surfaces. The incorrectness of the hypothesis was moreover shown by the experiment of Rumford.

The experiment of Davy, later by a year than that of Rumford, was, if possible, still more conclusive. Two pieces of ice rubbed against each other by Davy were rapidly melted, and a liquid was produced the specific heat of which is more than double that of ice. Davy, moreover, had taken all care to prove that the liberation of heat by friction was not compensated by any sensible absorption of heat in any portion of the apparatus.

Among the contemporaries of Rumford and Davy, Young was the only man who comprehended the full bearing of their experiments. In his 'Lectures on Natural Philosophy,' published in 1807, he has connected them with his immortal discoveries on the nature of light, and he almost arrived at the true principle of the mechanical theory of heat. He was the first to call in question the principle, assumed by Lavoisier and Laplace, of

which I have first spoken. *It has not perhaps been demonstrated in a single case*, says Young in his 'Lecture on the Measurement and Nature of Heat,' *that the quantity of heat absorbed in any phenomenon is precisely equal to the heat disengaged when the phenomenon is inverted.* In this simple doubt was virtually contained the whole mechanical theory of heat*.

Unhappily this was an epoch in which the law of double refraction was considered as an argument in favour of the theory of emission; it was the epoch when the most beautiful memoirs of Fresnel remained forgotten, and for years ran the risk of being lost. Thus when in 1824 the original mind of Sadi Carnot, struck by the spectacle of the industrial revolution accomplished by the steam-engine, sought to discover the general laws of the motive power of fire, he did not hesitate for an instant to take as the point of departure for his reasonings the materiality, and consequently the indestructibility of caloric†. I shall perhaps astonish you if I add that, in spite of this fundamental mistake, the name of Sadi Carnot and that of his learned commentator M. Clapeyron, will always occupy an important place in the history of the science. Sadi Carnot is the author of the forms of reasoning employed incessantly in the mechanical theory; it is in his essay that we find the first examples of those cycles of operations which consist in taking a body in a certain state, causing it to pass to a different one by following a certain path, and then causing it to return by a different route to its first condition. M. Clapeyron has cleared up what remained obscure in the memoir of Carnot, and has shown how we may translate analytically, and represent geometrically, this mode of reasoning, so new and so fruitful‡. These two geometers have in a manner created the logic of the science. When the veritable principles had been discovered, nothing was necessary but to introduce them into the forms of that logic; and it may be believed that, without the previous labour of Carnot and Clapeyron, the progress of the new theory would not have been nearly so rapid as it has been.

Finally, I will terminate this first part of my historic exposition by mentioning that M. Séguin, in a memoir published in 1839, and more especially devoted to political economy than to physics, has considered the steam-engine from a point of view which closely resembles the manner in which I attempted in our

* Lectures on Natural Philosophy, vol. i. p. 651 of the edition of 1807. Young admits that the equality of the heat absorbed and the heat liberated is probable; but the simple utterance of a doubt regarding this sort of axiom in 1807 is very worthy of remark.

† *Réflexions sur la puissance motrice du feu, et sur les machines propres à développer cette puissance.* Paris, 1824.

‡ "Mémoire sur la puissance motrice de la chaleur" (*Journal de l'Ecole Polytechnique*, vol. xiv. année 1834). [Translated in Taylor's Scientific Memoirs, Part III.]

first meeting to explain to you the transformation of heat into mechanical power*.

I now come to the labours, from 1842 to 1849, which have definitely established the science. These labours are the exclusive work of three men, who without concert, without even knowing each other, arrived at the same time, and almost in the same manner, at the same ideas. The priority in the order of publication belongs without any doubt to the German physician, Jules Robert Mayer, whose name has occurred so often in these discourses; and it is interesting to know that it was in reflecting on certain observations in his medical practice that he conceived the idea of the necessity of a relation of equivalence between work and heat. The variations of the difference of colour of arterial and venous blood directed his attention to the theory of respiratory phenomena. He soon saw in the respiration of animals the origin of their motive power; and the comparison of animals to thermic machines afterwards suggested to him the important principle with which his name will remain for ever connected. Such is the account which he gives himself of the development of his ideas, in his memoir "On Organic Motion and Nutrition," published in 1845. His first paper, entitled "Remarks on the Forces of Inanimate Nature," published in 1842 in Liebig's *Annalen*, does not, however, contain any allusion to vital phenomena, and deduces simply the equivalence of heat and work from the comparative study of friction, of the steam-engine, and of the properties of gases.

We find moreover in this memoir a first determination of the mechanical equivalent of heat deduced from the properties of gases, perfectly exact in principle, but the result of which, in consequence of the inaccurate values of the coefficient of expansion and the specific heat of air which were current in science twenty years ago, is considerably wide of the truth. The memoir "On Organic Motion and Nutrition," and the "Celestial Dynamics," published in 1848, contain physiological and astronomical applications of the new principle, and show that, notwithstanding a scientific education imperfect on many points, Mayer comprehended the bearing of his discovery, and knew how to follow it up.

Towards the epoch of the first publication of Mayer, M. Colding, Hydraulic Engineer of the city of Copenhagen, presented to the Royal Scientific Society of Denmark a series of memoirs on the power of the steam-engine and gas-engine, which contained ideas almost identical with those of Mayer, and an experimental determination of the mechanical equivalent of heat by friction, which does not appear to be very exact. These titles suffice to assure to M. Colding a place among the discoverers of the new theory. But we ought to remember that the

* *Etude sur l'influence des chemins de fer*, p. 380.

various memoirs of this physicist, written in a language the knowledge of which is but little extended, and first printed several years after their presentation to the Academy of Copenhagen, have exerted scarcely any influence on the subsequent developments of the science.

The third discoverer of whom it rests with me to speak, Mr. Joule, is the one who perhaps has done most for the demonstration of the new principle, and for its final adoption. His first investigation, published only in 1843, is incontestably posterior by some months to the first publications of Mayer and Colding. It contains experiments on the heat developed by induced currents, and does not appear to have excited much attention. It is to his experiments in 1845 on the calorific effects of the dilatation and compression of gases that belongs the privilege of giving the right of citizenship in science to the new ideas*. It is his experiments on friction that have given the first determination of the mechanical equivalent of heat which is worthy of confidence. It is his views on the constitution of gases which first gave the only example, up to the present time, of a complete explanation of a phenomenon of which the theory might predict the laws, but could not indicate the mechanism.

Immediately after these three names, that of M. Helmholtz ought to appear, for having in 1847, in his memoir on the Conservation of Force, united in a body the doctrine of the new ideas, and for having made fruitful and important applications of them to the phenomena of induction, to electro-chemistry, and to thermo-electric currents.

Lastly, the final constitution of the science, the clear and methodical establishment of the proper processes of investigation and of reasoning, as also its application in detail to the theory of machines, are principally due to the efforts of three authors, which are the last that I will cite—MM. Clausius, Macquorn Rankine, and William Thomson. Their most important researches have been published between 1849 and 1851.

Since the epoch last referred to, there have been many other researches conducted under the inspiration of the same ideas. I have had occasion to refer to several of them during the course of these two lectures. Others are mentioned in the table of mechanical equivalents which I have placed before you. I will not attempt to complete these indications. I will content myself with having shown you how the foundations of that edifice have been laid in the construction of which during the last ten years everybody has taken a part.

* This is a difficult passage. The original runs thus:—"C'est à ses expériences de 1845, sur les effets calorifiques de la dilatation et de la compression des gaz, qu'il appartenait de donner droit de cité dans la science aux idées nouvelles."

LXIV. *Notices respecting New Books.*

A Dictionary of Chemistry and the Allied Branches of other Sciences.
By HENRY WATTS, B.A., F.C.S. Parts I. and II. London:
Longmans, 1863.

WE have here the first two Parts of a work which, if completed upon the same scale and in the same style of excellence as it has been begun, will be one of the most important, in respect both of comprehensiveness and of real scientific value, that the chemical literature of our language possesses. We fear, however, from the announcement that it is to be completed in sixteen Parts, that the limits assigned to the work are such as will make it impossible for that fulness of detail which is one of the chief merits of the portion already published to be maintained throughout. This portion, consisting altogether of 384 pages, carries the work as far as "Arsenic"; so that a considerable share of Part III. will evidently still be needed to make room for all the articles which come under the first letter of the alphabet. With more than an eighth of the total space assigned to the work thus occupied by the letter A, it is easy to foresee that the subjects falling under the remaining letters must be treated very much less fully, or else that the book must be extended considerably beyond the limits at present contemplated. A new edition of the first part (comprising the letters A to E) of the great German Dictionary of Chemistry, commenced twenty-six years ago by Liebig, Poggendorff, and Wöhler, was begun in 1856 and finished last year. Here the first five letters of the alphabet extend over more than 4500 pages, and the letter A alone over 1720 pages, while in the first edition, which is at last approaching the other end of the alphabet, we find the letter S occupying upwards of 1400 pages. Of course the plan of Mr. Watts's Dictionary is altogether less comprehensive than that of the German one; yet, on examining the Parts before us, we find that the difference in the amount of really useful information contained in the two works is much less than the mere consideration of the size of each would have led us to expect. Mr. Watts has very judiciously economized space by the total omission of articles of small importance, instead of curtailing those devoted to more important subjects. In some instances, indeed, we find more information in his work than in the corresponding articles of its much more bulky German contemporary. The adoption of a close but clear type, and of a style that is condensed without being awkward, have also tended to increase still further the amount of matter which could be compressed into a small space. But, making every allowance for the saving thus effected, we cannot think that 4000 pages, instead of the 3000 which sixteen Parts such as the two already issued would provide, would be found to afford anything more than the space absolutely necessary for the completion of the work in a satisfactory manner. We have said so much on this point because we feel that it would be a serious loss to British science if the utility of a work like the present were to be

sacrificed through the desire to compress it within limits that are obviously too narrow.

Probably the first point that will strike the attention of every reader of this work, is the fact that the scale of atomic weights adopted in it is the so-called "unitary" scale proposed twenty years ago by Gerhardt. It is an important indication of the direction in which chemical opinion has been moving of late years, that Mr. Watts—a chemist of far too much experience and too sound judgment to have been influenced by the mere charms of novelty—should have found it desirable to adopt Gerhardt's notation, and the general system of ideas of which that notation is the exponent, in a work which is in no way designed to advocate any particular set of theoretical views, but simply to set forth in as clear and direct a manner as possible the results of chemical investigation. Whatever objections some chemists may still urge against this system, it is a fact, which can no longer be overlooked, that the school by which it has been adopted has acquired a degree of importance which makes it necessary for every one engaged in chemical pursuits to be acquainted with it; and hence a work, such as the present, in which this system is consistently carried out and applied to all chemical compounds, acquires a value that is quite independent of any opinion as to the truth or falsehood of the system itself. For the benefit, however, of persons who are likely to consult the Dictionary for the sake of practical rather than of scientific information, the old formulæ, as well as the new, are given in all articles to which they may be expected to refer.

Although by far the greater portion of the two Parts now before us is the work of Mr. Watts himself, several very valuable articles have been contributed by other writers, who seem to have well seconded the editor in endeavouring to bring down to the most recent date possible the information on the various subjects treated by them.

One specially valuable characteristic of the work is the fulness with which analytical processes of all kinds are described. Besides an excellent series of articles devoted to the various branches of analytical chemistry (Inorganic Analysis, by Mr. F. T. Conington; Organic Analysis, elementary and proximate, by the Editor; Volumetric Analysis of Liquids and Solids, by Mr. Dittmar; and Volumetric Analysis of Gases, by Dr. Russell), and those on Acidimetry, Alkalimetry, and Alcoholometry, all of which are very complete, we find under the heading of each of the more important substances—such as Acetic Acid, Ammonia, Antimony, Arsenic—a detailed account of the processes to be adopted for its detection and quantitative estimation.

In a work of such magnitude, occasional errors in detail must be looked for as a matter of course; but almost all those that we have observed are either obviously misprints, or have probably arisen from the length of time which the work has necessarily occupied in going through the press. To this latter cause is also doubtless due the occasional omission of any mention of very recent discoveries. For instance, in the article *ALKALI*, we find nothing about cæsium or

rubidium, and under AMIDES we miss a good many newly discovered "polyammonias." At page 214, however, a paragraph, apparently inserted at the last moment, refers us to LIGHT for a description of the methods of spectrum-analysis; so that we may hope that other opportunities will be found for supplying, in the course of the work, all similar omissions.

In conclusion, we have only to say that Mr. Watts is producing a work of which the value will be at once apparent to every chemist, and one which is admirably adapted for use as a book of reference by those who, though not constantly occupied with chemical pursuits, nevertheless require from time to time to consult some trustworthy and nearly complete authority on particular points; while, from the clearness of the style and the fulness of information on many subjects that are hardly treated at all in the common text-books, many parts of his Dictionary may be read with great advantage by every chemical student who has already mastered the first rudiments of his science.

Dual Arithmetic, a New Art. Invented and developed by OLIVER BYRNE. London: Bell and Daldy, 1863.

No greater service can be rendered to mathematical science than the facilitation of its ultimate reduction to numerical measurement. Notwithstanding all that has been done to ease calculation, still the actual work of arithmetic remains the great incubus. All practical mathematics resolve themselves ultimately into numbers, and, sooner or later, every practical mathematician has to face the arithmetical labour; yet there still remains, as a reproach to science, the difficulty of saying whether this ultimate reduction is most feared by those who, to avoid it, take refuge in analysis or in geometry, or by those to whom a superior energy or a different taste gives a mastery over its laborious detail.

We receive with a satisfaction, altogether independent of its success, any attempt like the present to place arithmetic on a more compact basis.

Mr. Byrne has done himself a great injustice by neglecting to place prominently before his reader a distinct and logical statement of the intention and objects of his method, and of the means by which he attains them. We conceive that we shall be doing a service to him, as well as to our readers, by endeavouring partially to supply this defect. We say *partially*, because we feel that our perusal of a work, the completion of which is avowedly reserved for future treatises, can but imperfectly replace the clear and symmetric idea which exists in the author's mind, and of which, as we must be permitted to say, it is his part to present a clear and symmetric statement to the public. If we fail in reproducing his meaning with exactness, the blame must fall on him, for presenting us with a chaos of rules, examples, and notes, without any logical guide beyond a table of contents.

Retaining the ordinary decimal system of the Arabic numeration, Mr. Byrne superadds the expression of the whole or a portion of a

number by means of the powers of 1·1, 1·01, 1·001, &c. entering as factors; separating these from the ordinary numerals by an arrow \downarrow , and from each other by commas. Thus he expresses by

$$32 \downarrow 2, 1, 3, 4$$

what we might write at full length in the ordinary notation of powers as

$$32 \cdot (1\cdot1)^2 \cdot (1\cdot01) \cdot (1\cdot001)^3 \cdot (1\cdot0001)^4;$$

or, when expanded, as

$$39\cdot25036 \ 52125 \ 50568 \ 92246 \ 17678 \ 7172;$$

and the expression in his notation

$$4 \ 0, 2$$

means

$$4 \cdot (1\cdot01)^2 \text{ or } 4\cdot0804.$$

He expresses by $4 \downarrow 0, \bar{2}$ the fraction

$$4 \cdot (1\cdot01)^{-2} \text{ or } \frac{4}{1\cdot0201}.$$

It will be obvious that the product or sum of any two numbers so represented is expressible at sight, without further labour than the multiplication or division of the numbers lying to the left of the symbol \downarrow , and the addition or subtraction of the numbers to its right. Thus the product of

$$16 \downarrow 4, 3, 0, 1, 9 \text{ and } 8 \downarrow 1, \bar{2}, 9, 7, 2$$

is

$$128 \downarrow 5, 1, 9, 8, 11,$$

and their quotient is

$$2 \downarrow 3, \bar{5}, \bar{9}, \bar{6}, 7,$$

or its reciprocal,

$$\frac{1}{2} \downarrow \bar{3}, \bar{5}, 9, 6, 7;$$

the numbers to the right of the arrow being, in fact, indices.

We must refer to the treatise itself for the mode of expressing ordinary numbers in a notation of this kind, and of comparing different expressions of the same number, whether absolute or approximate. It is obvious that every number may be expressed in an infinite variety of ways under this notation. For instance, 10·01 may be expressed indifferently as

$$10\cdot01, 10 \downarrow 0, 1, \text{ or } 9\cdot1 \downarrow 1.$$

Whether this indeterminate character of the notation be an advantage or a hindrance, we have not sufficient experience to decide.

The notation just explained appears to be only the preliminary,

both in detail and in theory, to another, which we will explain by an example taken from p. 207:

$$\begin{aligned}
 7276 \cdot 68024 &= 3 \downarrow 3, 3, 3, 0, 0, 0, 0, 747541336 \\
 &= \downarrow 0, 0, 0, 0, 0, 0, 0, 889287691 \\
 &= \downarrow \sqrt[8]{889287691}.
 \end{aligned}$$

Similarly,

$$\downarrow 0, 0, 5 \text{ is } \downarrow \sqrt[3]{5}$$

and

$$\downarrow 0, 0, 0, 317 \text{ is } \downarrow \sqrt[4]{317}.$$

In practice, this index, which may be always taken *ad libitum*, is a fixed number. Mr. Byrne chooses for this the number 8 and suppresses the index. The result is that the expression $X = \overline{x}$ or $\sqrt[8]{x}$ means nothing more than that x is the logarithm of X to the base $1 \cdot 00000001$, or $1 + \overline{10}^{-8}$, and that its use as such is the key to the whole work done.

We confess to considerable disappointment at discovering this to be the "resultant" of the method. It is quite certain that arithmetic is not to be revolutionized or even materially simplified by such means as this; and we are driven to the conclusion that there is more merit in the attempt than value in the result. After a perusal made with sufficient care to discover a few typographical errors in a carefully printed book, we find ourselves obliged to state that all Mr. Byrne's arithmetical skill and ingenuity do not enable him to solve his problems with as little work as a good selection of ordinary methods would require. We rise from our perusal far more convinced of the power of the author than of his method.

Mr. Byrne concludes his introduction as follows:—

"In my works on Algebra and the Calculus, which are being prepared for publication, the whole subject and its different applications will be treated in a general and exhaustive manner. My work on the Calculus, to be termed the 'Calculus of Form,' unfolds a new science, and establishes modern analysis on a purely mathematical basis, rejecting the reasoning of the differential and other methods."

We do not quite understand what reasoning it is that Mr. Byrne proposes to reject, or why. But we gather that he entertains some objection to the existing systems, probably thinking them illogical, complex, and prolix. Now we are not bigoted to them. We are painfully aware of their deficiencies in the last two respects, and we are not without some misgivings on the first head. But we may be forgiven for reminding Mr. Byrne that a proposal of change is as disagreeable as a dose of physic, and requires even more tact in its administration. If he expects to gain the ear of mathematicians, he must do what he has not done in his 'Dual Arithmetic'—give a clear and succinct statement of his plan, his principle, and his results, so that the educated reader may see the value of the prize which is

to be his reward for the toil of wading through the mass of formulæ and computative details necessary to a mastery of the subject presented to him. A chaos of introductory examples, set forth in an unexplained notation, is the least alluring commencement which could possibly be given to a scientific treatise.

We trust that we shall not have this fault to find with the forthcoming works. At any rate there is far too much of originality and power in the work before us, to leave us in doubt whether its sequel will not amply repay our careful study.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 317.]

June 19, 1862.—Major-General Sabine, President, in the Chair.

The following communications were read:—

“On the Reflexion of Polarized Light from Polished Surfaces.”
By the Rev. Samuel Haughton, M.A., F.R.S., &c.

When a plane-polarized beam of light is incident on a polished surface at a certain angle of incidence, and polarized in a certain azimuth, the reflected beam of light is circularly polarized.

The tangent of this angle of incidence is called by the author the Coefficient of Refraction, and upon it appears to depend the *brilliancy* of a polished surface.

The cotangent of the azimuth of incident polarization is called the Coefficient of Reflexion, and upon it appears to depend the rich *lustre*, strikingly exhibited in polished copper and gold.

The paper contains an account of the experiments made to determine, with precision, these constants for the following substances:—

A. *Transparent Bodies.*

- | | |
|-------------------------------|-----------------------|
| 1. Munich glass (<i>a</i>). | 4. Fluor-spar. |
| 2. Munich glass (<i>b</i>). | 5. Glass of antimony. |
| 3. Paris glass. | 6. Quartz crystal. |

B. *Pure Metals.*

- | | |
|---------------|---------------------|
| 1. Silver. | 7. Zinc. |
| 2. Gold. | 8. Lead. |
| 3. Mercury. | 9. Bismuth. |
| 4. Platinum. | 10. Tin. |
| 5. Palladium. | 11. Iron and steel. |
| 6. Copper. | 12. Aluminium. |

C. *Alloys.*

- | | |
|-------------------------------------|-------------------------------|
| 1. Copper and tin (speculum metal). | 9. Copper and zinc (3 Cu+Zn). |
| 2. Copper and zinc (10 Cu+Zn). | 10. " " (2 Cu+Zn). |
| 3. " " (9 Cu+Zn). | 11. " " (Cu+Zn). |
| 4. " " (8 Cu+Zn). | 12. " " (Cu+2 Zn). |
| 5. " " (7 Cu+Zn). | 13. " " (Cu+3 Zn). |
| 6. " " (6 Cu+Zn). | 14. " " (Cu+4 Zn). |
| 7. " " (5 Cu+Zn). | 15. " " (Cu+5 Zn). |
| 8. " " (4 Cu+Zn). | |

The determination of the optical constants of these substances leads to many interesting conclusions; among which the following may be stated:—

1. That transparent bodies, as well as metals, possess a coefficient of reflexion, which is sometimes very sensible, although there are bodies in which it is very small.

2. That *Silver* is the only substance which possesses the qualities of *brilliancy* and *lustre*, represented by the coefficients of refraction and reflexion, in a high degree.

3. Of the metals which have high *brilliancy* and little *lustre* may be named *Mercury*, *Palladium*, *Zinc*, and *Iron*.

4. Of the metals which have high *lustre* and little *brilliancy* there are only two, *Gold* and *Copper*.

5. Results of the highest interest appear from an examination of the optical constants of the alloys of copper and zinc, which cannot be given in an abstract.

6. In the details of the several experiments, the author calls attention to several remarkable laws, or indications of laws, which appear to him to require some notice from theorists.

a. When the azimuth of the incident beam is less than the circular limit, the axis major of the reflected ellipse, at the principal incidence, lies in the plane of incidence; but when the azimuth is greater than the circular limit, it is perpendicular to the plane of incidence, and as the incidence varies, the axis major twice approaches to a minimum distance from that plane.

b. There appears to the author to be some indication in the experiments on metals, that the quantity known to theorists as $\left(\frac{J}{I}\right)$ is, not a function of the incidence only; a conclusion which, if correct, would require the intervention of a third wave suppressed, or some such theoretical supposition, to account for it.

“On the Properties of Electro-deposited Antimony.” By George Gore, Esq.

In this communication the author has described two additional kinds of electro-deposited antimony possessing the property of evolving heat; one of them is obtained from a solution of bromide, and the other from a solution of iodide of antimony; there is also given additional information respecting the peculiar heating-antimony obtained from the aqueous terchloride.

The following is a brief statement and comparison of some of the properties of the three kinds of thermically active antimony. The specific gravity of the chloride deposit is 5.8, the bromide one 5.44, and the one from the iodide 5.25. The amount of heat evolved is greatest with the one from the chloride solution, and least with that from the iodide; the former evolves all its heat at 60° Fahr., by contact with a red-hot wire, the bromide one at 280° Fahr., whilst the iodide one requires a temperature of 340° Fahr.; the latter also acquires a reddish-brown colour by exposure to solar light.

The chloride deposit contains about 6.3 per cent. of saline matter,

the bromide one about 20, and the one from the iodide liquid about 22·2. The quantities deposited by a single equivalent of zinc were about 42·5 in the chloride, 50 in the bromide, and 51 in the iodide solution.

The explanation proposed of their formation is, that the antimony in depositing, being in the "nascent" state, combines chemically in a feeble manner with the saline ingredients of the electrolyte; but the complete sources of the evolved heat remain undecided.

"On the Sulphur-Compounds in Purified Coal-Gas, and on Crystallized Hydrosulphocarbonate of Lime." By the Rev. W. R. Bowditch, B.A., F.C.S., Wakefield.

"On the Geometrical Isomorphism of Crystals." By the Rev. W. Mitchell.

"On the Forces concerned in producing the larger Magnetic Disturbances." By Balfour Stewart, M.A., F.R.S.

The author begins by alluding to a previous communication made to the Royal Society, containing an account of the great magnetic storm of August 28–September 7, 1859, in which he had shown that the first effect of this great disturbance was to diminish in intensity both components of the earth's magnetic force at Kew, during a period of about six hours. Such an effect, he argues, can scarcely be supposed due to any combination of earth-currents, of which the period is only a few minutes.

But another appearance is noticeable on the photographic curves which regard the progress of this great disturbance.

While the great wave of force had a period of about six hours, there were superimposed upon it smaller disturbances having a period of a few minutes, and therefore comparable in this respect with earth-currents.

These smaller disturbances are of very frequent occurrence, and show themselves in the Kew magnetograph curves as serrated appearances, occasionally magnified into peaks and hollows.

Two hypotheses may be entertained regarding them.

1st. They may be conceived to represent small and rapid changes in the intensity of the whole disturbing force which acts upon the magnet; and since (as stated above) this force cannot be supposed due to earth-currents, so neither can its variations be caused by these.

2nd. The peaks and hollows may be supposed due to the direct action of earth-currents upon the magnets.

The following argument is advanced to show that the second of these hypotheses is untenable.

Let us compare together the two magnetic disturbances of August and September 1859 and August 1860; and suppose the peaks and hollows of the disturbance curves of these dates to be caused by earth-currents. This would require that currents of the same name should have simultaneously travelled between Margate and Ramsgate, and between Ramsgate and Ashford during the latter disturbance, whereas during the former these currents should have been of different names, that is to say, the one positive and the other negative.

According to Mr. Walker's observations, however, on both these occasions a current between Margate and Ramsgate was simultaneous with one of the same name between Ramsgate and Ashford.

Thus, if we adopt the second hypothesis, it would appear that these lines ought to have been affected differently on these two occasions, whereas by observation they were affected in the same manner; the conclusion is that this hypothesis does not represent the truth.

The author then shows that earth-currents observed simultaneously with a very abrupt disturbance which commenced about 11^h 17^m A.M., September 1, 1859, would lead us to infer that the former are induced currents due to sudden and rapid changes in the magnetism of the earth.

Referring now to the first hypothesis, which asserts that the peaks and hollows represent small and rapid changes in the intensity of the whole disturbing force which acts upon the magnet, it would follow that these peaks and hollows should in this case comport themselves with regard to the three elements of the earth's magnetism in the same way as the whole disturbing force of which they represent the changes. Thus, if the tendency of the great body of the disturbing force is to raise the curves for the three elements simultaneously, then a small peak in one element should correspond to a peak, and not to a hollow, in the other two. But if, on the other hand, the tendency of the disturbing force is to raise one of the curves and lower the other two, then a peak in the first should correspond to a hollow in the others.

This is shown to be the case in the disturbances extending from the beginning of 1858 to the end of 1860; and the author therefore concludes that peaks and hollows represent small and rapid changes in the intensity of the whole disturbing force which acts upon the magnet.

It is then shown that use may be made of these peaks and hollows, if we wish to analyse the forces concerned in producing disturbances. Let us suppose that several independent forces are concerned. It is very unlikely that a small and rapid change takes place at the same instant in more than one of these. The measurement therefore of simultaneous abrupt changes for the three elements may enable us to determine the character of one of the elementary disturbing forces at work.

It is not even necessary to confine ourselves to very rapid changes, provided we take peaks or hollows which present a similar appearance for all the elements, as such can only be produced by the action of a single force.

The author then shows that a peak of the horizontal force always corresponds to a peak of the vertical force, and not to a hollow, and that, when similar peaks are compared together, the horizontal-force peak is always as nearly as possible double in size that of the vertical force.

This curious fact would imply that the resolved portion of the disturbing force which acts in the plane of the magnetic meridian is

always in nearly the same direction. The dip of this resolved portion will be about $17\frac{1}{2}^{\circ}$.

It is also found that a declination peak corresponds to a peak of either force, except in the case of the great disturbance of August to September 1859, during the most violent portion of which a peak of the declination corresponded to a hollow in either force. The length, however, of a declination peak does not bear an invariable ratio to that of a force peak—this ratio varying much from one disturbance to another, but not much from one part to another of the same disturbance. In this last case, however, the variation of the ratio, although not great, is yet greater than that of the ratio between the two force peaks; so that it is somewhat difficult to obtain similar peaks when comparing the declination curve with that of either force.

It thus appears that the force which acts upon the magnets does not vary much from one part to another of the same disturbance, and it therefore becomes possible to give the elements of this force, which will thus characterize the disturbance.

The author then attempts, by means of comparing similar appearances, to represent the force at work for each disturbance between the beginning of 1858 to the end of 1860. The great disturbance of August to September 1859 is here remarkable as one in which two independent disturbing forces seem to have acted at once,—one of these being of the normal type, in which all the elements were raised or depressed together, while in the other the declination was raised when both elements of the force were depressed.

It will be observed that this method of analysis does not completely determine the disturbing force, but merely fixes the line of its resultant action, along which the force itself may be either positive or negative; or, again, there may be two nearly opposite forces acting against one another, the visible disturbance denoting merely the difference in strength between the two; and there is some reason to think that this last supposition represents the true state of the case.

For while the definite relation which exists between the peaks of the two force-components shows that all disturbing forces affect these in nearly the same way, yet sometimes, though very rarely, in the general progress of the curve one of the elements will be above the normal while the other is below it. Now, this may be accounted for in the following manner. Suppose we have a disturbance producing an elevation in the horizontal force represented by +40, and one in the vertical force represented by +20. This will be of the normal type. Suppose now that at the same time we have another force nearly similar, whose action on the two force-elements is represented by -39 -21. This is also sufficiently near the normal type. The result of these two disturbances superimposed will be +1 and -1, showing that the one element is raised above its normal position, while the other is depressed below it. This idea of two opposite forces acting simultaneously in disturbances is that entertained by General Sabine from other considerations.

“Experimental Researches on the Transmission of Electric Signals through Submarine Cables.”—Part I. Laws of Transmission through various lengths of one Cable. By Fleeming Jenkin, Esq.

Professor W. Thomson has in various papers stated and developed the mathematical theory of the transmission of signals through long submarine cables. The present paper contains an experimental research into the same subject. The conclusions arrived at by theory are confirmed by the experiments, and some new facts of considerable importance are established.

All the observations in this part of the paper were made on the Red Sea cable, when coiled in iron tanks at Birkenhead.

By observation on a reflecting galvanometer, an arrival-curve was obtained for various lengths of cable with various arrangements of battery. By arrival-curve is meant the curve representing the gradual rise of the current at the remote end of the cable when the near end is put in permanent connexion with the battery.

The analysis of the various arrival-curves led to the following conclusions:—

1. “The electromotive force has no appreciable effect on the velocity with which the current is transmitted.

2. “The rate of decrease in the current at the remote end, after contact has been made for a given time with earth at the near end, is the same as the rate of increase observed after making contact with the battery at the near end for an equal time.”

With reference to the use of alternate positive and negative currents as compared with alternate connexion with the positive or negative pole of a battery and earth,

3. It was found that the “reversals in no way modified the arrival-curve during its increase, nor did they modify the curve showing the decrease of the current.”

The effect of ordinary morse signals was next observed on the galvanometer through various lengths of cable.

The changes in the received current, caused by repeated dots, by repeated dashes, by dots and dashes alternately, and by dots and dashes separated by a pause, were observed at different speeds. Repeated dots, when represented graphically, give an even wavy line with large amplitudes of oscillation for slow speeds or through short lengths, but rapidly approaching a straight line as the speed of transmission or the length of the cable was augmented.

If the maximum permanent deflection caused by the battery be called 100, dots sent at the rate of 15 per minute through 2192 knots of cable caused oscillations in the received current of 12·7 per cent.; and sent at the rate of 50 per minute, this caused an oscillation of less than 1 per cent.

4. From this it was concluded that “on all submarine cables there is a limit to the number of signals which can be sent per minute, a limit which cannot be exceeded by any ingenious contrivance.”

If we continue to call the maximum deflection due to permanent contact 100, the mean height of the current observed during dots is

below 50, on account of time lost between the two contacts while moving the sending-key.

When dashes or lines are sent, *i. e.* long contacts with the battery followed by short earth-contacts, an even wavy line is obtained, the mean height of which is above 50; and when dots and dashes are combined, the curves representing the changes of the current become very irregular, sometimes flying above 50, sometimes falling below this line; and when long pauses, or a succession of long battery-contacts are introduced, the curves become hopelessly confused, especially at the higher speeds, so that the signals cannot be disentangled, even when the change of current can be continually followed. From this it is concluded that,

5. "There is a wide margin between the limit set to the speed of transmission by the gradual diminution of the received signals, and that set by their interference."

Reverse currents have been recommended as a means of accelerating the rate of speaking through submarine cables. Their effect was tested; the arrival-curves and signal-curves obtained by their use differed in no way from those obtained by simple currents and earth-contacts. Hence it was concluded that,

6. "The use of reverse currents does not alter the limit set by the gradual diminution of the received signals, nor that set by their interference."

It occurred to the author that, if by any means the current could invariably after each signal be brought to one constant strength and maintained at that strength between the signals, the confusion of interference would be avoided. He considered that, if the second or earth-contact of each signal bore a fit proportion to the first contact, this object might be effected; and he considered that a succession of very short pairs of contacts of a certain relative length, would maintain the current at the constant final strength during any pause separating signals. He therefore prepared a paper band with openings cut so as to make pairs of equal battery- and earth-contacts for dots, long battery-contacts, followed by nearly equal earth-contacts, for a dash, and a succession of pairs of very short contacts wherever a pause was required, the battery contacts being rather the shorter of the two.

The success of this plan was such that the signals were distinctly recorded, not only by the galvanometer, but by a relay when the total variations caused by the shortest signals were invisible on the galvanometer, *i. e.* even less than 1 per cent. of the maximum final current.

7. Hence it was concluded that by the means adopted, or by analogous means, "signals can be sent without confusion at any speed which will allow the shortest signal used to cause a sensible variation in the received current."

These experiments were tried on dry cable coiled in iron tanks, and might therefore not be applicable to extended and submerged cables.

The author has, however, proved that the retardation and insulation of an iron-covered cable are very little affected by the mere presence or absence of water; and wherever the conclusions obtained from the experiments agree with the deductions of theory, it is clear

that the experiments and theory confirm one another, and the conclusions may be safely applied to the practical case of a submerged and extended cable; for it is impossible to suppose that results due only to an accidental arrangement of the cable should by chance coincide with the deductions from a defective hypothesis.

The experimental arrival-curves do not exactly agree with the curve given by Professor W. Thomson (Proceedings of the Royal Society, 1855. Phil. Mag. 1856).

The experimental curve approaches its maximum much more slowly than the mathematical curve, and continues to rise 1 or 2 per cent. long after all effects from retardation as given by theory would cease.

Some of this effect may be due to the mutual influence of the coils of the cable*; but the greater part of the discrepancy is due to the change of the insulation due to continued electrification, first published by the author in a paper read before the Royal Society in 1859-60†.

The identity of the arrival-curve during increase and decrease shows that,

8. "The apparent increase of resistance of the gutta percha is rather due to an absorption of electricity which is again given out, than to a real change in the conductivity of the material."

The theoretical and practical conclusions on the effect of repeated signals were next examined. Little change of insulation could take place during the repeated signals, because the greater part of the cable remained continually electrified; and greater coincidence between the experiments and the theory was therefore to be expected.

The curve expressing the rate at which the amplitude of oscillation in the received current diminishes as the number of signals increases, was constructed from Professor W. Thomson's equations‡; and the experimental amplitudes with 1500, 1802, and 2192 knots of cable in circuit, were found to coincide in the most accurate manner with this curve—establishing completely the soundness of the mathematical theory.

9. These results prove beyond all question that "the rate of transmission varies as the square of the length, whether by rate of transmission be meant that speed at which repeated signals fail to produce any sensible effect, or the rate producing so great an amplitude that common hand-signals can be received without confusion."

It is also found (when small compared with the total resistance) that,

10. "The resistance of the battery and receiving instrument produces nearly the same effect as the addition of an equal length of submarine cable."

If the amplitude of oscillation in the received current caused by dots at any one speed through any one straight cable were known,

* *Vide* paper read by Professor W. Thomson at the British Association, Aberdeen, 1859. Also paper by Professor W. Thomson and F. Jenkin, Phil. Mag. 1861; also a letter from Mr. F. C. Webb in 'The Engineer,' August 1859.

† Published in full in Appendix to the Report of the Committee of the Board of Trade on the Construction of Submarine Cables.

‡ *Vide* Proceedings of the Royal Society, 1855. Phil. Mag. 1856.

the amplitude through any other cable at any other speed could immediately be taken from the curve, now verified by experiment.

Unfortunately this one fact is wanting. The author hopes to be able to supply the want in the second part of this paper.

“On the Thermal Effects of Fluids in Motion.”—Part IV. By J. P. Joule, LL.D., F.R.S., and Professor W. Thomson, F.R.S.

A brief notice of some of the experiments contained in this paper has already appeared in the ‘Proceedings.’ Their object was to ascertain with accuracy the lowering of temperature, in atmospheric air and other gases, which takes place on passing them through a porous plug from a state of high to one of low pressure. Various pressures were employed, with the result (indicated by the authors in their Part II.) that the thermal effect is approximately proportional to the difference of pressure on the two sides of the plug. The experiments were also tried at various temperatures, ranging from 5° to 98° Cent., and have shown that the thermal effect, if one of cooling, is approximately proportional to the inverse square of the absolute temperature. Thus, for example, the refrigeration at the freezing temperature is about twice that at 100° Cent. In the case of hydrogen, the reverse phenomenon of a rise of temperature on passing through the plug was observed, the rise being doubled in quantity when the temperature of the gas was raised to 100°. This result is conformable with the experiments of Regnault, who found that hydrogen, unlike other gases, has its elasticity increased more rapidly than in the inverse ratio of the volume. The authors have also made numerous experiments with mixtures of gases, the remarkable result being that the thermal effect (cooling) of the compound gas is less than it would be if the gases after mixture retained in integrity the physical characters they possessed while in a pure state.

“On the Spectra of Electric Light, as modified by the Nature of the Electrodes and the Media of Discharge.” By the Rev. T. R. Robinson, D.D., F.R.S.

The author, after referring briefly to the researches of previous inquirers, and the hypothesis now generally adopted, that the bright lines observed in these spectra depend so absolutely on the chemical nature of the substances present that their occurrence is an unerring test of that presence, expresses his belief that it cannot be admitted in its full extent without much more decisive proof than has yet been afforded. It assumes,

1. That each substance has a set of lines peculiar to itself.
2. That those lines are not produced or modified by any molecular agent except heat.
3. That the spectrum of one substance is in nowise modified by the presence of another; and in such cases both spectra coexist independently, and are merely superposed.
4. As may be inferred from 2, that electricity does not make matter luminous directly, but only by heating it; so that the electric spectrum differs in nothing from that produced by heat of sufficient intensity.

His attention was directed to this subject several years ago by the difference of colour of discharges in carbonic oxide at common and diminished pressure; and the results of his experiments appear to show that none of these four points is universally true.

His apparatus consisted of a powerful induction machine, with which a Leyden jar was connected; of prisms, first one of 45° , afterwards one of 60° (whose deviations were reduced to the scale of the first); and of an optical theodolite, in which a collimator with a variable slit gives the beam whose spectrum is observed. He points out an important defect of this arrangement, and discusses the probable liabilities to error proceeding from the graduation reading only to minutes, and from other sources of uncertainty.

The media of discharge were air, nitrogen, oxygen, hydrogen, and carbonic oxide, to which were added in some instances the vapours of mercury, phosphorus, and bisulphuret of carbon. For electrodes, 23 metals and graphite were used—15 with each of the five gases at common pressure and at one of $0\cdot2$, the others only with some of them. In all, 185 *different* spectra were measured, of which 93 were at common pressure.

At common pressure the spectra show a number of bright lines on a coloured ground, the light of which is in general stronger towards the red than the violet end, and strongest in the green. In some this ground is so bright as to efface all but the most luminous lines: this is especially the case with hydrogen. Of the lines, some are very brilliant; but they range in light down to the very lowest degree of faintness, such that (at least with the author's apparatus) they can only be seen when the room is entirely dark, and are bisected with great difficulty. They vary also in width, from a mere hair's breadth to six or seven times the apparent width of the slit.

On exhausting the tube in which the discharge is made, at first the only change is that the brilliant lines lose a little of their lustre, till at pressures varying from 3° to $0\cdot5$ the spectrum rather suddenly fades away, sometimes leaving only a suspicion of one or two lines; with others the least-refrangible rays vanish, while the violet remain, though very faint, especially with aluminium. In hydrogen spectra the three bright bands of this gas vanish at unequal densities; and it is remarkable that this occurs when the gas is diluted to the same proportions by mixing air with it.

Exhausting yet further, this transition spectrum becomes again bright; fresh lines appear, and it is changed into a new one, which, however, is never as splendid as that at common pressure, especially at the red end, and in which the very brilliant lines are less frequent. This want makes the difference between the two kinds of spectra seem greater than it really is. Fewer lines are visible in the rarefied media, and of these about four-tenths are not found in the spectra of common pressure.

If the tables in which the measures are given be examined in reference to the points alluded to as doubtful, it will be obvious,

1. That many lines are found in *all the gases*, and in *many, perhaps all the metals*: the existence of such lines must be independent of

the chemical nature of electrodes or media ; it is otherwise with their brightness, which may be intense with one substance and feeble with another. This unchemical origin is still more clearly shown by a modified experiment of Plücker, where the discharge is made by the induction of glass without the presence of any metal. When *the same* glass vessel was filled in succession with nitrogen, oxygen, and hydrogen, though not above twenty-three lines were seen in its capillary tube, and those very faint, yet more than half the number were common to two of the gases, or to the three. These might perhaps be referred to soda or lead detached from the glass ; but some of them are not found in those spectra.

2. The difference between the common-pressure, transition, and rarefied spectrum shows that the character and even the existence of certain lines depend on the mere density of the medium, the chemical circumstances remaining unchanged.

3. That the spectra are not merely superposed without change is evident from several facts. The spectra of air do not in every case show all the lines of oxygen and nitrogen, and occasionally have some not visible in either of them : the spectrum of graphite in oxygen is quite different from those of carbonic oxide. There is even reason to believe that for certain lines the actions of bodies may be antagonistic. The spectrum of mercury electrodes and mercury vapour showed 48 lines, and the author expected that the spectra for any gas with mercury electrodes would add to those of mercury the peculiar lines of that gas, which could thus be certainly determined. In the nitrogen spectrum, however, 20 of the mercurial lines had disappeared, in the hydrogen 18, and in the carbonic oxide 13.

4. The brilliancy or visibility of the lines is very little increased by greatly augmenting the heating power of the discharge. The two halves of the induction machine can be made to act either consecutively for tension, or collaterally for quantity. In the latter case the quantity is doubled, and therefore the heating power quadrupled. When the apparatus is so used, the violet bands are something brighter, but not so much so as to be noticed by an unpractised observer. The red and green show no appreciable difference ; but the author is inclined to think the change may be greater in the ultra-violet part. He proposes, however, to repeat the experiment with coils of much greater power as to quantity. If electricity can produce thermic vibrations by its transmission, there seems no *à priori* reason why it cannot produce luminous ones ; and no evidence that it cannot is known to him.

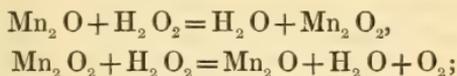
It seems to follow from these observations that the tendency to show such lines belongs to matter in general, but that different forms of it have different powers of manifesting that tendency, and that those powers may sometimes interfere. If this be confirmed by further research, the result will be that, though the *electric* spectrum may give useful indications to the analyst, it should never be his sole dependence, or be trusted without full cognizance of the conditions which may affect its indications.

“On Fermat’s Theorem of the Polygonal Numbers.”—Second Communication. By the Right Hon. Sir Frederick Pollock, F.R.S., Lord Chief Baron.

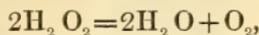
“On the Oxidation and Disoxidation effected by the Alkaline Peroxides.” By B. C. Brodie, Esq., F.R.S.

A preliminary notice containing an abstract of the greater portion of this paper has already appeared*.

Having shown that the alkaline peroxides are capable of acting either as agents of oxidation or reduction, the author proceeds to connect the double function of this class of peroxides with the peculiar catalytic decompositions which they undergo. It is shown that the catalytic decomposition may be regarded as a combination of these two actions, an oxidation and a reduction simultaneously occurring. Thus in an alkaline solution of the peroxide of hydrogen, protoxide of manganese is oxidized to peroxide. In the acid solution the peroxide of manganese is reduced to protoxide, the results being expressed in the following equations:—



while the result of the catalytic decomposition effected by the peroxide of manganese is given in the equation derived from the above by elimination,



the result being the same as though the peroxide of manganese were alternately reduced and oxidized by the alkaline peroxide. We are thus enabled to analyse the catalytic action into its constituent decompositions.

That in numerous cases the catalytic change is brought about by the intervention of intermediate compounds, which are alternately formed and destroyed during the action, is shown in various examples. For instance, the addition of a solution of peroxide of sodium to an excess of a solution of a protosalt of copper causes the formation of a precipitate of a yellow peroxide of copper. If, on the other hand, a few drops of the salt of copper be added to an excess of the alkaline peroxide, the same yellow body is formed, but the whole of the peroxide is ultimately decomposed; after the decomposition hydrated protoxide of copper remains. Similar phenomena occur with an ammoniacal solution of the copper-salt. If a few drops of this solution be added to an ammoniacal solution of the peroxide of hydrogen, the solution becomes of a yellow colour, and the catalytic action is set up. This action may continue, in dilute solutions, for several hours; during the whole of this time the yellow colour is permanent; but ultimately, when the peroxide is entirely decomposed, the blue colour of the ammoniacal solution of the protoxide reappears. The ammoniacal solution of the protoxide of copper

* Phil. Mag. S. 4. vol. xxiv. p. 392.

decomposes the peroxide of hydrogen into water and oxygen, precisely as sulphuric acid decomposes alcohol into ether and water. But in this case the colour of the solution gives actual evidence of the presence of the intermediate compound by the agency of which the catalytic action is effected, and which is formed, but disappears from the final result.

LXVI. *Intelligence and Miscellaneous Articles.*

ON THE VELOCITY OF THE PROPAGATION OF SOUND IN GASEOUS BODIES. BY J. STEFAN.

IT appears not to have been noticed that the new theory of gases admits of the application of Newton's formula for the velocity of sound, if a regular arrangement of the molecules be assumed, such as Krönig has employed in his important work on the theory of heat.

Let us suppose an infinitely extended space filled with a gas to be divided by parallel planes into very thin layers. When condensation takes place in one layer, it will pass on from layer to layer with a velocity which is the velocity of the propagation of sound. The new theory of gases, according to which the molecules are in very rapid progressive motion and behave like elastic balls when they strike one another, requires that the velocity of the progression of sound should be dependent upon the velocity of the progressive motion of the molecules. These two velocities would be exactly equal if the movements of the molecules were all in the direction in which the propagation of sound took place.

Following Krönig, let us divide the whole space filled with gas into equal cubes, all of them placed in the same direction. Let us suppose in each of these cubes three molecules, each one moving perpendicularly between two opposite sides of the cube. Let us assume that each molecule only strikes against molecules which are moving in the same line and which are situated in the neighbouring cubes. These hypotheses relative to the constitution of gases are sufficient, as Krönig has shown, to explain the most important properties of gases. But in order to make the explanation simple and natural, it is necessary in each particular case to represent the division of the space into elementary cubes in a definite manner. If we, for example, in the case before us, arrange the cubes so that their two opposite sides lie parallel with the surfaces of the layers into which we have divided our gas, it follows that only one third of the molecules take part in the transmission of *vis viva* from layer to layer, the third, namely, which is composed of those molecules moving perpendicularly to the surfaces of the layers. In order that all the molecules may play the same part in this transmission, it is necessary so to place the elementary cubes that the diagonal drawn through two opposite angles of the cube shall be perpendicular to the surfaces of the layers. The velocity with which the *vis viva* or the condensation

from layer to layer moves is then equal to the velocity of a molecule estimated in the direction of the diagonal. The length of this diagonal is $\sqrt{3}$, when 1 equals the length of a side of the cube. The cosine of the angle which is formed by the diagonal and the side of the cube equals therefore $\frac{1}{\sqrt{3}}$. Let u be the velocity of a molecule, then the component of the same, estimated from the normals, is

$$x = \frac{u}{\sqrt{3}}.$$

Let p be the pressure of the gas against the unit of surface, v a certain volume, n the number of molecules in the same, and m the magnitude of a molecule, then, according to the theory developed by Krönig and Clausius, we have the relation

$$p v = \frac{n m u^2}{3}.$$

Putting

$$\frac{n m}{v} = \rho,$$

where ρ means the specific gravity of the gas, we have

$$\frac{u^2}{3} = \frac{p}{\rho},$$

and accordingly

$$x = \sqrt{\frac{p}{\rho}},$$

which is Newton's formula for the velocity of the propagation of sound.

A more complete theory of the propagation of sound, grounded upon the new notions about the construction of gases, requires moreover that Laplace's correction should be applied to the above formula. In order to arrive at this, it is necessary to enter into the irregularities in the movements of the molecules, and perhaps also to consider the circumstance that, as Clausius has shown, over and above the progressive motions of the molecules, motions must take place in the molecules themselves.

The above disposition of the elementary cubes is also applicable to the theory of the conduction of heat, when the object in view is only to obtain an approximative value of the conducting-power.—Poggendorff's *Annalen*, No. 3, 1863.

Vienna, 13 March, 1863.

ON THE ORIGIN OF AMYGDALOID ROCKS.

BY DR. TSCHERMAK*.

The general opinion in regard to these rocks is that they are of igneous origin,—in fact, that they are a sort of lava whose vesicular

* Communicated by Count Marschall.

cavities have been filled with various substances. But, lately, M. Volger has asserted that amygdaloids are merely metamorphosed conglomerates of sedimentary origin. Indeed a closer examination of these rocks shows the existence of three distinct varieties of them. In the first, and by far the most common of these varieties, decomposition has begun with the crystallized minerals imbedded in the rock, changing them into amorphous nodules, and at last leaving only the cavities in which they were included. The second variety, of far scarcer occurrence, owes its origin to the filling up of pre-existing cavities. Those of the third variety, in conformity with M. Volger's views, are really metamorphosed conglomerates including geodes, which have originally been pebbles rolled by water. Metamorphism and pseudomorphism act within geodes exactly in the same way as on minerals included in veins or in solid rocks. Amygdaloids are consequently only a particular stage in those changes which rocks are continually undergoing.—*Imperial Academy of Sciences of Vienna*, Meeting of February 12, 1863.

ON THE MOTION OF CAMPHOR TOWARDS THE LIGHT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I have never had the slightest intention of misrepresenting Dr. Draper, or of detracting from the merit due to his industry and skill. But I deny that the paper which he published in your Magazine in 1840, or the volume which he published in 1844, contains the true theory of the motion of camphor towards the light, or that I ever admitted that it did so. The former contains a question which, had it been followed out and properly tested by experiment, would have led to the true theory; but that it was not thus tested is proved by the fact that in the volume of 1844 it was again published as a question and nothing more.

I now leave my claims and those of Dr. Draper to be settled by persons who may feel interest enough in the subject to read my papers and his volume. I think they will find that the latter, full as it is of details of ingenious experiments, leaves one as much in doubt at the end as at the commencement, and cannot possibly be considered as the earlier and later steps of a successful inquiry. Indeed it is so difficult to trace any consecutive steps that the volume was viewed by me as a whole, and as containing the latest revised opinions of the author.

I remain, &c.,

King's College, London,
May 25, 1863.

C. TOMLINSON.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

SUPPLEMENT to VOL. XXV. FOURTH SERIES.

LXVII. *Remarks on the Mechanical Equivalent of Heat.*

By DR. J. R. MAYER*.

THE vast and magnificent structure of the experimental sciences has been erected on only a few pillars.

History teaches us that the searching spirit of man required thousands of years for the discovery of the fundamental principles of the sciences, on which the superstructure was then raised in a comparatively short time.

But these very fundamental propositions are nevertheless so clear and simple, that the discovery of them reminds us, in more than one respect, of Columbus's egg.

But if, now that we are at last in possession of the truth, we speak of a method by the application of which the most essential fundamental laws might have been discovered without waste of time, it is not that we would criticise in any light spirit the efforts and achievements of our forerunners: it is merely with the object of laying before the reader in an advantageous form one of the additions to our knowledge which recent times have brought forth.

The most important—not to say the only—rule for the genuine investigation of nature is, to remain firm in the conviction that the problem before us is to learn to *know* phenomena, before seeking for explanations or inquiring after higher causes. As soon as a fact is once known in all its relations, it is therein explained, and the problem of science is at an end.

Notwithstanding that some may pronounce this a trite assertion, and no matter how many arguments others may bring to oppose it, it remains none the less certain that this primary rule has been too often disregarded even up to the most modern times; while all the speculative operations of even the most highly gifted minds which, instead of taking firm hold of facts

* Translated by G. C. Foster, B.A., from a pamphlet intended for popular circulation, published in 1851 under the title *Bemerkungen über das mechanische Aequivalent der Wärme*. Von J. R. Mayer. Heilbronn und Leipzig, 1851.

as such, have striven to rise above them, have as yet borne but barren fruit.

We shall not here discuss the modern naturalistic philosophy (*Naturphilosophie*) further than to say that its character is already sufficiently apparent from the ephemeral existence of its offspring. But even the greatest and most meritorious of the naturalists of antiquity, in order to explain, for example, the properties of the lever, took refuge in the assertion that a circle is such a marvellous thing that no wonder if motions, taking place in a circle, offer also in their turn most unusual phenomena. If Aristotle, instead of straining his extraordinary powers in meditations upon the fixed point and advancing line, as he calls the circle, had investigated the numerical relations subsisting between the length of the arm of the lever and the pressure exerted, he would have laid the foundation of an important part of human knowledge.

Such mistakes, committed as they were, in accordance with the spirit of those times, even by a man whose many positive services constitute his everlasting memorial, may serve to point us in the opposite road which leads us surely to the goal. But if, even by the most correct method of investigation, nothing can be attained without toil and industry, the cause is to be sought in that divine order of the world according to which man is made to labour. But it is certain that already immeasurably more means and more toil have been sacrificed to error than were needed for the discovery of the truth.

The rule which must be followed, in order to lay the foundations of a knowledge of nature in the shortest conceivable time, may be comprised in a few words. The natural phenomena with which we come into most immediate contact, and which are of most frequent occurrence, must be subjected to a careful examination by means of the organs of sense, and this examination must be continued until it results in quantitative determinations which admit of being expressed by *numbers*.

These numbers are the required foundations of an exact investigation of nature.

Among all natural operations, the free fall of a weight is the most frequent, the simplest, and—witness Newton's apple—at the same time the most important. When this process is analysed in the way that has been mentioned, we immediately see that the weight strikes against the ground the harder the greater the height from which it has fallen; and the problem now consists in the determination of the quantitative relations subsisting between the height from which the weight falls, the time occupied by it in its descent, and its final velocity, and in expressing these relations by *definite numbers*.

In carrying out this experimental investigation, various difficulties have to be contended with; but these must and can be overcome; and then the truth is arrived at, that for every body a fall of 16 feet, or a time of descent of one second, corresponds to a final velocity of 32 feet per second.

A second phenomenon of daily occurrence, which is in apparent contradiction to the laws of falling bodies, is the ascent of liquids in tubes by suction. Here, again, the rule applies, not to allow the maxim, *velle rerum cognoscere causas*, to lead us into error through useless and therefore harmful speculations concerning the qualities of the vacuum, and the like; on the contrary, we must again examine the phenomenon with attention and awakened senses; and then we find, as soon as we put a tube to the mouth to raise a liquid, that the operation is at first quite easy, but that afterwards it requires an amount of exertion which rapidly increases as the column of liquid becomes higher. Is there, perchance, an ascertainable limit to the action of suction? As soon as we once begin to experiment in this direction, it can no longer escape us that there is a barometric height, and that it attains to about 30 inches. This number is a second chief pillar in the edifice of human knowledge.

Question now follows question, and answer, answer. We have learned that the pressure exerted by a column of fluid is proportional to its height and to the specific gravity of the fluid; we have thus determined the specific gravity of the atmosphere, and by this investigation we are led to carry up our measuring-instrument, the barometer, from the plain to the mountains, and to express numerically the effect produced by elevation above the sea-level upon the height of the mercury-column. Such experiments suggest the question, Whether the laws of falling bodies, with which we have become acquainted at the surface of the earth, do not likewise undergo modification at greater distances from the ground. And if, as *à priori* we cannot but expect, this should be really the case, the further question arises, In what manner is the number already found modified by distance from the earth? We have thus come upon a problem the solution of which is attended with many difficulties; for what has now to be accomplished, is to make observations and carry out measurements in places where no human foot can tread. History, however, teaches that the same man who put the question was also able to furnish the answer. Truly he could do so only through a rich treasure of astronomical knowledge. But how is this knowledge to be attained by us?

Astronomy is, without question, even in its first principles, the most difficult of all sciences. We have here to deal with objects and spaces which forbid all thought of experiment,

while at the same time the motions of the innumerable heavenly bodies are of so complicated a kind, that astronomical science, in its stately unfolding, is rightly considered the highest triumph whereof human intellect here below is able to boast.

In accordance with the natural rule that, both in particulars and in general, man has to begin with that which is easiest and then to advance step by step to what is more difficult, it might well be supposed that astronomy must have arrived at a flourishing state of development later than any other branch of human knowledge. But it is well known that in reality the direct opposite was the case, inasmuch as it was precisely in astronomy, and in no other branch, that the earliest peoples attained to really sound knowledge. It may, indeed, be asserted that the science of the heavenly bodies had in antiquity reached as high a degree of perfection as the complete want of all the auxiliary sciences rendered possible.

This early occurrence of a vigorous development of astronomy, which, indeed, was a necessary forerunner of the other sciences, since it alone furnished the necessary data for the measurement of time, is observable among the most various races of mankind: the reason of it, moreover, lies in the nature of things, and in the constitution of the human mind. It furnishes a remarkable proof that a right method is the most important condition for the successful prosecution of scientific inquiry.

The explanation of this phenomenon lies in the fact that the need which was felt at a very early period, of a common standard for the computation of time, made it necessary to institute observations such that their results required to be expressed by definite *numbers*. There was a felt necessity of determining the time in which the sun accomplishes his circuit through the heavens, as well as the time in which the moon goes through her phases, and other similar questions. In order to meet this necessity, there was no temptation to take up the Book of Nature, after the manner of expositors and critics, merely to cover it with glosses:

“Mit eitler Rede wird hier nichts geschafft.”

It was *numbers* that were sought, and *numbers* that were found. The overpowering force of circumstances constrained the spirit of inquiry into the right path, and therein led it at once from success to success.

Now that after long-continued, accurate, and fortunate observations the needful knowledge of the courses and distances of the nearest heavenly bodies, as well as of the figure and size of the earth, has been acquired, we are in a position to treat the question, What is the numerical influence exerted by increased distance from the earth upon the known laws of falling bodies?

and we thus arrive at the pregnant discovery that, at a height equal to the earth's semidiameter, the distance fallen through and the final velocity, for the first second, is four times less than on the surface of the earth.

In order to pursue our inquiry, let us now return to the objects which immediately surround us. From the earliest times, the phenomena of combustion must have claimed in an especial degree the attention of mankind. In order to *explain* them, the ancients, in accordance with the method of their naturalistic philosophy, put forward a peculiar upward-striving element of Fire, which in conjunction with, and in opposition to, Air, Water, and Earth, constituted all that existed. The necessary consequence of this theory, which they discussed with the most acute sagacity, was, that in regard to the phenomena in question and all that related to them, they remained in complete ignorance.

Here, again, it is quantitative determinations, it is numbers alone, which put the Ariadne's clue in our hand. If we want to know what goes on during the phenomena of combustion, we must *weigh* the substances before and after they are burned; and here the knowledge we have already acquired of the weight of gaseous bodies comes to our aid. We then find that, in every case of combustion, substances which previously existed in a separate state enter into an intimate union with each other, and that the total weight of the substances remains the same both before and after the combination. We thus come to know the different bodies in their separate and in their combined states, and learn how to transform them from one of these states into the other; we learn, for instance, that water is composed of two kinds of air which combine with each other in the proportion of 1 : 8. An entrance into chemical science is thus opened to us, and the numerical laws which regulate the combinations of matter (*die Stöchiometrie*) hang like ripe fruit before us.

As we proceed further in our investigations, we find that in all chemical operations—combinations as well as decompositions—changes of temperature occur, which, according to the varying circumstances of different cases, are of all degrees of intensity, from the most violent heat downwards. We have measured quantitatively the heat developed, or counted the number of heat-units, and have so come into possession of the law of the evolution of heat in chemical processes.

We have long known, however, that in innumerable cases heat makes its appearance where no chemical action is going on; for instance, whenever there is friction, when unelastic bodies strike one another, and when aëriiform bodies are compressed.

What then takes place when heat is evolved in such ways as these?

We are taught by history that in this case also the most sagacious hypotheses concerning the state and nature of a peculiar "matter" of heat, concerning a "thermal æther," whether at rest or in a state of vibration, concerning "thermal atoms," supposed to exercise their functions in the interstices between the material atoms, or other hypotheses of like nature, have not availed to solve the problem. It is, notwithstanding, of no less wonderfully simple a nature than the laws of the lever, about which the founder of the peripatetic philosophy cudgelled his brains in vain.

After what has gone before, the reader cannot be in any doubt about what is the course now to be pursued. We must again make quantitative determinations: we must measure and count.

If we proceed in this direction and measure the quantity of heat developed by mechanical agency, as well as the amount of force used up in producing it, and compare these quantities with each other, we at once find that they stand to each other in the simplest conceivable relation—that is to say, in an invariable direct proportion, and that the proportion also holds when, inversely, mechanical force is again produced by the aid of heat.

Putting these facts into brief and plain language, we may say, *Heat and motion are transformable one into the other.*

We cannot and ought not, however, to let this suffice us. We require to know *how much* mechanical force is needed for the production of a given amount of heat, and conversely. In other words, the law of the invariable quantitative relation between motion and heat must be expressed *numerically*.

When we appeal hereupon to experiment, we find that raising the temperature of a given weight of water 1 degree of the Centigrade scale corresponds to the elevation of an equal weight to the height of about 1200 [French] feet*.

This number is THE MECHANICAL EQUIVALENT OF HEAT.

The production of heat by friction and other mechanical operations is a fundamental fact of such constant occurrence, that the importance of its establishment on a scientific basis will be recognized by naturalists without any preliminary enumeration of its useful applications; and, for the same reason, a few historical remarks touching the circumstances attending the discovery of the foregoing fundamental law, will not be out of place here.

In the summer of 1840, on the occasion of bleeding Europeans newly arrived in Java, I made the observation that the

* [Applying the corrected specific heat of air, M. Regnault finds the equivalent according to the method of Mayer to be 426 kilogram-metres. Mr. Joule's equivalent is 425.—G. C. F.]

blood drawn from the vein of the arm possessed, almost without exception, a surprisingly bright red colour.

This phenomenon riveted my earnest attention. Starting from Lavoisier's theory, according to which animal heat is the result of a process of combustion, I regarded the twofold change of colour which the blood undergoes in the capillaries as a sensible sign—as the visible indication—of an oxidation going on in the blood. In order that the human body may be kept at a uniform temperature, the *development* of heat within it must bear a quantitative relation to the heat *which it loses*—a relation, that is, to the temperature of the surrounding medium*; and hence both the production of heat and the process of oxidation, as well as the *difference in colour of the two kinds of blood*, must be on the whole less in the torrid zones than in colder regions.

In accordance with this theory, and having regard to the known physiological facts which bear upon the question, the blood must be regarded as a fermenting liquid undergoing slow combustion, whose most important function—that is, sustaining the process of combustion—is fulfilled without the constituents of the blood (with the exception, that is, of the products of decomposition) leaving the cavities of the blood-vessels or coming into such relation with the organs that an interchange of matter can take place. This may be thus stated in other words: by far the greater part of the assimilated food is burned in the cavities of the blood-vessels themselves, for the purpose of producing a physical effect, and a comparatively small quantity only serves the less important end of ultimately entering the substance of the organs themselves, so as to occasion growth and the renewal of the worn-out solid parts.

If hence it follows that a general balance must be struck in the organism between receipts and expenditure, or between work done and wear and tear, it is unmistakeably one of the most important problems with which the physiologist has to deal, to make himself as thoroughly acquainted as it is possible for him to be with the budget of the object of his examination. The wear and tear consists in the amount of matter consumed; the work done is the evolution of heat. This latter effect, however, is of two kinds, inasmuch as the animal body evolves heat on the one hand directly in its own interior, and distributes it by communication to the objects immediately surrounding it; while, on the other hand, it possesses, through its organs of motion, the power of producing heat mechanically by friction or in similar ways, even at distant points. We now require to know

* Compare also on this head the interesting publication of Bergmann, *Ueber die Verhältnisse der Wärme-Oekonomie der Thiere zu ihrer Grösse*. Göttingen, 1848.

Whether the heat directly evolved is ALONE to be laid to the account of the process of combustion, or whether it is the SUM of the heat evolved both directly and indirectly that is to be taken into calculation.

This is a question that touches the very foundations of science ; and unless it receives a trustworthy answer, the healthy development of the doctrine concerned is not possible. For it has been already shown, by various examples, what are the consequences of neglecting primary quantitative determinations. No wit of man is able to furnish a substitute for what nature offers.

The physiological theory of combustion starts from the fundamental proposition, that the quantity of heat which results from the combustion of a given substance is *invariable*—that is, that its amount is *uninfluenced* by the circumstances which accompany the combustion ; whence we infer, “*in specie*,” that the chemical effect of combustible matter can undergo no alteration in amount even by the vital process, or that the living organism, with all its riddles and marvels, cannot create heat out of nothing.

But if we hold firm to this physiological axiom, the answer to the question started above is already given. For, unless we wish to attribute again to the organism the power of creating heat which has just been denied to it, it cannot be assumed that the heat which it produces can ever amount to more than the chemical action which takes place. On the combustion-theory there is, then, no alternative, short of sacrificing the theory itself, but to admit that the *total* amount of heat evolved by the organism, partly directly, and partly indirectly by mechanical action, corresponds quantitatively or is equal to the amount of combustion.

Hence it follows, no less inevitably, *that the heat produced mechanically by the organism must bear an invariable quantitative relation to the work expended in producing it.*

For if, according to the varying construction of the mechanical arrangements which serve for the development of the heat, the same amount of work, and hence the same amount of organic combustion, could produce *varying* quantities of heat, the quantity of heat produced from one and the same expenditure of material would come out smaller at one time and larger at another, which is contrary to our assumption. Further, inasmuch as there is no difference in kind between the mechanical performances of the animal body and those of other inorganic sources of work, it follows that

AN INVARIABLE QUANTITATIVE RELATION BETWEEN HEAT AND WORK IS A POSTULATE OF THE PHYSIOLOGICAL THEORY OF COMBUSTION.

While following in general the direction indicated, it was accordingly needful for me in the end to fix my attention chiefly on the physical connexion subsisting between motion and heat; and it was thus impossible for the existence of the mechanical equivalent of heat to remain hidden from me. But, although I have to thank an accident for this discovery, it is none the less my own, and I do not hesitate to assert my right of priority.

In order to ensure what had been thus discovered against casualties, I put together the most important points in a short paper which I sent in the spring of 1842 to Liebig, with a request that he would insert it in the *Annalen der Chemie und Pharmacie*, in the forty-second volume of which, page 233, it may be found under the title "Bemerkungen über die Kräfte der unbelebten Natur"*.

It was a fortunate circumstance for me that the reception given to my unpretending work by this man, gifted with so deep an insight, at once secured for it an entrance into one of the first scientific organs, and I seize this opportunity of publicly testifying to the great naturalist my gratitude and my esteem.

Liebig himself, however, had about the same time already pointed out, in more general but still unmistakable terms, the connexion subsisting between heat and work. In particular, he asserts that the heat produced mechanically by a steam-engine is to be attributed solely to the effect of combustion, which can never receive any increase through the fact of its producing mechanical effects, and, through these, again developing heat.

From these, and from similar expressions of other scientific men, we may infer that science has recently entered upon a direction in which the existence of the mechanical equivalent of heat could not in any case have remained longer unperceived.

In the paper to which reference has been made, the natural law with which we are now concerned is referred back to a few fundamental conceptions of the human mind. The proposition that a magnitude, which does not spring from nothing, cannot be annihilated, is so simple and clear that no valid argument can be urged against its truth, any more than against an axiom of geometry; and until the contrary is proved by some fact established beyond a doubt, we may accept it as true.

Now we are taught by experience, that neither motion nor heat ever takes its rise except at the expense of some measurable object, and that in innumerable cases motion disappears without anything except heat making its appearance. The axiom

[* A translation of this paper appeared in the *Philosophical Magazine* for November last (see vol. xxiv. p. 371). In this translation are embodied the corrections of several misprints in the original, given by the author in a foot-note inserted at this point of the present paper.—G. C. F.]

that we have established leads, then, now to the conclusion that the motion that disappears becomes heat, or, in other words, that both objects bear to each other an invariable quantitative relation. The proof of this conclusion by the method of experiment, the establishment of it in all its details, the tracing of a complete harmony subsisting between the laws of thought and the objective world, is the most interesting, but at the same time the most comprehensive problem that it is possible to find. What I, with feeble powers and without any external support or encouragement, have effected in this direction is truly little enough; but—*ultra posse nemo obligatus*.

In the paper referred to (vol. xxiv. p. 375) I have thus expressed myself with regard to the genetic connexion of heat and moving force:—

“If it be now considered as established that in many cases (*exceptio confirmat regulam*) no other effect of motion can be traced except heat, and that no other cause than motion can be found for the heat that is produced, we prefer the assumption that heat proceeds from motion, to the assumption of a cause without effect and of an effect without a cause,—just as the chemist, instead of allowing oxygen and hydrogen to disappear without further investigation, and water to be produced in some inexplicable manner, establishes a connexion between oxygen and hydrogen on the one hand and water on the other.”

From this point there is but *one* step to be made to the goal. At page 376 it is said: “The solution of the equations subsisting between falling-force [that is, the raising of weight] and motion requires that the space fallen through in a given time, *e. g.* the first second, should be experimentally determined; in like manner, the solution of the equations subsisting between falling-force and motion on the one hand and heat on the other, requires an answer to the question, How great is the quantity of heat which corresponds to a given quantity of motion or falling-force? For instance, we must ascertain how high a given weight requires to be raised above the ground in order that its falling-force may be equivalent to the raising of the temperature of an equal weight of water from 0° to 1° C. The attempt to show that such an equation is the expression of a physical truth may be regarded as the substance of the foregoing remarks.

“By applying the principles that have been set forth to the relations subsisting between the temperature and the volume of gases, we find that the sinking of a mercury column by which a gas is compressed is equivalent to the quantity of heat set free by the compression; and hence it follows, the ratio between the capacity for heat of air under constant pressure and its capacity under constant volume being taken as = 1.421, that the warm-

ing of a given weight of water from 0° to 1° C. corresponds to the fall of an equal weight from the height of about 365 metres."

It is plain that the expression "equivalent" is here used in quite a different sense from what it bears in chemistry. The difference will be shown most distinctly by an example. When the same weight of potash is neutralized, first, with sulphuric acid, then with nitric acid, the numbers which express the ratio which the absolute weights of these three substances bear to one another are called their equivalents; but there is no thought here either of the quantitative equality or of the transformation of the bodies in question.

This peculiar signification which the word "equivalent" has acquired in chemistry, is doubtless connected with the fact that the chemist has been able to determine the object of his investigation by a common quantitative standard, their absolute weights. Let us suppose, however, that we could determine one body, for instance water, only by weight, and another, water-forming or explosive gas, only by volume, and that we had agreed to choose 1 lb. as the unit of weight, and 1 cubic foot as the unit of volume; we should then have to ascertain how many cubic feet of explosive gas could be obtained from one pound of water, and conversely. This number, without which neither the formation nor the decomposition of water could be made the subject of calculation, might then be suitably called "the explosive-gas equivalent of water."

In this latter sense a raised weight might, in accordance with the known laws of mechanics, be called the "equivalent" of the motion resulting from its fall. Now, in order to compare these two objects, the raised and the moving weight, which admit of no common measure, we require that constant number which is generally denoted by g . This number, however, and the mechanical equivalent of heat, whereby the relation subsisting between heat and motion is defined, belong both of them to one and the same category of ideas.

In the paper that I have mentioned it is further shown how we may arrive at such a conception of force as admits of being consistently followed to its consequences and is scientifically tenable; and the importance of this subject induces me to return to it again here.

The word "force" (*Kraft*) is used in the higher or scientific mechanics in two distinct senses.

I. On the one hand, it denotes every push or pull, every effort of an inert body to change its state of rest or of motion; and this effort, when it is considered alone and apart from the result produced, is called "pushing force," "pulling force," or shortly

“force,” and also, in order to distinguish between this and the following conception, “dead force” (*vis mortua*).

II. On the other hand, the product of the pressure into the space through which it acts, or, again, the product—or half-product—of the mass into the square of the velocity, is named “force.” In order that motion may actually occur, it is in fact necessary that the mass, whatever it may be, should under the influence of a pressure, and, in the direction of that pressure, traverse a certain space, “the effective space” (*Wirkungsraum*): and in this case a magnitude which is proportional to the “pushing force” and to the effective space, likewise receives the name “force;” but to distinguish it from the mere pushing force, by which alone motion is never actually brought about, it is also called the “*vis viva* of motion,” or “moving force.”

With the *generic conception* of “force,” the higher mechanics, as an essentially analytic science, is not concerned. In order to arrive at it, we must, according to the general rule, collect together the characters possessed in common by the several species. As is well known, the definition so obtained runs thus—“Force is everything which brings about or tends to bring about, alters or tends to alter motion.”

This definition, however, it is easy to see, is tautological; for the last fourteen words of it might be omitted, and the sense would be still the same.

This erroneous solution is occasioned by the nature of the problem, which requires an impossibility. Mere pressure (dead force) and the product of the pressure into the effective space (living force) are magnitudes too thoroughly unlike to be by possibility combined into a generic conception. Pressure or attraction is, in the theory of motion, what affinity is in chemistry—an abstract conception: living force, like matter, is concrete; and these two kinds of force, however closely connected in the region of the association of ideas, are in reality so widely separated that a frame which should take them both in must be able to include the whole world.

There are several conceivable ways of escaping from the difficulty. For instance, just as we speak of absolute weight, specific weight, and combining weight, without its ever entering any one’s head to want to construct a generic idea out of these distinct notions, so two or more meanings may be attached to the word force. This is what is actually done in the higher mechanics, and hence in this branch of science we meet with no mention of a generic conception of “force.”

There has been no lack of recommendations to carry, in like manner, the notions of “dead” and “living force” as distinct and separate through the other departments of science; it has,

however, been found impossible to put in practice such recommendations; for the use of ambiguous expressions, which can in no case contribute anything to clearness, is altogether inadmissible if confusion can possibly arise. It is true that the mathematician is in no danger of confounding in his calculations a product with one of its factors; but in other departments of knowledge a systematic confusion of ideas exists on this point; and if anything is to be done towards clearing it up, the source of the error must be stopped; for if we once recognize two meanings of the word "force," it would be the labour of Sisyphus to try to distinguish between them in each separate case. In order, then, to arrive at any result, we must make up our minds to do without any common denomination of the magnitudes mentioned above as I. and II., and either to give up the use of the word "force" altogether, or to employ it for *one* only of these two categories.

The notion of force was consistently employed in the latter sense by Newton. In solving his problems, he decomposes the product of the attraction into the effective space into its two factors, and calls the former by the name "force."

As an objection to this mode of proceeding, it must, however, be remarked that in many cases it is not possible thus to decompose the product in question. Let us take, for instance, the following very simple case: a mass M , originally at rest, is caused to move with the (uniform final) velocity c ; from the knowledge of the magnitudes M and c it is certainly possible to deduce the value of the product of the force (in Newton's sense) into its effective space, but we are not thereby enabled to conclude as to the magnitude of this force itself.

As a matter of fact, the necessity soon made itself felt of treating and *naming* this product as a whole. It also has been called "force," and the expressions "*vis viva* of motion," "moving force," "working force," "horse-power" (or force), "muscular force," &c. have been long naturalized in science.

However happy we may, in many respects, think the choice of this word, there is still the objection that a new meaning has been fixed upon an already existing technical expression, without the old one having been called in from circulation at the same time. This formal error has become a Pandora's box, whence has sprung a Babylonian confusion of tongues.

Under existing circumstances no choice is left us but to withdraw the term "force" either from Newton's dead force or from Leibnitz's living force; but in either case we come into conflict with prevailing usage. But if once we have made up our minds to introduce into our science a logically accurate use of terms, even at the cost of existing expressions which have become easy

and pleasant to us by long usage, we cannot long hesitate in the choice we have to make between the conceptions I. and II.

Let us consider the elementary case of a mass, originally at rest, which receives motion: this happens, as has been already said, by the mass being subjected to a certain push or pull under the influence of which it traverses a certain space, the effective space. Now, however, both the velocity and also the intensity of the push (Newton's force) always vary at every point of the effective space; and in order to multiply these variable magnitudes into the effective space, that is, to deduce the quantity of motion from the intensity of the pushing force, we must call in the aid of the *higher* mathematics.

But hence it follows that, except in statics, where the effective space is nought and the pressure constant, the Newtonian conception of force is available only in the higher branches of mechanics; and it is plainly not advisable so to choose our conception of "force" that it cannot be consistently employed in that branch (namely the elementary parts of the theory of motion) which of all others is chiefly concerned with fundamental notions.

It is, however, a totally mistaken method to try to adapt the idea of a force, such as gravity, conceived in Newton's sense, to the elementary parts of science, by leaving out of consideration one of its most important properties, namely its dependence on distance, and to make a "force" out of Galileo's gravity thus inexactly and in some relations most incorrectly conceived. Some such ideal force (No. III.) seems to hover before the minds of most writers on natural science as the original type of a "force of nature."

Such quantitative determinations as hold good only approximately and under certain conditions ought never to be employed to establish *definitions*. In a calculation, it is true, we may correctly enough take an arc, which is sufficiently small in comparison with the radius, as equal to the sine or to the tangent; but if we attempted to use such a relation in settling first principles, we should lay a foundation for fallacies and errors.

The Newtonian idea of force, however, transplanted in the manner that is commonly done into the region of elementary science, is no whit better than the notion of a straight curve. Newton's force, or attraction, *in specie* gravity, g , is equal to the differential quotient of the velocity by the time; that is, $g = \frac{dc}{dt}$.

This expression is quite exact, but in order to understand and apply it a knowledge of the higher mathematics is required. On the other hand, it is quite true that, so long as we have to do only with cases in which the space fallen through is so small in comparison with the earth's semidiameter that it may be disre-

garded, the equation just given may be abbreviated into the very convenient form $g = \frac{c}{t}$ without any considerable error; but this expression can never be mathematically exact so long as the space fallen through has any calculable magnitude. But on the strength of an equation thus radically inaccurate, there are planted in the receptive mind of youth such false notions as,—that gravity is a uniformly accelerating (?) force, a moving (?) force whose action is proportional to the time (?); that force is directly proportional (?) to the velocity produced; and many other like errors.

It would certainly be a great merit if authors of treatises on physics would help to remedy this state of things, and in framing their definitions would start only from thoroughly exact determinations of magnitudes; for elementary physics in its present form, instead of being a well-grounded science, is only a sort of half-knowledge, such that on passing to the higher and strictly scientific departments the student must try to forget its principles and theorems as quickly as he can.

If we have once convinced ourselves by unprejudiced examination that the retention, under that name, of the conception of force distinguished above by I. has nothing but its origin to recommend it, but much to condemn it, the rest follows almost spontaneously. It accords with the laws of thought, as well as with the common usage of language, to connect every production of motion with an *expenditure* of force. Hence “force” is—

Something which is expended in producing motion; and this something which is expended is to be looked upon as a cause equivalent to the effect, namely, to the motion produced.

This definition not only corresponds perfectly with facts, but it accords as far as possible with that which already exists; for, as I shall show, it contains by implication the conception of force as met with in the higher mechanics, and referred to above by II.

If a mass M , originally at rest, while traversing the effective space s , under the influence and in the direction of the pressure p , acquires the velocity c , we have $ps = Mc^2$. Since, however, every production of motion implies the existence of a pressure (or of a pull) and an effective space, and also the exhaustion of one at least of these factors, the effective space, it follows that motion can never come into existence except at the cost of this product, $ps = Mc^2$. And this it is which for shortness I call “force.”

The connexion between expenditure and performance (in other words, the exhaustion of force in producing its effect) presents itself in the simplest form in the phenomena of gravitation. The

necessary condition of every falling motion is that the centre of gravity of the two masses concerned in it (that is, of the earth and of the falling weight) should approach each other. But in the case of the falling together of the two masses, the approach of their centres of gravity reaches its natural limit, and hence the production of a falling movement is thus bound up with an expenditure, namely, with the exhaustion of the given falling-space, and thereby also of the product of that space into the attraction. The falling down of a weight upon the earth is a process of mechanical combination; and just as in combustion the capacity of performance (that is, the condition of the development of heat) ceases when the act of combination comes to an end, so also the production of motion ceases when the weight has fallen to its lowest position. The weight, when lying on the solid ground, is, like the carbonic acid formed in combustion, nothing but a *caput mortuum*. The affinity, whether mechanical or chemical, is still there after the union just as much as before, and opposes a certain resistance to the reduction of the compound; but its power of performance (*Leistungsfähigkeit*) is at an end as soon as there is no further available falling-space.

Whenever the attraction becomes indefinitely small, or ceases altogether, space is no longer effective space; and thus it follows, from the diminution which gravity undergoes with distance, that falling-space is limited in the centrifugal direction also, and hence that the cause of motion or "force" is, under all circumstances, a finite magnitude which becomes exhausted in producing its effect.

This fundamental physical truth will be most easily perceived when applied to a special case and reduced to figures. When a pound weight is lifted one foot from the ground, the available force is, as every one knows, = 1 foot-pound. If the falling-height of this weight amounts to n feet, n not being a large number, the force may be taken as approximately = n foot-pounds. But supposing n , or the original distance of the weight from the earth, to be very considerable, or indeed infinite, the force (that is, the number of foot-pounds) does not by any means thereby become infinite, but, according to Newton's law of gravitation, it becomes at most = r foot-pounds, where r is the number of feet contained in the earth's semidiameter. Thus how great soever the distance through which a weight falls against the earth, or the time occupied by its fall may be, it can acquire no higher final velocity than 34,450 Paris feet per second. On the other hand, were the mass of the earth four times as great as it is, its bulk remaining the same, the force would likewise become four times as great, and the maximum velocity would be 68,900 feet.

It is one of the essentials of a *good* terminology that it should

put fundamental facts of this kind in a clear light; exactly the opposite, however, is done by the nomenclature at present in use. A few expressions, employed by a very meritorious naturalist in combating my views, may serve to support this assertion.

“Although,” he says, “it is quite true that in nature no motion can be annihilated, or that, as it is commonly expressed, the quantity of motion once in existence continues unceasingly and without any lessening, and although in this sense the character of indestructibility belongs to every proximate cause even, every primary cause, that is, every true physical force, possesses the additional characteristic of being inexhaustible. These characteristics will best admit of being unfolded by the closer consideration of gravity, the most active and widely diffused of the natural forces (primary causes), which, as it were the soul of the world, indestructibly and inexhaustibly upholds the life of those great masses on whose motions depends the order of the universe, while requiring no food from without to call forth its ever renewed activity.”

If these words are intended to contain a material contradiction of the views I have put forward, they must be meant to imply that, by virtue of its being inexhaustible, the attractive power of the earth must be capable of imparting to a falling weight, under certain conceivable circumstances, an infinite velocity. But our author himself in several places lets us see that he has a (quite well-founded) mistrust of any so decided a conclusion: this is shown in the following, among other passages:—“If we follow up the chain of causes and effects to its first beginnings, we come at length to the true forces of nature, to those primary causes whose activity does not require that they should be preceded by any others, which ask for no nourishment, but which can ever call forth new motions, as it were, out of an inexhaustible soil, and can uphold and quicken those that are already in being.”

Again: “If the moon every moment falls, at least virtually, a certain distance towards the earth, what is the force which the next moment pulls it away again, as it were, in order to give rise to a new falling force? It is precisely its indestructibility and inexhaustibility, its power at all times and under all circumstances to bring about without ceasing, at least virtually, the same effects, that is the essence of every true force or primary cause.”

This “as it were” and “at least virtually,” which always slips in at the critical moment, affords room for the suspicion that our author is himself not quite confident of the power of his “true natural causes” to give rise to an inexhaustible amount of motion (of actual exertion of force); and the indefiniteness of these expressions is quite characteristic of the Protean part which

the force of gravity plays in writings on natural science. The most arbitrary explanations are given of this word, and then, when facts no longer admit of anything else, a retreat is sought in the Newtonian conception.

Gravity being called a force, and at the same time the term force being connected, in accordance with the common use of language, with the conception of an object capable of producing motion, leads to the false assumption that a mechanical effect (the production of motion) can be produced without a corresponding expenditure of a measurable object; and here is likewise plainly the reason why our author could neither keep clear in his facts nor consistent in his reasoning. If once the production of motion out of nothing is granted, the annihilation of motion must also be admitted as a consequence; and the magnitude of motion must, in accordance with this assumption, be simply proportional to the velocity, or $= Mc$, and the "quantity of motion once in existence" must be $= + Mc - Mc = 0$. But notwithstanding his "inexhaustible forces," the writer referred to expressly declares that motion is indestructible; but, instead of stating his opinion as to what becomes of motion which disappears by friction, he says in another place again that it remains "undecided" whether the effect of a force (the amount of motion produced by it) is measured by the first or by the second power of the velocity (that is, whether it is or is not destructible): he even appears, from repeated expressions, to hold it possible that a given quantity of heat can produce motion *in infinitum*! If such were the case, it would certainly be useless to consider the convertibility of these magnitudes: the ground would rather have been won for the contact theory.

The polemics of my respected critic, whom I have here introduced as the representative and spokesman of prevailing views, and to whom I feel that my sincere thanks are due for his attentive examination of my first publication, appear to me to be necessarily without result, inasmuch as the first problem in combating my assertions, which all revolve about the *one* point of an invariable quantitative relation between heat and motion, must be to find out that this relation is variable, and in what cases. Formal controversy without a material basis is only beating the air; and as to what relates specially to the questions about force, the first point to consider is, not what sort of thing a "force" is, but to what thing we shall *give the name* "force." Backwards and forwards talk about gravity is fruitless, since all who understand the matter are agreed as to its nature; for gravity is and remains a differential quotient of the velocity by the time, directly proportional to the attracting mass, and inversely proportional to the square of the distance: on this point a final

decision was come to long ago. But whether it is expedient to call this magnitude a force is quite another question.

Since, whenever an innovation of essential importance is proposed, the public is so ready to misapprehend, I will here state once more, as clearly as I can, my reasons for saying that "the force of gravity" is an improper expression.

It is an unassailable truth that the production of every falling motion is connected with a corresponding expenditure of a measurable magnitude. This magnitude, if it is to be made an object of scientific investigation (and why should it not?), must have a name given to it; and in accordance with the logical instinct of man, as manifested in the genius of language, no other name can be here chosen than the word "force." But since this expression is already used in a quite different sense, we might be tempted to create for the conception which is as yet—in the fundamental parts of science at least—unnamed an entirely new name. But before betaking ourselves to this extreme course, which for reasons that are not far to seek would be the one whereby we should be brought most into conflict with existing usage, it is reasonable to inquire whether the word "force," which in itself answers so well to the requirements of the case, is in its right place where it was first put by the schools.

According to the common custom of speech, we understand by "force" something moving—a cause of motion; and if, on the one hand, the expression "moving force" is for this reason, strictly speaking, a pleonasm, the notion of a not moving or "dead" force is, on the other hand, a *contradictio in adjecto*. If it be said, for instance, that a load which presses with its weight on the ground exerts thereby a force—a force which, though never so great, is unable of itself to bring about the smallest movement—the mode of conception and of expression is quite justified by scholastic usage, but it is so far-fetched that it becomes the source of unnumbered misapprehensions.

Between gravity and the force of gravity there is, so far as I know, no difference; and hence I consider the second expression unscientific, inasmuch as it is tautological.

Let it not be objected that the "force" of pressure, the "force" of gravity, cohesive "force," &c. are the higher causes of pressure, gravity, and the like. The exact sciences are concerned with phenomena and measurable quantities. The first cause of things is Deity—a Being ever inscrutable by the intellect of man; while "higher causes," "supersensuous forces," and the rest, with all their consequences, belong to the delusive middle region of naturalistic philosophy and mysticism.

By a law that is universally true, waste and want go hand in hand. If to the case before us, where this rule likewise meets

with confirmation, we apply an equalizing process, and take away the word "force" from the connexion in which it is superfluous and hurtful, and bring it to where we are in want of it, we get rid at one time of two important obstacles. The higher mathematics at once cease to be required in order to gain *admittance* into the theory of motion: nature presents herself in simple beauty before the astonished eye, and even the less gifted may now behold many things which hitherto were concealed from the most learned philosophers.

Force and matter are indestructible objects. This law, to which individual facts may most simply be referred, and which therefore I might figuratively call the heliocentric stand-point, constitutes a natural basis for physics, chemistry, physiology, and philosophy.

Among the facts which, though known, have been hitherto only empirically established and have remained isolated, but which can be easily referred to this natural law, is the one that electric and magnetic attraction cannot be isolated any more than gravity, or that the strength of this attraction undergoes no alteration, so long as the distance remains the same, by the intervening of indifferent substances (non-conductors).

Among facts which have remained unknown up to the most recent times, I will refer only to the influence which the ebb and flow of the tide exerts, in accordance with the known laws of mechanics, on the motion of the earth about its axis. A fact of such importance, standing, as it does, in close relation with the fundamental law just stated, having been able to escape the attention of naturalists, is of itself a proof that the prevailing system has no *exclusive* title.

For the rest, it will not have escaped those who are acquainted with the modern literature of science that a modification of scientific language in the sense of my views is actually beginning to take place. But in matters of this kind the chief part of the work must be left to time.

According to what has been said thus far, the *vis viva* of motion must be called a force. But since the expression *vis viva* denotes in mechanics, not only a magnitude which is proportional to the mass and to the square of its velocity, but also one which is proportional to the mass and to the height from which it has fallen, force thus conceived naturally divides itself into two very easily distinguished species, each of which requires a distinct technical name, for which the words *motion* (*Bewegung*) and *falling-force* (*Fallkraft*) seem to me the most appropriate*.

[* The distinction here drawn between "motion" and "falling force" is the same as that made by Helmholtz (*Die Erhaltung der Kraft*, 1847) between "*vis viva*" (*lebendige Kraft*) and "tension" (*Spankraft*). The En-

Hence, according to this definition, "motion" is always measured by the product of the moved mass into the *square* of the velocity, never by the product of the mass into the velocity.

By "falling-force" we understand a raised weight, or still more generally, a distance in space between two ponderable bodies. In many cases falling-force is measured with sufficient accuracy by the product of the raised weight into its height; and the expressions "foot-pound," "kilogramme-metre," "horse-power," and many others, are conventional units for the measurement of this force, which have of late come into general use, especially in practical mechanics. But in order to find the exact quantitative expression for the magnitude in question, we must consider (at least) two masses existing at a determinate distance from each other, which acquire motion by mutually approaching; and we must investigate the relation which exists between the conditions of the motion, namely, the magnitude of the masses and their original and final distance, and the amount of motion produced.

It very remarkably happens that this relation is the simplest conceivable; for, according to Newton's law of gravitation, the quantity of motion produced is directly proportional to the masses and to the space through which they fall, but inversely proportional to the distances of the centres of gravity of the masses before and after the movement. That is, if A and B are the two masses, c and c' the velocities which they respectively acquire, and h and h' their original and final distances apart, we have

$$Ac^2 + Bc'^2 = \frac{A \cdot B(h-h')}{h \cdot h'};$$

or in words, *the falling-force is equal to the product of the masses into the space fallen through divided by the two distances.*

By help of this theorem, which, as will be easily seen, is nothing but a more general and convenient expression of New-

glish expressions "dynamical energy" and "statical energy" were used by Prof. W. Thomson (Phil. Mag. S. 4. vol. iv. p. 304, 1852) in the same sense, but were afterwards abandoned by him in favour of the terms "actual energy" and "potential energy" introduced by Prof. Rankine. More recently ('Good Words' for October 1862) Professors Thomson and Tait have employed the expression "kinetic energy" in place of "actual energy." The German word *Kraft* in the text has been uniformly translated *force*, to which term the ambiguity of the German original has thus been transferred. This ambiguity, however, may be avoided in English by allowing the word "force" to retain the meaning which it bears in common language, that is, to denote all resistances which it requires the exertion of a *power* to overcome (whence the expressions gravitating force, cohesive force, &c.), and by using the word "energy" to denote force as defined by Mayer.—G. C. F.]

ton's law of gravitation*, the laws of the fall of bodies from cosmical elevations, and also the general laws of central motions, can be developed without its being needful to employ equations of more than the second degree.

Having now become acquainted with two species of force—motion and falling force—we can arrive at a conception of “a force” in general, according to the well-known rule, by collecting together the common characteristics of the two species. To this end, we must consider the properties of these objects somewhat more closely. Their most important property depends on their mutual relation. Whenever a given quantity of falling force disappears, motion is produced; and by the expenditure of this latter, the falling force can be reproduced in its original amount.

This constant proportion which exists between falling-force and motion, and is known in the higher mechanics under the name of “the principle of the conservation of *vis viva*,” may be shortly and fitly denoted by the term “transformation” (*Umwandlung*). For instance, we may say that a planet, in passing from its aphelion to its perihelion, transforms a part of its falling force into motion, and, as it moves away from the sun again, changes a part of its motion into falling force. In using the word “transform” in this sense, nothing else can or is intended to be expressed but a constant numerical ratio.

But it follows from the axiom mentioned at page 500, that the production of a definite quantity of motion from a given quantity of falling-force, and *vice versa*, implies that neither falling-force nor motion can be annihilated either totally or in part. We thus obtain the following definition:—

Forces are transformable, indestructible, and (in contradistinction from matter) imponderable objects. (Conf. paper already quoted, p. 502.)

It is easy to see that this definition embraces, among other things, the fact that the motion which disappears in mechanical processes of different kinds bears a constant relation to the heat thereby produced, or that motion is convertible, as an indestructible magnitude, into heat. Thus heat is, like motion, a force; and motion, like heat, an imponderable.

I have characterized the relation which various forces bear to

* Newton's formula relates to the particular case in which the two distances (the initial and the final distance) are equal, so that their product becomes a square. In this case, however, both the space fallen through and the velocity become nought; and hence, when this expression has to be taken as the starting-point for the calculation of real velocities, mathematical artifices become necessary which are inadmissible in the elementary branches of science.

one another by saying (Phil. Mag. S. 4. vol. xxiv. p. 372) that they are "different forms under which one and the same object makes its appearance." At the same time I have expressly guarded myself from making the certainly plausible, but unproved and, as it seems to me, hazardous deduction that thermal phenomena are to be regarded as merely phenomena of motion. The following is what I said upon this point (*loc. cit.* p. 376):—

"But just as little as the connexion between falling-force and motion authorizes the conclusion that the essence of falling-force is motion, can such a conclusion be adopted in the case of heat. We are, on the contrary, rather inclined to infer that before it can become heat, motion—whether simple, or vibratory as in the case of light and radiant heat, &c.—must cease to exist as motion."

The relation which, as we have seen, subsists between heat and motion has regard to quantity, not to quality; for (to borrow the words of Euclid) things which are equal to one another are not therefore similar. Let us beware of leaving the solid ground of the objective, if we would not entangle ourselves in difficulties of our own making.

In the mean time it at least results from the foregoing considerations that the phenomena of heat, electricity, and magnetism do not owe their existence to any peculiar fluids; and the immateriality of heat, asserted half a century ago by Rumford, becomes, through the discovery of its mechanical equivalent, a certainty.

The form of force denoted by the name "heat" is plainly not single, but includes several distinct, though mutually equivalent, objects, three principal forms of which are distinguished in common language: namely, I. Radiant Heat; II. Free (sensible) Heat, Specific Heat; and III. Latent Heat.

There can be no doubt that radiant heat must be regarded as a phenomenon of motion, especially since the recent detection of phenomena of interference in the radiation of heat. But whether there really exists, as is commonly assumed, a peculiar æther, of which the vibratory motion is perceived by us as radiant heat, or whether the seat of this motion is the particles of material bodies, is a question that is not yet made out.

Still greater obscurity hangs about the essential nature of specific heat, or what goes on in the interior of a heated body. Not only does the unanswered question of the æther enter again here, but, before we can be in a position to form any clear ideas on this subject, we require to have an exact knowledge of the internal constitution of matter. We are, however, still far from having reached this point; for, in particular, we do not know whether such things as atoms exist—that is, whether matter

consists of such constituents as undergo no further change of form in chemical processes.

But a span of that time which stretches both backwards and forwards into eternity is meted out to man here on earth, and the space which his foot can tread is narrowly bounded above and below: so also his scientific knowledge finds natural limits in the direction of the infinitely small as well as of the infinitely great. The question of atoms seems to me to lead beyond these limits, and hence I consider it unpractical. An atom in itself can no more become an object of our investigation than a differential, notwithstanding that the *ratio* which such immensely small auxiliary magnitudes bear to one another may be represented by concrete numbers. In every case, however, the conception of an atom must be regarded as merely relative, and must be considered in connexion with some definite process; for, as is well known, the particles of an acid and base may play the part of atoms in the formation and decomposition of a salt, while in another process these atoms may themselves undergo further division.

But assuming that, in a chemical sense, atoms have a real existence—an assumption which, among other things, the laws of isomorphism certainly render probable—the further question arises whether, by the continued division of matter, we can at last arrive at molecules which are atoms in relation to the *phenomena of heat*, such that heat cannot penetrate to their interior, and such that, when the whole mass is heated, they for their parts undergo no increase of bulk. But since we are unable to grapple with such preliminary questions as these, we are forced to confess that, whether the existence of an æther and of atoms be admitted or not, we are, so far as regards the nature of specific heat, in a state of ignorance.

The expression “latent heat” has reference to its correctly recognized property of indestructibility. In all cases in which thermometrically sensible specific heat disappears, it must be assumed that it eludes our perception only by taking on some other state of existence, and that by an appropriate process of inverse transformation the free heat can be reproduced in its original amount. These are the facts on which the doctrine of latent heat rests; and hence, if we have regard to them only, all the connected phenomena may be claimed as so many confirmations of the principle of the transformation and conservation of force.

The conception of latent heat is accordingly nothing else than the conception of something equivalent to free heat, and thus the doctrine of free and specific heat embraces pretty nearly the whole domain of physics. A few examples, chosen from among the abundance of facts, may serve to show how, according to my

view, the phenomena wherein heat becomes latent are to be regarded.

If heat is communicated to a gas retained under constant pressure, the free heat of the gas is increased, and at the same time a calculable quantity of heat becomes *latent*; the gas is thereby caused to expand, and there is consequently produced an amount of *vis viva* proportional to the pressure and to the space through which expansion takes place. Therefore as soon as we know how much of the heat that has become latent is to be attributed to the expansion of the gas, we know also the amount of the remainder of the latent heat corresponding to the *vis viva* produced. Now Gay-Lussac has proved by experiment that the specific heat of a gas undergoes *no* sensible alteration in flowing from a containing vessel into a vacuum. Hence it follows that a gaseous body opposes no perceptible resistance to the separation of its particles, and that the rarefaction of a gas does not of itself (that is, when it occurs without any evolution of force) cause any heat to become latent. The total quantity of heat which becomes latent by the expansion of a gas is therefore to be taken as the equivalent of the *vis viva* produced.

It results from the principle of the indestructibility of heat—a principle which no one calls in question—that the quantity of heat which has thus become latent must again become free when heat is in any way produced at the expense of the acquired *vis viva* of motion. Motion is latent heat, and heat is latent motion.

The celebrated law of Dulong, that the amount of heat produced by the compression of a gas is dependent on the amount of force expended, and not upon the chemical nature, tension, or temperature of the gas, is a special application of the above general principle. But in the communication so often mentioned I have shown that this law of nature is capable of a very much wider application, and that the heat which becomes latent in the expansion of a gas reappears again in every case, if the *vis viva* thereby produced is employed to generate heat, whether by the compression of air, by friction, or by the impact of non-elastic bodies; and I have there calculated the mechanical equivalent of heat upon principles of which the accuracy cannot be disputed. I also measured at that time, by way of control, the heat produced in the manufacture of paper in Holland, and compared it with the working force expended, and so found a sufficient degree of concordance between the two quantities. I have recently, moreover, succeeded in constructing, for the purpose of the direct determination of the mechanical equivalent of heat, a very simple thermal dynamometer on a small scale, with which the truth of the principle in question can be demonstrated *ad oculos*; and I have reason to believe that the efficiency of

water-wheels and steam-engines might be easily and advantageously measured by means of a similar calorimotorial apparatus. It must, however, be left to the future judgment of practical men to decide whether, and to what extent, this method deserves to be preferred to Prony's.

Heat further becomes latent in certain changes of the state of aggregation of bodies. Since it is a settled fact that both solid and liquid bodies oppose a certain resistance to the separation of their parts, and since in general an expenditure of *vis viva* is required for the overcoming of mechanical resistances, we are led to conclude *à priori* that whenever the cohesion of a body is diminished or done away with, force or heat must become latent; and this, as is well known, perfectly accords with experience.

Starting from this point of view, the French physicist Person has attempted to detect a direct quantitative relation between the latent heat of metals, on which he has made a great number of observations, and their cohesion; but at present determinations of this kind are beset with almost insurmountable difficulties.

The heat which becomes latent in the evaporation of water has been considered from quite a similar point of view by Holtzmann in his important memoir "On the Heat and Elasticity of Gases and Vapours." Starting from the principle that elevation of temperature is equivalent to the raising of a weight, this philosopher has likewise calculated the mechanical equivalent of heat from the quantity of heat which becomes latent by the expansion of a gas; and he very rightly conceives of the latent heat of steam as made up of two parts, whereof one, the smaller, is expended in overcoming the opposing pressure of the atmosphere, and can hence be easily calculated by means of the mechanical equivalent of heat, while the remaining part, the amount of which can also be calculated, is what Holtzmann calls the heat required to destroy the cohesion of the water. In all steam-engines this latter portion is wasted, and Holtzmann calculates from these data the superior efficiency of high-pressure compared with low-pressure engines*.

If the view here taken of the latent heat of fusion and evaporation is correct, heat must also become latent when hard bodies are reduced to powder; and when such substances pass into the liquid condition from a state of fine division, they must absorb a smaller quantity of heat than when they are liquefied without previous comminution. A few experiments that I have

* The engines which give the greatest useful effect must be those in which the steam receives an addition of heat during its expansion.

instituted in this direction have not hitherto given any decisive result.

It is also worthy of notice that certain solid bodies which are capable of assuming allotropic states, as, for instance, the oxygen-compounds of iron, evolve a considerable quantity of heat on passing from a less to a more hard condition. Such facts, the number of which will doubtless continually increase with time, agree perfectly with the above principle, that diminution of cohesion involves an *expenditure* of heat, and, on the other hand, increase of cohesion a *production* of heat.

Customary language, according to which gravity is called a moving force and heat a substance, occasions, on the one hand, the significance of an important natural object, falling-space, or the space through which a body falls, to be kept as much as possible out of sight, and, on the other hand, heat to be removed to the greatest possible distance from the *vis viva* of motion. The sciences are thus reduced to an artificial system, over whose fissured surface we can advance in safety only by the powerful aid of the higher analysis.

Without doubt the fact that so simple and obvious a matter as the connexion between heat and motion could remain unperceived up to the most recent times must also be attributed to the same defect. Nevertheless, as has been already pointed out, the quantitative determination of chemical heating-effects and of galvanic actions, as well as researches into vital phenomena, instituted in the spirit of those of Liebig, must soon have led to the law, not difficult to discover, of the equivalence of heat and motion.

In reality this law and its numerical expression, the mechanical equivalent of heat, were published almost simultaneously in Germany and in England.

Starting from the fact that the amount of chemical as well as of galvanic effect is dependent only and solely on the amount of material expenditure, the celebrated English physicist Joule was led to the principle that the phenomena of motion and of heat rest essentially upon one and the same foundation, or, as he expressed himself, in the same way as I have done, heat and motion are transformable one into the other.

Not only did this philosopher indisputably make an independent discovery of the natural law in question, but to him belongs the credit of having made numerous and important contributions towards its further establishment and development. Joule has shown that when motion is produced by means of electro-magnetism, the heating effect of the galvanic current is diminished in a corresponding and fixed proportion. He has further ascer-

tained that by reversing the poles of a magnetic bar a quantity of heat is produced proportional to the square of the magnetic tension,—a fact which was also discovered by myself, though at a later date. In particular, Joule has likewise demonstrated, by means of numerous experiments, that the heat evolved by friction under various circumstances stands in an unvarying proportion to the amount of force expended. According to his most recent experiments of this kind, he has fixed the mechanical equivalent of heat at 423*.

Joule has likewise investigated experimentally, in relation to this question, the thermal behaviour of elastic fluids when expanded, and has thereby confirmed the earlier results of other physicists.

The new subject soon began to excite the attention of learned men; but inasmuch as both at home and abroad the subject has been exclusively treated as a foreign discovery, I find myself compelled to make the claims to which priority entitles me; for although the few investigations which I have given to the public, and which have almost disappeared in the flood of communications which every day sends forth without leaving a trace behind, prove, by the very form of their publication, that I am not one who hankers after effect, it is not therefore to be assumed that I am willing to be deprived of intellectual property which documentary evidence proves to be mine.

By help of the mechanical equivalent of heat many problems can be solved which, without it, could not be attacked at all: among them, the calculation of the thermal effect of the falling together of cosmical masses may be especially mentioned. It will not be out of place to indicate here briefly a few results of such calculations.

The following is one problem of this kind. It is assumed that a cosmical body enters the atmosphere of our earth with a velocity of four geographical miles per second, and that, in consequence of the resistance which it here encounters, it loses so much of its *vis viva* of motion that its remaining velocity when it again quits the atmosphere amounts to 3 miles: the question now arises, How great is the thermal effect which accompanies this process?

A simple calculation, based upon the mechanical equivalent of heat, shows that the quantity of heat required is about eight times as great as the heat of combustion of a mass of coal of equal weight with the body in question, 1 kilogramme of coal

* That is, 1 thermal unit = 423 kilogrammetres.

being taken as yielding 6000 thermal units. Hence it follows that the velocity of the motion of shooting-stars and fire-balls, which, as is well known, attains, according to astronomical observations, to from 4 to 8 miles, is a cause fully sufficient to produce the most violent evolution of heat, and an insight into the nature of these remarkable phenomena is thereby afforded to us*.

The following is a problem of a similar kind: if two cosmical masses, moving in space about their common centre of gravity, were by any cause whatever, for example by the resistance of the surrounding medium, caused to fall together, the question again arises, How great is the thermal effect corresponding to this process of mechanical combination?

Even though the elements of the orbits (that is, their eccentricity) may be unknown, we can nevertheless calculate from the given weight and volume of the masses in question the maximum and the minimum of the required effect. Thus let it be supposed, for the sake of an example, that our earth had been divided into two equal globes which had united in the manner described: calculation teaches us that the amount of heat which would have been evolved in such a case would considerably exceed that which an equal weight of matter could furnish by the most intense process of chemical action.

It is more than probable that the earth has come into existence in some such way, and that in consequence our sun, as seen from the distance of the fixed stars, exhibited at that epoch a transient burst of light. But what took place in our solar system perhaps millions of years ago, still goes on at the present time here and there among the fixed stars; and the transient appearance of stars, which in some cases, like the celebrated star of Tycho Brahe, have at first an extraordinary degree of brilliance, may be satisfactorily explained by assuming the falling together of previously invisible double stars.

Contrasting with such explosive bursts of light is the steady radiation, shown continuously through enormous periods, by the greater number of fixed stars, and among them by our sun. Do these appearances, which in so special a manner tempt to higher speculations, constitute a real exception to the exhaustion of a cause in producing its effect, which, in accordance with the foregoing considerations, we have regarded as an established law of Nature? or does the small sum of human knowledge authorize us in supposing that here also there is an equivalence between

* The idea that the meteors here referred to owe their light to a mechanical process—whether friction, or the compression of the air—is not new; but without a knowledge of the mechanical equivalent of heat it could have no scientific foundation.

performance and expenditure, and in searching for the conditions of that equivalence?

To enter further upon this subject would lead us beyond the intended scope of this publication; and I therefore close in the hope that the reader will please to supplement by his own reflection much that in this tract has been left unsaid.

LXVIII. *On some Reactions of Hydride of Benzoyle.*

By ARTHUR H. CHURCH, B.A. Oxon.*

Action of Sodium on Hydride of Benzoyle.

IT has been observed that when sodium is heated with pure hydride of benzoyle, although the metal disappears, there is no evolution of hydrogen. While recording this phenomenon in his *Traité de Chimie Organique*, Gerhardt, struck by its singularity, expressed a doubt as to the accuracy of the observation. The point evidently required further elucidation, yet the proneness of the materials to oxidize and otherwise change rendered the investigation by no means inviting. However, in pursuing it, I have been rewarded with several interesting results which I would now briefly record.

Having prepared some hydride of benzoyle in a state of perfect purity, I determined to bring it into contact with sodium in the presence of a liquid solvent itself inert. For this purpose I chose that portion of coal-naphtha which boils between 100° and 110° C. After having rendered it anhydrous, I dissolved in it a weighed quantity of the hydride (about one-twentieth of the naphtha used). Into the solution, contained in a small long-necked flask with a condensing tube, a clean weighed globule of sodium attached to a platinum wire is introduced, and the flask then gently warmed until an action has been set up. Occasionally it becomes necessary to moderate the violence of the change by cooling the vessel. No gas whatever is evolved; and when the reaction is complete, the globule of fused sodium remains bright even when the liquid is long kept boiling, the dark green crusts of the new sodium-compounds ceasing to be formed. But further experiments showed that this reaction is more completely under control when sodium-amalgam is used in place of the pure metal: the new body is then obtained in nearly colourless gelatinous flakes. In order to determine quantitatively the nature of the change, I have weighed in several experiments the residual sodium withdrawn from the flask when cold, and plunged in a counterpoised vessel of Persian naphtha—the

* Communicated by the Author.

amount of sodium in the product being ascertained also by a direct determination in the whole quantity made from a known weight of benzoic aldehyde. Of the following analyses, the most accordant of a large number, the two first were performed on the compound as prepared with pure sodium; the two latter on the body owing its origin to the action of sodium-amalgam. Although the flasks were supplied with a slow stream of nitrogen in order to prevent oxidation during the experiment, yet small quantities of benzoate of sodium were invariably produced. This formation explains the large discrepancies in my results, amounting in some cases to a difference in the percentage of sodium of more than 3 per cent.

I.	3.13	gram.	of	$C^7 H^5 O$,	H	}	.695	gram.	of	Na	by	analysis.
				dissolved	.			.671				
II.	.76					}	.169					analysis.
								.149				
III.	1.045						.218					analysis.
IV.	1.019						.22					analysis.

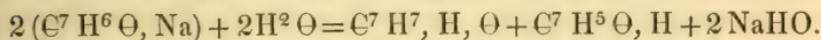
These numbers correspond to the following percentages of sodium in the product:—

I.	II.	III.	IV.
Mean.	Mean.		
17.83	17.3	17.18	17.75

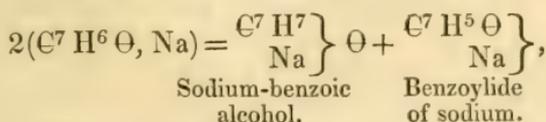
which seem to point to the empirical formula



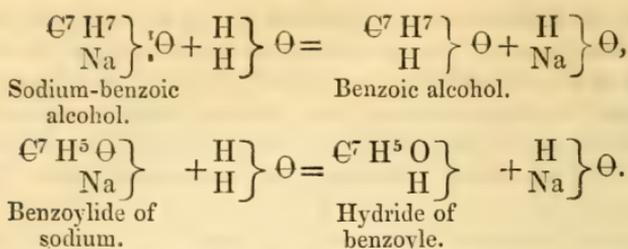
for the sodium-compound, which requires 17.83 per cent. of sodium. This formula is scarcely admissible on several grounds. To double it merely would not be to make it more intelligible; but I think a reasonable rearrangement of the doubled formula is suggested by the decomposition which the new sodium-body undergoes when treated with water (or acids),—benzoic alcohol (hydrate of benzyle or toluenyle), benzoic aldehyde, and hydrate of sodium being formed, according to the equation



Now, if we assign to the original sodium-compound the formula



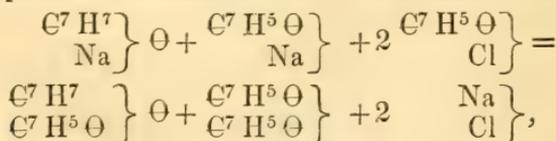
then the reaction with water may be thus explained:—



The correctness of these equations is abundantly confirmed by every observation hitherto made—such as the amount of sodium absorbed, the non-evolution of hydrogen either in the action of sodium on the aldehyde or in the action of water upon the sodium-compound; the actual experimental production, as above mentioned, of (about) 1 equiv. of benzoic alcohol and 1 equiv. of benzoic aldehyde to 2 equivs. of hydrate of sodium; but above all, the nature of the two chief final products, benzoic alcohol and benzoic aldehyde, which may most reasonably be supposed to owe their origin to the action of water upon their respective sodium-compounds. It should, however, be here stated that when sodium is heated with hydride of benzoyle, the action is occasionally arrested when but half complete: it is difficult to account for the phenomenon, except on the supposition that benzoyle is formed instead of benzoylide of sodium.

Since the sodium-compounds do not admit of accurate analysis or complete separation from one another, I have not only identified the benzoic alcohol and aldehyde which they yield when treated with water, with the common forms of these substances, but I have examined the action of hydrochloric acid and of chloride of benzoyle upon them.

When hydrochloric acid gas is passed through benzole in which the sodium-compounds are suspended, chloride of toluenyle (benzyle) is produced. This liquid, identified by Cannizzaro* with chlortoluole, boils at 185° C. It distils over with some quantity of regenerated oil of bitter almonds. When, in this experiment, chloride of benzoyle is substituted for hydrochloric acid, the benzole when distilled off, after separation of the chloride of sodium, leaves as a residue a partly crystalline mass which theoretically should contain benzoyle along with the benzoyle-compound of benzoic alcohol—the benzoate of toluenyle,

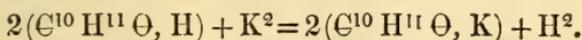


Hitherto, however, I have not been able to verify this supposi-

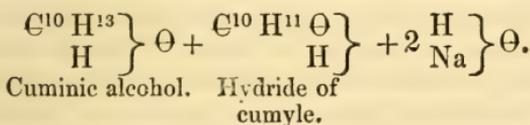
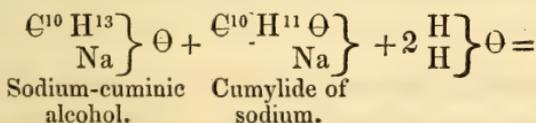
* *Jahresbericht*, 1855, p. 621.

tion completely, owing to the difficulty of separating and purifying the products of the reaction.

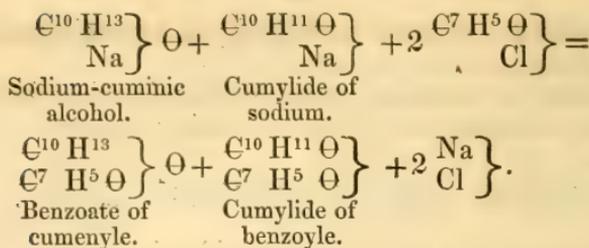
According to Gerhardt, hydride of cumyle, when heated with potassium, disengages hydrogen, forming cumylide of potassium:—



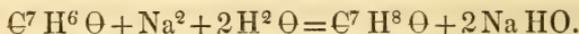
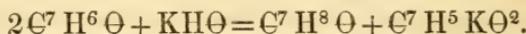
I have not succeeded in obtaining an identical result. If potassium or sodium and cuminole be heated to the boiling-point of the latter substance, a certain quantity of hydrogen is indeed liberated, but far less than the volume demanded by the above equation; while this disengagement of gas is attended with a more profound and irregular decomposition. This change also occurs in the benzoyle series. When, however, cuminole and sodium-amalgam are brought together, as before described, in the presence of naphtha (cymole itself may be used), sodium-cuminic alcohol and cumylide of sodium are formed in equivalent proportions. Water acting upon these compounds on the one hand regenerates the original hydride of cumyle, and on the other liberates cuminic alcohol—



The cuminic alcohol thus derived is identical with that which occurs in minute proportion in cumin oil, and which is also formed artificially from cuminole by the action of alcoholic potash. It boils at 244° C. Besides these two bodies, a somewhat viscid oil occurs in the product of the reaction. This oil distils over at a high temperature, above 300° C., and is apparently identical with Gerhardt's cumyle. It may be more readily obtained in another way. When the product of the action of sodium on cuminole is treated with chloride of cumyle, the radical is formed, accompanied, however, with cuminate of cymenyle. This mode of procedure, accomplished by Gerhardt, gives therefore a mixed product; and this fact explains one or two anomalies noticed by that chemist, especially in the action of chloride of benzoyle upon his so-called "cumylure de potassium." When the above-named bodies are mixed and heated, the change proceeds to some extent as follows:—



Two methods of obtaining benzoic alcohol, in addition to the process given above, have been described. M. Cannizzaro originally prepared this body by the action of alcoholic potash upon oil of bitter almonds; but M. Friedel* has lately procured it from the same substance by the simultaneous action of water and sodium (sodium-amalgam). These somewhat analogous reactions may be thus formulated:—



Sometimes, however, chiefly when a modification of Friedel's process is employed, along with benzoic alcohol, a white crystalline substance is formed, which I have made the subject of further investigation.

Conjoint Action of Sodium and Water on Hydride of Benzoyle.

Into an intimate mixture of 10 parts of pure hydride of benzoyle and 1 part of water, such an amount of sodium-amalgam is to be added as shall contain 5 parts of sodium, the materials being repeatedly shaken with gradual addition of 2 more parts of water. The flask in which the experiment is made, after the addition of some more sodium-amalgam, should be connected with an apparatus from which hydrogen is being disengaged, and then set aside for ten days or a fortnight, during which time it should be frequently agitated, on each occasion a few drops of water being poured in. The reaction being over, the flask will be found to contain an aqueous solution of soda, benzoate of sodium, together with a semisolid cake chiefly consisting of benzoic alcohol and a new white crystalline substance, which may either be gradually eliminated from the mass by repeated treatments with very dilute boiling alcohol, or be freed from the bodies accompanying it by repeated digestions with strong soda solution. The substance thus purified having been once recrystallized from dilute boiling alcohol, is to be washed with water, dried at 100° C., washed once with cold absolute ether, and then twice recrystallized from the same liquid heated to ebullition.

* *Comptes Rendus*, vol. lv. p. 54.

Combustions of this substance were made with the following results:—

I. .327 grm. of substance gave .926 grm. of CO_2 and .2265 grm. of H_2O .

II. .2725 grm. of substance gave .7755 grm. of CO_2 and .189 grm. of H_2O .

These numbers correspond to the following percentages:—

	I.	II.	Theory ($\text{C}^7\text{H}^8\text{O}$).
Carbon	77.51	77.61	77.78
Hydrogen	7.69	7.70	7.41
Oxygen	14.80	14.69	14.81
	100.00	100.00	100.00

Although the formula of benzoic alcohol, $\text{C}^7\text{H}^8\text{O}$, corresponds so closely with these analytical results, it is scarcely probable that it accurately represents the atomic weight of the new body, which I am inclined to think stands in much the same relation to the true alcohol as benzoine, $\text{C}^{14}\text{H}^{12}\text{O}^2$, does to the true aldehyde. On this supposition of a doubled formula, I propose to name the new substance provisionally *dicresole*. No definite compounds have as yet been obtained from it; so that its true character and position remain undecided.

Dicresole is insoluble in cold water, but it dissolves sparingly in boiling water, a small quantity of pearly scales being precipitated as the liquid cools. It is more soluble in boiling ether, and is very easily dissolved by hot alcohol. It is also soluble in benzole. The presence of benzoic alcohol, of a trace even, renders dicresole far more soluble, and prevents its crystallization.

Dicresole dissolves in sulphuric acid with a deep green colour. Ebullition with nitric acid converts dicresole partly into nitrobenzoic acid, and partly into a yellow nitro-substitution product. Heated for some time with strong potash solution, a portion appears to dissolve, but without colour, while the larger part of the dicresole remains unchanged.

Dicresole dissolved in naphtha and boiled with potassium disengages hydrogen, with the formation of a red compound: the reaction is, however, incomplete and unsatisfactory.

Dicresole melts at or near 129°C ., and, on cooling, often remains liquid till the thermometer has fallen to 70°C . It boils at a very high temperature, and cannot be distilled without partial decomposition. The sublimed substance is deposited in the form of complex volutes of great beauty. Dicresole crystallizes so imperfectly from its various solvents, that I have been unable to determine to what crystalline system it belongs. I regret that its chemical properties have not afforded me any means of

definitely settling the atomic weight of dicresole; while its excessively high boiling-point, and its partial decomposition when distilled, prevent the determination of its vapour-density.

Dicresole may be distinguished from benzoine, which it resembles in some particulars, by its different fusing-point (10° C. higher), by the green colour which it produces with sulphuric acid, and by its indifference to a strong solution of caustic potash. Benzoine itself, indeed, by the action of sodium-amalgam and water, is partially hydrogenated, with the production of a substance apparently identical with dicresole.

I have obtained in the cumyle series a body seemingly homologous with dicresole, but the quantity was quite insufficient for quantitative analysis. It appears that the method of converting an aldehyde into its alcohol by the action of sodium-amalgam upon a solution of the aldehyde in an inert liquid such as benzole, followed by treatment of the sodium compounds thus formed with water, is of very general application. At the same time, the alteration of the aldehyde consequent upon the presence of a caustic alkali, as in Friedel's process, is eliminated by thus dividing the reaction into two stages.

LXIX. On the Delineation of a Cubic Scrole.

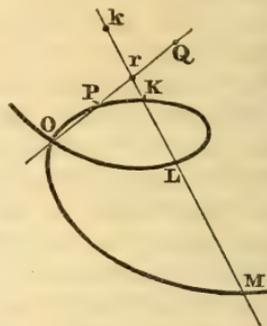
By A. CAYLEY, Esq.*

IMAGINE a cubic scrole (skew surface of the third order) generated by lines each of which meets two given directrix lines. One of these is a nodal (double) line on the surface, and I call it the nodal directrix; the other is a single line on the surface, and I call it the single directrix. The section by any plane is a cubic passing through the points in which the plane meets the directrix lines; *i. e.* the point on the nodal directrix is a node (double point) of the curve, the point on the single directrix a single point on the curve; the two directrix lines, and the cubic curve, the section by any plane, determine the scrole. Consider the sections by a series of parallel planes. Let one of these planes be called the basic plane, and the section by this plane the basic section or basic cubic; and imagine any other section projected on the basic plane by lines parallel to the nodal directrix: such section may be spoken of simply as 'the section,' and its projection as 'the cubic.' The cubic has a node at the node of the basic cubic; that is, the two curves have at this point *four* points in common. The two curves have, moreover, in common the *three* points at infinity (or, in other words, their asymptotes

* Communicated by the Author.

are parallel); in fact the points at infinity of either curve are the points in which the line at infinity, the intersection of the basic plane and the plane of the section, meets the scrole; and these points are therefore the same for each of the two curves. The remaining *two* points of intersection of the cubic with the basic cubic are also fixed points on the basic cubic, *i. e.* they are the points of intersection of the basic plane by the two generating lines parallel to the nodal directrix. Hence the cubic meets the basic cubic in *nine* fixed points, *viz.* the node counting as four points, the three points at infinity, and the two points the feet of the generators parallel to the nodal directrix. It follows that if $U=0$ is the equation of the basic cubic, $V=0$ the equation of some other cubic meeting the basic cubic in the nine points in question, then the equation of 'the cubic' is $U + \lambda V = 0$, λ being a parameter the value of which varies according to the position (in the series of parallel planes) of the plane of the section. Suppose that the basic cubic $U=0$ is given, and suppose for a moment that the cubic $V=0$ is also given, these two cubics having the above-mentioned relations, *viz.* they have a common node and parallel asymptotes: the cubic $U + \lambda V = 0$ might be constructed by drawing through the node (say O) a radius vector meeting the cubics in P, P' respectively, and taking on this radius vector a point Q such that $PQ = \frac{\lambda}{1 + \lambda} PP'$, or, what is the same thing, $OQ = \frac{OP + \lambda OP'}{1 + \lambda}$; the locus of the point Q will then be the cubic $U + \lambda V = 0$. And we may even suppose the cubic $V=0$ to break up into a line and a conic (hyperbola), and then (disregarding the line) use the hyperbola in the construction. In fact, if the hyperbola is determined by the following five conditions, *viz.* to pass through the node and through the feet of the two generators parallel to the nodal directrix, and to have its asymptotes parallel to two of the asymptotes of the basic cubic, and if the line be taken to be a line through the node parallel to the third asymptote of the basic cubic, then the hyperbola and line form together a cubic curve meeting the basic cubic in the nine points, and therefore satisfying the conditions assumed in regard to the cubic $V=0$. And it is to be noticed that as in general the cubic $V=0$ is the projection of some section of the scrole, so the hyperbola and line are the projection of a section of the scrole, *viz.* the section through one of the generating lines (there are three such lines) parallel to the basic plane. But it is better to construct 'the cubic' by a different method, using only the basic cubic $U=0$, and which results more immediately from the geometrical theory. Taking the

basic plane as the plane of the figure, let O be the node, or foot of the nodal directrix, K the foot of the single directrix, Kk the projection of the single directrix, k being the projection of the point in which the single directrix meets the plane of the section. Drawing through O any radius vector meeting the basic cubic in P , and the line Kk in r , and producing it to a properly determined point Q , $OPrQ$ will be the projection of the generating line which meets the nodal directrix, the basic cubic, the single directrix, and the section in the points the projections whereof are O , P , r , Q respectively. And the consideration of the solid figure shows easily that the condition for the determination of the point Q is



$$PQ = Kk \cdot \frac{Pr}{rK}.$$

Hence, starting from the basic cubic and the line Kk , we have a construction for the point Q the locus whereof is the cubic, the projection of a section of the scrole; for the projections of the parallel sections, we have only to vary the length Kk . By what precedes, the construction gives for the locus of Q a cubic having a node at O , and having its asymptotes parallel to those of the basic cubic. As P moves up to K , the distances Pr , rK become indefinitely small; but their ratio is finite, hence the cubic, the locus of Q , does *not* pass through the point K . The construction shows, however, that it does pass through the points L , M , which are the other two intersections of Kk with the basic cubic; these points L , M are in fact the feet of the generators parallel to the nodal directrix.

The general conclusion is, that a series of cubics having each of them at one and the same given point a node—having their asymptotes parallel—and besides passing through the same two given points—may be considered as the projections of a series of parallel sections of a cubic scrole; and such a series of cubics will thus afford a delineation of the scrole.

LXX. *Observations on a passage in Professor Tyndall's Lectures on Force and on Heat.* By D. D. HEATH, Esq.*

IN the abstract of Professor Tyndall's Lecture on Force delivered at the Royal Institution June 6, 1862, there occurs the following passage:—

“There is one other consideration connected with the permanence of our present terrestrial conditions, which is well worthy of our attention. Standing upon one of the London bridges, we observe the current of the Thames reversed, and the water poured upward twice a day. The water thus moved rubs against the river's bed and sides, and heat is the consequence of this friction. The heat thus generated is in part radiated into space, and then lost, as far as the earth is concerned. What is it that supplies this incessant loss? The earth's rotation. Let us look a little more closely at the matter. Imagine the moon fixed, and the earth turning like a wheel from west to east in its diurnal rotation. Suppose a high mountain on the earth's surface; on approaching the moon's meridian, that mountain is, as it were, laid hold of by the moon, and forms a kind of handle by which the earth is pulled more quickly round. But when the meridian is passed the pull of the moon on the mountain would be in the opposite direction, it now tends to diminish the velocity of rotation as much as it previously augmented it; and thus the action of all fixed bodies on the earth's surface is neutralized. But suppose the mountain to lie *always* to the east of the moon's meridian, the pull then would be always exerted against the earth's rotation, the velocity of which would be diminished in a degree corresponding to the strength of the pull. *The tidal wave occupies this position*: it lies always to the east of the moon's meridian; and thus the waters of the ocean are in part dragged as a brake along the surface of the earth, and as a brake they must diminish the velocity of the earth's rotation. The diminution, though inevitable, is, however, too small to make itself felt within the period over which observations on the subject extend. Supposing then that we turn a mill by the action of the tide, and produce heat by the friction of the millstones; that heat has an origin totally different from the heat produced by another mill which is turned by a mountain stream. The former is produced at the expense of the earth's rotation, the latter at the expense of the sun's radiation.”

The paragraph is repeated word for word in the ‘Lectures on Heat.’ There is a reference to Mayer in both places, which leaves me in doubt whether Professor Tyndall has given any serious attention to the matter himself, or has merely copied

* Communicated by the Author.

what he found in an author whom he trusts. Not having access to Mayer, I will speak of the argument as his own.

I cannot persuade myself that it is not vague, incorrect, and inconsistent. If, however, it had appeared in any transactions or other work addressed to the learned, I should not have meddled with it: they can take care of themselves. But where it is, it comes with the authority of a teacher, and its doctrine will be quoted in all the drawing-rooms in London, and I hope in all the schools in the kingdom. Therefore I think that, if questionable, it ought to be questioned, and an explanation called for.

My first difficulty is in ascertaining what is meant to be asserted. The statement at the end, that "the heat produced by the friction of the millstones of a tide-mill is produced at the expense of the earth's rotation," paralleled as it is with "the heat of another mill turned by a mountain stream"—which is indisputably derived from the *work done* by, and therefore is entirely "at the expense of the sun's radiation,"—leads to the inference that the thing spoken of is all the heat produced everywhere by the tidal action; and that it is conceived that this heat, estimated in its dynamical equivalent, would on an average be found to be at all events not greater than the contemporaneous loss of *vis viva* of rotation.

But the most elementary consideration of the matter shows that this is not so. The heat developed at each point is practically ascertained by the strength of the current (*i. e.* of the *relative* velocity of the water), and the coefficient of friction; and as the velocity of the current is sensible and the supposed retardation of rotation is quite insensible in thousands of years, it is quite indifferent to the calculation whether there is a retardation or not. When Professor Tyndall drops his leaden ball from the top of his lecture-room, it is mathematically correct to state that the earth moves, or tends to move, upwards to meet it with an equal momentum*, and that the collision will generally cause a loss of motion and of *vis viva* in the earth as well as in the ball: but if the same experiment happened to be performing simultaneously at his antipodes, the earth's motion would be neutralized. His formula, that "the heat generated increases as the square of the velocity," is strictly true only in the latter case; but the error in the former is infinitesimally small; for though the momentum of the earth is equal to that of the ball, its loss of *vis viva* is immeasurably less. And in like manner, even if every tidal motion over the whole ocean were checking the rotation, the whole mass of the water is so small that the loss of *vis viva* and production of heat would be rightly calculated from the loss

* Mr. Grove, by the way, does not seem to be aware of this elementary doctrine (*Correlation of Forces*, 4th ed. p. 20).

of relative motion in it alone. And much more is this true when the tidal motions are contemporaneously acting in all possible directions, some helping and others retarding the mean rotation, and so, if not keeping it constant, at all events reducing the retardation to a small residual phenomenon.

Is then the statement about "the heat produced" and the comparison with the stream-mill an inadvertence? or do I misunderstand it? And is the true meaning of the paragraph to be found in the earlier portion, where it is said that "the earth's rotation supplies the *loss* of that *part* of the heat which is radiated into space?" Of the whole energy extracted from the tide, is a part *restored* to it in some way by the resulting heat? and does the rest escape, leaving the earth's rotation in some way to make up the balance?

I have already said that the *vis viva* lost by the earth must be immeasurably small compared to that lost by the tide. But what part of the heat representing the total loss is ultimately radiated into space? Is it reasonable to suppose it much, if at all, less than the whole? I should suppose that much the greater part of the heat generated would be at once seized by the water, and in the first instance *tend* towards increasing the temperature of the ocean, but that this tendency would discharge itself at the surface by evaporation and radiation, and that a similar passage through the atmosphere would end in radiating the amount received. And the course of the rest through the solid earth to the atmosphere into space, though more complicated, would, I suppose, be similar. If there is a secular change going on in the temperature of the superficial strata, of the sea, of the air, or if there is any other *increasing* call for any of the work that heat may do, the case may be otherwise. But all the heat produced and not radiated must be for ever producible in the form of additional temperature or work, mechanical or "internal," done in the system: and I find in the passage quoted neither argument nor even suggestion of any causal connexion either between the heat retained and the maintenance of the tide, or of the part radiated and the destruction of the rotation.

I conceive, then, that it is certain that the total heat of the millstones is not "at the expense of," or derived from, the destruction of the earth's rotation, and that no reason is shown for thinking that the millionth part of it is so. Rather, I should say that, if there be any "slip" or loss of rotation by the earth, there is ground for thinking the total heat generated is by so much less than it would have been had any extraneous cause maintained the original amount. And this seems to dispose of the main point of the paragraph.

But further, whether the rotation is diminishing or not, there

does not appear to me to be any cogency in the very picturesque argument or illustration by which the affirmative is enforced.

For what are the essential conditions of the "mountain on the earth's surface?" It is fixed to the solid earth, otherwise equably rotating and indifferent to the moon's action. The direct action is on the mountain alone, which therefore *begins* to move. This motion is arrested by the fixed attachment of the base, and thereupon a *tension* is set up which is the efficient cause of the rest of the mass participating in the impressed motion. The moving force is the attraction on the mountain, and the mass moved the whole earth, and the numerical calculation is perfectly definite: but all depends on the actual strain. If the mountain were a satellite close to the surface and subject to the attraction of the solid earth, it would itself be affected by the moon's attraction, but would not affect the earth. Now none of these conditions are proved to be present, and some are absent, in the tidal phenomena. There is no more cohesion between the crest of the wave and its base than between an apparently fixed cloud and the mountain top on which it rests: both are merely permanent shapes of shifting materials. If one were to try to form the equation for the moving force and the mass moved, how would one proceed? The only connexion is that of friction and of pressure. Both of these (if the latter can be at all conceived as an *unilaterally* effective force exerted by the superimposed crest on the subjacent waters) depend directly on the velocities and internal disposition of the waters; only indirectly, through these, on the moon's action; and for aught that is shown these may be acting in the direction of the rotation at that point, and at any rate are as likely as not to be less energetic there, in whatever direction, than at some other points. All that can be said, as it appears to me, is that the moon's attraction at the crest tends to increase any existing velocity towards the west, or to diminish any in the opposite direction. But then the rest of the ocean is swaying to and fro also, and it cannot be asserted that it (like the supposed solid globe) is indifferent, or that this increased attraction at the crest may not be merely the exact equipoise wanted to prevent an acceleration of rotation.

The question, therefore, whether "the waters are in part dragged as a brake from east to west," seems to me unaffected, or at least not settled by the fact of the existence of crests lying obliquely to the moon. Are they? Laplace and Airy say they are not*; but in the cursory inspection I have made of their pages I have not lighted upon the proof of their assertion. They also give proofs† that precession and nutation (and *à fortiori* the

* *Système du Monde*, p. 267. *Enc. Metr.* "Tides and Waves," p. 126.

† *Mécanique Céleste*, part 1, book v. sects. 11, 12. *Enc. Metr.* qu. sup.

earth's rotation) are unaffected by the fluctuations and friction of the tides. But I will not take upon myself to say their proofs are conclusive; and indeed my impression is that all Laplace means is that the difference, if any, is of an order that may be neglected in comparison with the disturbances he takes account of. The following considerations, which have suggested themselves, or partly have been suggested to me on this subject, are only added in the hopes of eliciting some remarks from one or other of the accomplished mathematicians who write in this Magazine.

If m be the mass of the moon, r its distance, and ϖ its angular velocity (both projected on the equatorial plane), MK^2 the moment of inertia of the earth and sea taken in its instantaneous shape, ω its angular velocity; and if P be taken to represent twice the sum of the products of each molecule of the sea by the square of its distance from the polar axis and by its *relative* velocity eastward; we shall have, from age to age, by the principle of the conservation of areas,

$$mr^2\varpi + MK^2\omega + P = \text{a constant quantity.}$$

P is an extremely small quantity compared with the other terms; and MK^2 , though it may vary, must do so within narrow limits. If therefore $r^2\varpi$, or the area described by the moon in a second, vary *much*, it must be "at the expense of" ω , the earth's velocity of rotation.

Now it seems very precarious to assume as a permanent law of nature, or even as an established existing fact, that the configuration of the ocean under tidal influence may be even roughly likened, as to its attraction on the moon, to a prolate spheroid. A glance at a tide-map will show but little outward resemblance to this figure; and as to theory, the Astronomer Royal warns us (p. 285) that "the amount of elevation of the water depends in a remarkable degree upon other circumstances than the magnitude of the forces. . . . In two parallel canals of different depths acted on by precisely the same forces, there might be high water in one when there was low water in the adjacent part of the other: or there might be elevations and depressions at the same time in both, but their magnitudes might bear any proportions whatever."

But if this common representation has so much substantial truth as to justify us in assuming two wave-crests exactly antipodal to each other, and either equal in elevation or the nearest to the moon not less than the other, we should then have an exceedingly small tangential force (being the *difference* of two small attractions multiplied by the sines of two small and nearly equal angles) in the direction of the moon's motion; and this would

constantly increase the area described per second, or $r^2\omega$. And if this state of things continued for ever, it would seem that the diurnal rotation must be drawn upon. But as the tidal effects would decrease with the increasing distance of the moon, and so the disturbing elevations themselves tend to disappear, and probably also to become less oblique, it is perhaps premature to assert anything on this subject. Whether in the course of this process there would be a loss of *vis viva* in the whole system more than enough to account for the *work done* in pushing the moon away from the centre, would be another question to be solved before Professor Tyndall could draw upon this fund for some infinitesimal supply of heat.

If, then, the continual maintenance of the tides is incompatible with the existence of internal friction without some external renewing force, I conceive the natural and the only conclusion would be, that the tidal fluctuations themselves must be diminishing, and that whether the rotation be also slackening or not. But as I do not see my way to imagining any state of things in which an ocean solicited by external bodies can be without tidal motion, I confess I believe that there is no such incompatibility—that the simple account of the matter is, that the moon's attractive force (which, acting on a world where the coefficient of friction happened to be less, would produce a greater oscillation) spends itself partly on overcoming friction, and partly on producing a constant wave, but one less than is indicated by a theory which neglects friction. I suppose that the sudden addition of a satellite to a planet would get up a tide in spite of frictional resistance. If so, its continual presence can keep it up.

Kitlands, Dorking,
April 23, 1863.

LXXI. *Chemical Notices from Foreign Journals.*
By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 219.]

H. STE.-CLAIRE DEVILLE has made* the following observations on the phenomenon of the dissociation of water.

When even a rapid current of hydrogen is passed through a porous tube and the emergent gas collected, it is found to consist of pure air instead of hydrogen. Hence the hydrogen is dispersed in the atmosphere, and the air is absorbed in the inside of the porous tube in virtue of endosmose.

If this porous tube is fitted, by means of perforated corks,

* *Comptes Rendus*, February 2, 1863.

in a shorter impermeable porcelain tube, an annular space is obtained between the tubes which may be filled with any gas. For this purpose a glass tube is fitted in the cork at each end, by one of which the gas is admitted, and by the other of which it emerges. The porous tube is likewise fitted with tubes, by which a different gas may enter the tube at one end and emerge at the other. The apparatus being thus arranged, if a current of carbonic acid is introduced into the annular space between the two tubes, and a regulated current of hydrogen into the interior of the porous tube, hydrogen gas may be lighted at the tube fitting in the annular space where the carbonic acid would be expected to emerge, while from the porous tube almost pure carbonic acid emerges. Thus in virtue of endosmose these gases have changed place; the experiment is well suited for lecture purposes.

If the apparatus thus arranged be heated in a furnace from 1100° to 1300°C ., it may be used to demonstrate the phenomenon of the spontaneous decomposition of water which Deville has called *dissociation**. In this case the annular space is filled with coarse fragments of biscuit-porcelain. Instead of hydrogen, aqueous vapour is made to arrive in the interior of the porous tube, while carbonic acid is passed into the annular space. The gases are collected in long glass tubes placed in a bath containing caustic potash, to stop all carbonic acid. When the furnace is in full activity, a highly explosive gaseous mixture is collected consisting of hydrogen and oxygen. Here the aqueous vapour is spontaneously decomposed inside the porous tube; the hydrogen traverses the porous diaphragm, which thus, like an ordinary filter, separates it from the oxygen. A considerable quantity of carbonic acid diffuses and mixes with the hydrogen.

The detonating gas is not quite pure: the action of hydrogen on carbonic acid produces a certain quantity of carbonic oxide. Notwithstanding all possible precautions, too, a certain quantity of hydrogen always escapes, and the detonating gas always contains an excess of oxygen. It further contains some nitrogen, which is introduced along with carbonic acid.

The carbonic acid determines in the operation the separation of the gases by endosmose, but it may also act mechanically. Water alone, heated in a porcelain tube even to almost the fusing-point of platinum, emerges entirely unchanged, and is not decomposed to any appreciable extent.

To explain these facts, Deville enters into the following considerations. The temperature of the combustion of hydrogen and oxygen does certainly not exceed 2500°C . At this point the gases occupy a volume ten times that which they occupy at

* Phil. Mag. vol. xx. p. 448.

0°: it is the limit beyond which water is entirely decomposed. But this decomposition is accompanied, as will be seen, by a considerable absorption of latent heat, which is necessary to maintain the molecules of hydrogen and oxygen at a greater distance than the radius of the sphere of their affinity. The phenomenon is analogous to that of the ebullition of liquids, in taking place at the same temperature, whatever be that of the source of heat. In short, water cannot resist the action of a temperature which decuples its volume at 0°, and then it decomposes, while its elements absorb latent heat, which he calls latent heat of decomposition, the amount and existence of which may be readily calculated.

According to Clausius, the specific heat of a body does not vary with the temperature. The quantity of heat produced in the formation of a gramme of water, from Favre and Silbermann's determinations, is 3883 thermal units. Now the quantity of heat absorbed by a gramme of water in passing from 0° to 2500° is given by the formula

$$637 + (2500 - 100)0.475 = 1680,$$

in which 637 is the quantity of heat needed to transform 1 gramme of water into vapour at 100°, while $(2500 - 100)0.475$ is the heat needed to raise this vapour from 100° to 2500°. The difference between 3883 and 1680 = 2153 thermal units represents the latent heat of the decomposition of water, the heat absorbed by its elements at the moment of their separation.

The comparison between the effects of cohesion and affinity is maintained in the inverse phenomena, volatilization and decomposition. Assuming this relation, the decomposition of bodies at a relatively low temperature, or the phenomenon of dissociation, corresponds to the vaporization of a liquid at a temperature below its boiling-point, and the quantity of the body decomposed will be proportional to its tension of dissociation expressed in millimetres of mercury, as the quantity of vapour formed above a liquid is proportional to the maximum tension of its vapour.

A liquid has no tension in its own vapour, and the quantity of water vaporized in a closed space compared with the volume of water is very small. In like manner the quantity of vapour dissociated at 1200° in a porcelain balloon is so small that the density of vapour is not affected by it.

Of water placed in a closed vessel of small volume, the quantity vaporized is very small, the tension of the liquid being annulled as soon as the space is saturated; but if a fragment of chloride of calcium is introduced, the water will evaporate until this is liquefied, the tension always remaining the same. This

is the part which silver and oxide of lead play in aqueous vapour dissociated at 1000° . They absorb the oxygen; and if simultaneously the hydrogen were removed, the decomposition would continue until the complete saturation of the auxiliary bodies.

In heating aqueous vapour as described in this memoir, an effect is produced analogous to that of exposing a volatile liquid to a current of gas, a vessel full of water in a current of dry air. In the above experiment the carbonic acid carries away, while the porous tube separates, the quantities of oxygen and hydrogen which the tension of the dissociation of aqueous vapour at this temperature produces.

According to Wöhler*, when a mixture of chloride of calcium, silicofluoride of sodium, and sodium are melted together, a compound of calcium with silicium is obtained. This silicide of calcium forms small cylindrical prisms which have the appearance of graphite, and are of an iron-black colour with a semi-metallic lustre. These prisms, like some crystals of mica, may be split into small round discs. The body is unaltered by air or water, but is violently attacked by hydrochloric acid with a copious disengagement of hydrogen, and is changed into a yellow substance without losing the form of the disc.

The body thus formed appears to be a new oxide of silicon, and has the following properties. It is of an intense sulphur-yellow colour, and consists of small transparent laminae: when moist, it gradually becomes white in the air. Gently heated in the air, it ignites, and burns with a luminous flame, leaving silicic acid, which is coloured by amorphous silicon. Heated in a tube, it disengages spontaneously inflammable siliciuretted hydrogen gas, and leaves a mixture of silica and amorphous silicon in dark-brown lustrous laminae. Treated with dilute ammonia, it is converted into gelatinous silica, hydrogen being copiously disengaged. With strong ammonia it ignites. Soda acts in the same manner, while fluoric acid is without action.

Hitherto the analyses have not given conclusive results, which arises from the difficulty of obtaining the body quite pure.

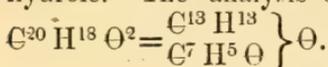
Linnemann has investigated† the action of nascent hydrogen on benzophenone, the ketone of benzoic acid. The material was prepared by distilling benzoate of lime with lime, and the part distilling between 290° and 325° taken for experiment. The action of sodium-amalgam upon it is brisk, but little hydrogen is liberated. After some time the soda formed diminishes the energy of the action; and this is removed by neutralization with sulphuric acid and filtration from sulphate of soda, which is first

* Liebig's *Annalen*, February 1863.

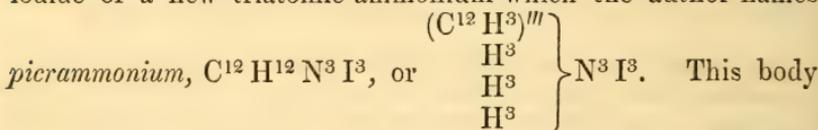
† *Ibid.*

washed with alcohol, then with ether. These united washings are mixed with water until turbidity sets in; the liquid is then agitated with ether; on evaporating the ether, the substance remains free from soda. This is again dissolved in aqueous alcohol, and treated with sodium-amalgam until free hydrogen is disengaged. The substance is finally exhausted with ether, shaken with water to remove alcohol, and finally evaporated spontaneously. A yellowish liquid is left which solidifies on lengthened standing, or immediately if touched. This is finally crystallized from benzole, from which it is deposited in white silky needles. The composition of the substance, which Linnemann provisionally calls *benzhydrole*, is $\text{C}^{13} \text{H}^{14} \Theta$; it is formed from benzophenone, $\text{C}^{13} \text{H}^{10} \Theta$, by the assimilation of four atoms of hydrogen.

It is insoluble in cold water, but completely soluble in alcohol, ether, and benzole. It melts at $67^{\circ}.5$, and it distils at 296° to 297° without decomposition. As far as it has yet been investigated, it appears to be a monatomic alcohol. By being heated with benzoic acid, it forms a compound which can be obtained crystallized in small colourless transparent cubes which melt at 80° , and are decomposed by potash into benzoic acid and benzhydrole. The analysis of this compound led to the formula



Lautemann has examined* the action of iodide of phosphorus on picric acid. When iodide of phosphorus is added to a strong aqueous solution of picric acid, a violent reaction is set up, which raises the liquid to ebullition; when almost all iodide of phosphorus is decomposed and phosphuretted hydrogen begins to be disengaged, carbonic acid is passed through the liquid, which drives off water and excess of hydriodic acid. The liquid, when adequately concentrated in this manner, solidifies, on cooling, to a mass of long needles. These constitute the triiodide of a new triatomic ammonium which the author names



cannot be crystallized from water; for though excessively soluble in that medium, on evaporating the solution it is decomposed. It cannot resist the feeblest oxidizing action, and even by being kept it is decomposed. Hence all attempts to isolate either the hydrated oxide of picrammonium or its triamine have failed.

* Liebig's *Annalen*, January 1863.

The author has described the sulphate, the acid sulphate, and the acid phosphate, all of which salts crystallize well.

Lautemann has found* that, by the action of hydriodic acid, kinic acid, $C^{14}H^{12}O^{12}$, is reduced to benzoic acid, $C^{14}H^6O^4$; the action succeeds both by heating an aqueous solution of hydriodic acid with kinic acid, and by heating an aqueous solution of kinic acid with iodide of phosphorus. In the latter case the quantity of benzoic acid obtained is almost that required theoretically.

The author has also found that kinic acid is reduced to benzoic acid when taken into the organism.

According to Millon and Commaille†, when a solution of ammoniacal subchloride of copper is added to a solution of nitrate of silver, also containing ammonia, an instantaneous precipitate of absolutely pure metallic silver is obtained. The precipitated silver is amorphous, and in the very finest state of division, while that obtained by electric currents or by metals is always crystalline and generally lustrous. The amorphous silver is of a light grey, but sometimes almost white; under the burnisher it takes the brightest metallic lustre, and in virtue of its fine state of division it can be applied to the most varied materials, such as wood, stone, leather, &c.

By the weight of silver precipitated, the quantity of suboxide of copper engaged in the reaction is at once determined; the presence of protosalt does not interfere with the reaction, which thus gives at once an accurate method of analysing a mixture of proto- and sub-salt of copper.

The authors find that the chloride of silver in ammoniacal solution is precipitated by this method, which is thus very well suited for working up laboratory residues. They also suggest its application on the large scale to the extraction of silver from argentiferous minerals.

Kolbe and Lautemann found‡ that when sodium is dissolved in phenylic alcohol, $C^{12}H^6O^2$, in an atmosphere of carbonic acid, salicylic acid, $C^{14}H^6O^6$, is formed; and by similar treatment the homologous cresylic alcohol, $C^{14}H^8O^2$, and thymylic alcohol, $C^{20}H^{14}O^2$, are resolved into new acids, cresotic acid, $C^{16}H^8O^6$, and thymotic acid, $C^{22}H^{14}O^6$, which are homologous with salicylic acid. Scheuch has investigated§ in this direction the deportment of eugenic acid, the main constituent of oil of cloves. This

* Liebig's *Annalen* February 1863.

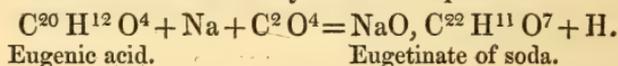
† *Comptes Rendus*, February 16, 1863.

‡ *Phil. Mag.* vol. xx. p. 384.

§ Liebig's *Annalen*, January 1863.

substance has the formula $C^{20}H^{12}O^4$; it contains two atoms of hydrogen less, and two atoms of oxygen more than thymylic alcohol. The eugenic acid was prepared by boiling oil of cloves with caustic soda to expel hydrocarbons, precipitation with hydrochloric acid, and rectification of the oil thus separated. Scheuch found that the eugenic acid thus prepared contained salicylic acid, from which it was separated by treatment with ammonia.

About 50 grammes of the purified oil were placed in a retort and a rapid current of carbonic acid passed through it, while about 3 grammes of sodium were added; the mixture was finally somewhat heated to complete the reaction. The product solidified on subsequent cooling to a mass consisting of undecomposed eugenic acid, the soda-salt of a new acid, *eugetinic acid*, $NaO, C^{22}H^{11}O^7$, and of the soda-salt of an isomeric acid analogous in its composition to carbovinic acid. This is treated with hydrochloric acid, which liberates eugenic acid, and decomposes the carboeugenic acid into carbonic acid and eugenic acid, chloride of sodium being formed. The eugenic acid separates as oil and is removed, while the eugetinic acid remains in solution. On treating this liquid with carbonate of ammonia, then adding hydrochloric acid, shaking with ether, the eugetinic acid is dissolved out. On subsequently evaporating the ethereal solution and crystallizing the residue from hot water, the new acid is obtained in long, thin, colourless prisms, which melt at $124^\circ C$. The formation of the acid may be thus expressed:—



By heating Arragonite in an iron crucible made as air-tight as possible, and by heating lithographic stone or chalk in a porcelain vessel with a ground stopper, G. Rose*, in conjunction with Dr. Siemens, has succeeded in producing marble, thus successfully repeating Sir J. Hall's well-known experiment. The marble prepared from Arragonite was especially distinct, and closely resembled Carrara marble.

Wöhler long ago observed that phosphorous acid imparted to the flame of hydrogen a characteristic pale green tint; Dusart has observed that this is the case with phosphorus; and Blondlot on these facts has based methods for the toxicological investigation of phosphorus. Christoffe and Beilstein* have investigated this reaction by means of the spectroscope.

In a flask about a litre in capacity, the neck of which was

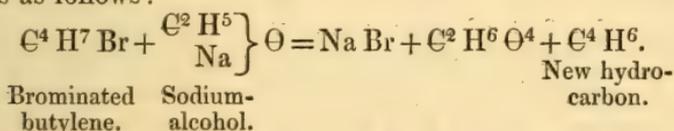
* *Berliner Monatsbericht*, December 1862.

† *Comptes Rendus*, March 2, 1863.

provided with a platinum jet, hydrogen gas was liberated, which was found to produce no effect on the spectroscope. A very minute quantity of phosphorus was then introduced, by which the pale green flame was immediately produced, and on examining this in the spectroscope two magnificent green lines were seen on the left of the sodium, and then a third less visible between the two first and that of the sodium. The same results were obtained with red phosphorus, phosphorous and hypophosphorous acids. The authors took ordinary iron wire, which is considered to contain no phosphorus, and having treated it in the above manner, they obtained the characteristic reaction. Chemically pure iron prepared from the oxalate gave a perfectly colourless flame.

Phosphide of iron, when treated by acid, does not disengage hydrogen; but if this is introduced into a flask in which hydrogen is being disengaged, the flame with its characteristic reaction may be obtained. The case of antimonuret of iron is quite analogous: it does not disengage hydrogen when treated with acids, but in contact with nascent hydrogen it gives off a gas rich in antimony.

Sawitzsch, by the action of amylate of soda on bromide of vinyl, obtained the hydrocarbon C^3H^4 , allylene*. Caventou†, by applying this reaction to the monobrominated butylene, C^4H^7Br , has obtained a new member of the allylene series, C^4H^6 . When brominated butylene is treated with ethylate of soda at the temperature of boiling water, bromide of sodium is formed along with alcohol and the new hydrocarbon. The reaction is as follows:—



This body is liquid below 15° , and it volatilizes with rapidity if it is not kept in ice. It has a powerful, slightly alliaceous odour; it boils at 18° , and distils between 18° and 24° . This new body Caventou names *crotonylene*, to show its relations to crotonic acid, $C^4H^6\Theta^2$, which may be considered as a product of the oxidation of crotonylene. When this hydrocarbon is treated with bromine it enters into combination with it, forming the compound $C^4H^6Br^2$; and by the continued action of excess of bromine this appears to yield another bromide, $C^4H^6Br^4$.

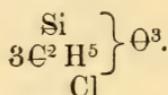
Friedel and Crafts‡ communicate the result of some experi-

* Phil. Mag. vol. xxi. p. 358.

† Comptes Rendus, April 13, 1863.

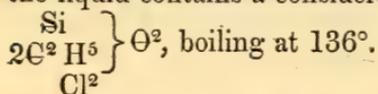
‡ Ibid. March 30, 1863.

ments on new organic compounds of silicon, which confirm the modern views of the atomic weight of this substance. If $H^2 = 2$ volumes, 2 volumes of chloride of silicon contain Cl^4 , or 142 of chlorine combined with 28 of silicon. The formula of chloride of silicon is thus $SiCl^4$, that of anhydrous silicic acid SiO^2 , while the normal hydrated silicic acid would have the formula $\left. \begin{matrix} Si \\ H^4 \end{matrix} \right\} O^4$. When silicic ether is heated with chloride of silicon in closed vessels to a temperature of 160° , a product is obtained which does not fume in the air, and which distils in greatest part between 152° and 158° . A portion of this obtained after several fractional distillations, and whose boiling-point was between 155° and 157° , gave analytical results agreeing with the formula

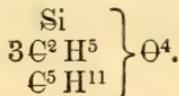


In this substance a quarter of the ethyle and a molecule of oxygen have been replaced by an atom of chlorine, and the body is in reality a monochlorhydrine of silicic ether.

In the product of the above reaction a certain amount of substance was found distilling at a lower temperature and richer in chlorine. Part of this, boiling between 133° and 140° , was analysed; and the results led the authors to the conclusion that the liquid contains a considerable quantity of a dichlorhydrine,



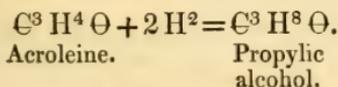
When the first of these compounds is heated with amylic alcohol, hydrochloric acid gas is disengaged, the liquid becomes heated, and distils almost entirely between 205° and 225° . The part distilling between 216° and 225° gave, on analysis, numbers agreeing exactly with the formula



The authors describe the production of silicon-ethyle. When chloride of silicon was mixed with zinc-ethyle no reaction took place in the cold; but in a sealed tube it began at 140° , and at 160° it was complete. On opening the tube, some quantity of gas was liberated, while the liquid residue consisted of chloride of silicon along with a liquid boiling at 153° . This liquid, boiling at 153° , washed with water to remove a small quantity of chloride of silicon, and redistilled, was found to be perfectly

limpid, insoluble in and lighter than water, and unattackable by concentrated solution of potash, or nitric acid. It burns with a luminous flame, evolving white fumes of silicon. The analysis and vapour-density were both found to agree with the formula $\text{Si}(\text{C}^2\text{H}^5)^4$.

Linnemann* communicates the results of a series of experiments on the transformation of bodies belonging to the acryle series into bodies of the fatty acid series. Acroleine, for instance, he converted into propylic alcohol. This was effected by adding an aqueous solution of acroleine containing free acroleine to sodium-amalgam diluted with mercury. No disengagement of gas took place, but a smell of acrylic alcohol was perceived, which ultimately, however, acquired greater similarity to that of amylic alcohol. The alkaline liquid from this first treatment was then treated several times successively with fresh amalgam, and the resultant liquid mixed with chloride of calcium and distilled. To this distillate chloride of calcium was added, on which a supernatant liquid, amounting to about one-tenth of the acroleine used, was separated. This was repeatedly rectified over chloride of calcium and then fractionally distilled. About two-thirds distilled over between 86° and 90° , and the rest between 90° and 100° . Of the fraction between 86° and 90° the boiling-point was found to be constant at 87° to 88° . This had the composition of propylic alcohol, and the boiling-point is also the same as that found by Friedel for propylic alcohol prepared from acetone†. The fraction distilling between 90° and 99° was found to have a constant boiling-point at 96° to 98° , which is that of propylic alcohol obtained from fermentation, and its composition is also the same. Linnemann thinks that there are two isomeric propylic alcohols whose boiling-points differ by about 10° , and both of which are formed by the treatment of acroleine with sodium-amalgam. Their formation takes place by the assimilation of two molecules of hydrogen,



The same chemist has also succeeded in transforming acrylic acid, $\text{C}^3\text{H}^4\Theta^2$, into propionic acid, $\text{C}^3\text{H}^6\Theta^2$, by the action of sodium-amalgam, that is, by means of hydrogen in the nascent state.

* Liebig's *Annalen*, March 1863.

† Phil. Mag. vol. xxiv. p. 309.

LXXII. *Another treatment of the Equations, which conduct to the Skew Surface of Mr. Cayley in the Supplementary Number to Vol. 24 of this Journal. By Dr. ADOLPH STEEN, Professor at the University of Copenhagen*.*

IN searching for the equation of the skew surface generated by a line which passes through three directors, viz. the plane cubic

$$(\alpha^3 + \beta^3)xy - (x^3 + y^3)\alpha\beta = 0,$$

and the two lines

$$(x - mz = 0, \quad y - nz = 0),$$

$$(x - \alpha = 0, \quad y - \beta = 0),$$

Mr. Cayley arrived at the following system of equations:—

$$(\alpha^3 + \beta^3)xy - (x^3 + y^3)\alpha\beta = 0,$$

$$X = x + AZ,$$

$$Y = y + BZ,$$

$$(n - B)x - (m - A)y = 0,$$

$$B(x - \alpha) - A(y - \beta) = 0.$$

Eliminating A, B, x, y, he obtains thence the equation of the surface in a most beautiful and ingenious manner, in which, however, I missed some of that sublime simplicity so frequently met with even in transcendental researches. After some trials I succeeded in finding a very plain and elementary solution of the problem.

We first eliminate x and y in the three first equations, whence

$$(\alpha^3 + \beta^3)(X - AZ)(Y - BZ) - \alpha\beta((X - AZ)^3 + (Y - BZ)^3) = 0.$$

The same values of x and y put in the last two equations give

$$(n - B)(X - AZ) - (m - A)(Y - BZ) = 0,$$

$$B(X - AZ - \alpha) - A(Y - BZ - \beta) = 0;$$

or, with a slight transformation,

$$(Y - nZ)A - (X - mZ)B = mY - nX,$$

$$(Y - \beta)A - (X - \alpha)B = 0;$$

that is, two equations of the first degree to determine A and B. Thence we find

$$\begin{aligned} \frac{A}{X - \alpha} &= \frac{B}{Y - \beta} = \frac{(Y - nZ)A}{(X - \alpha)(Y - nZ)} = \frac{(X - mZ)B}{(Y - \beta)(X - mZ)} \\ &= \frac{mY - nX}{(X - \alpha)(Y - nZ) - (Y - \beta)(X - mZ)}; \end{aligned}$$

* Communicated by the Author.

and introducing in the numerator of the last fraction $Y - nZ$ and $X - mZ$ for X and Y , we obtain

$$\frac{A}{X - \alpha} = \frac{B}{Y - \beta} = \frac{m(Y - nZ) - n(X - mZ)}{(X - \alpha)(Y - nZ) - (Y - \beta)(X - mZ)},$$

which give the following values of $X - AZ$ and $Y - BZ$, viz.

$$X - AZ = \frac{(X - mZ)[(X - \alpha)(Y - nZ) - X(Y - nZ) - \beta X + \alpha nZ]}{(X - \alpha)(Y - nZ) - (Y - \beta)(X - mZ)}$$

$$= \frac{(X - mZ)(\beta X - \alpha Y)}{(X - \alpha)(Y - nZ) - (Y - \beta)(X - mZ)},$$

$$Y - BZ = \frac{(Y - nZ)(\beta X - \alpha Y)}{(X - \alpha)(Y - nZ) - (Y - \beta)(X - mZ)}.$$

Substituting these values in a former equation, we find

$$(\alpha^3 + \beta^3)(X - mZ)(Y - nZ)(\beta X - \alpha Y)^2[(X - \alpha)(Y - nZ) - (Y - \beta)(X - mZ)] - \alpha\beta[(X - mZ)^3 + (Y - nZ)^3](\beta X - \alpha Y)^3 = 0,$$

or, by division with $(\beta X - \alpha Y)^2$,

$$\left. \begin{aligned} (\alpha^3 + \beta^3)(X - mZ)(Y - nZ)[(X - \alpha)(Y - nZ) - (Y - \beta)(X - mZ)] \\ - \alpha\beta[(X - mZ)^3 + (Y - nZ)^3](\beta X - \alpha Y) \end{aligned} \right\} = 0.$$

Here the last factor can be altered thus,

$$\beta X - \alpha Y = \beta(X - \alpha) - \alpha(Y - \beta),$$

and thereby the equation put under the form

$$\left. \begin{aligned} \alpha(X - \alpha)(X - mZ)[\alpha^2(Y - nZ)^2 - \beta^2(X - mZ)^2] \\ - \beta^2(X - \alpha)(Y - nZ)^2[\alpha(Y - nZ) - \beta(X - mZ)] \\ + \beta(Y - \beta)(Y - nZ)[\alpha^2(Y - nZ)^2 - \beta^2(X - mZ)^2] \\ - \alpha^2(Y - \beta)(X - mZ)^2[\alpha(Y - nZ) - \beta(X - mZ)] \end{aligned} \right\} = 0.$$

This form puts in evidence the extraneous factor

$$\alpha(Y - nZ) - \beta(X - mZ),$$

which, equated to zero, is the plane through the node and one of the directing lines. Omitting this factor, we have the equation of the surface in a new form,

$$\left. \begin{aligned} [\alpha(Y - nZ) + \beta(X - mZ)][\alpha(X - \alpha)(X - mZ) + \beta(Y - \beta)(Y - nZ)] \\ - \beta^2(X - \alpha)(Y - nZ)^2 - \alpha^2(Y - \beta)(X - mZ)^2 \end{aligned} \right\} = 0.$$

Carrying out the multiplications, we find

$$\left. \begin{aligned} [\alpha^2(X - \alpha) + \beta^2(Y - \beta)](X - mZ)(Y - nZ) \\ + \alpha\beta(X - \alpha)(X - mZ)^2 + \alpha\beta(Y - \beta)(Y - nZ)^2 \\ - \beta^2(X - \alpha)(Y - nZ)^2 - \alpha^2(Y - \beta)(X - mZ)^2 \end{aligned} \right\} = 0;$$

or, the last terms being contracted into one,

$$\left. \begin{aligned} & [\alpha^2(X-\alpha) + \beta^2(Y-\beta)](X-mZ)(Y-nZ) \\ & + [\beta(X-\alpha) - \alpha(Y-\beta)][\alpha(X-mZ)^2 - \beta(Y-nZ)^2] \end{aligned} \right\} = 0.$$

This is the very form of the equation given by Mr. Cayley, and putting in evidence, as he remarks, both the directing lines, as also the cubic curve, if $Z=0$.

This solution will, I hope, in some measure serve to increase the interest of this investigation as illustrating the theory of skew surfaces in general, and making it more accessible to scholars.

LXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 490.]

June 19, 1862.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

“On the Relative Speed of the Electric Wave through Submarine Cables of different lengths, and a unit of Speed for comparing Electric Cables by bisecting the Electric Wave.” By Cromwell F. Varley, Esq.

The present paper gives the results of some experiments which were undertaken to determine, first, the relative speed of the electric wave through cables of various lengths; secondly, the retarding effect of the iron covering of the cable; and thirdly, methods for the increase of the speed of the electric wave.

When a long submarine cable or subterranean wire is connected at one end through a galvanometer to the earth, and the other end is connected with a battery, a current flows through it, deflecting the galvanometer-needle.

If the needle be made very light and small, so as to have but a small amount of inertia, and the cable be long, the current will be seen to arrive after the lapse of a short but appreciable interval of time, and will gradually augment in intensity approaching to, but never attaining, the maximum.

Professor Thomson has investigated this subject mathematically, and arrived at the conclusion that in submarine cables of different lengths the speed is inversely as the square of the distance.

Through the Atlantic Cable, the conducting wire of which weighed 93 lbs. to the statute mile, and the length of which was rather more than 2300 statute miles, the electric current did not show itself on Thomson's sensitive reflecting galvanometer until more than one second after contact had been made with the battery at the other end.

In experiments made by the author in 1854 upon 1600 miles of wire between London and Manchester, connected up in one continuous circuit, the current was not visible upon the chemical recording instruments then in use until after the lapse of about three seconds.

These experiments were repeated by Professor Faraday; and he has made known the results.

From the imponderable nature of electricity (considered for a moment as a fluid), from its incompressibility, and other circumstances, the author infers that the electric current commences flowing out at the one end at the very instant that contact is made with the battery at the other end; but it is a considerable time before it reaches an appreciable strength; it then goes on augmenting in strength, approaching to, but never absolutely attaining, its maximum force.

As the first part of the wave commences to appear instantly, and as the top of the wave would require an indefinitely long period of time to be reached, it will be evident that the part of the wave best suited for investigation is half the maximum, as at that period the changes of its intensity in a given time are more rapid than at any other.

When attempting to measure by means of a galvanometer the arrival of the wave at half its maximum, the weight and momentum of the magnet of the galvanometer were found to interfere so much that, excepting through very long lengths of cable, nothing approaching to an accurate determination of the speed could be obtained. The use of electro-magnets was equally, if not more, objectionable, as they require a very appreciable but uncertain time to be magnetized.

The following method, however, of *bisecting the electric wave* has obviated these difficulties, and admits of the determination of the relative rates of transmission through cables of different lengths with very great accuracy.

The machine used consisted of an axle carrying two "commutators." This axle was driven by clockwork, governed, in one case, by a fly rotating in mercury, and in the second experiment by means of a fly in the air, together with a friction spring.

The commutator consists of two wheels, each wheel being in two halves. Upon the broad edge of the wheel rest two springs, one of which is connected with one pole of the battery, and the other with the other pole. The two halves of the wheel were constantly connected, by means of two other springs, the one with the cable wire, and the other with the earth, so that when the wheel was turned round, during one half of the revolution a positive current was flowing through the cable wire, and during the other a negative. The other commutator on the same axle was precisely similar in construction; but the two springs resting on the edge of the wheel were connected with two wires of a galvanometer, and one half of the wheel was connected with the receiving end of the cable wire tested, and the other half of the commutator was connected with the earth.

The two commutators were so arranged that when, by the rotation of the wheel, the current of electricity from the battery was reversed, the connexions of the galvanometer were reversed also; and therefore, if the speed of the electric wave through the cable were indefinitely great, the currents would flow through the galvanometer in one direction, no matter how fast the currents in the cable are reversed. As, however, a given amount of time elapses before these waves reach their maximum at the distant end of the circuit, and

as also a given time elapses after the battery has been reversed at the one end before the current is reversed at the other or distant end, it is clear that by gradually augmenting the rate of rotation of the commutator until the wheel is a quarter of a revolution in advance of the wave, a point is arrived at when the galvanometer's connexions are reversed precisely at the moment that the wave reaches its maximum strength, and consequently the wave is bisected, one half of it flowing through the galvanometer in one direction, and the other half in the other. At this rate of rotation the galvanometer falls to zero; because, the wave being exactly bisected, the one half tends to deflect the needle to the right, and the other to the left, but, owing to the weight of the needle and the rapidity of the reversals, it (the needle) stands nearly steadily at zero. The galvanometer used consisted of a rather heavy astatic pair of needles suspended by a silk fibre. The wire acting upon the needles was about the twentieth part of an inch in thickness, in order that it should offer no serious resistance to the electric current. Its resistance was less than one Varley unit (1 mile copper wire $\frac{1}{16}$ inch in diameter).

The rate of rotation necessary to obtain the first zero is the point recommended for comparing the relative speeds of the electric waves through submarine cables of different dimensions.

By augmenting the speed beyond that necessary to produce the first zero, the needle becomes deflected in the opposite direction and gradually approaches a maximum; that is to say, when the electric wave is half a revolution behind, the currents all flow through the galvanometer in one direction again. This is termed the second maximum (the first maximum being that obtained when the wheel is not rotating at all); and by augmenting the speed still more, until the wave is three-quarters of a revolution behind, the wave is again bisected and a second zero is obtained, and so on.

The great variation of speed necessary to give these and other results was such that the means then at the author's disposal in the first experiments were not sufficiently regular to admit of very accurate readings.

The experiments now communicated were made upon two cables, one containing six conducting wires, a portion of which was laid in the Mediterranean. This cable had been lying exposed to sun and weather in the East India Docks for some years, and the gutta percha had become deteriorated to a considerable extent; its exact length was not known; and from these combined causes it could not be used for determining the rate at which the wave travels through given lengths, but it has served to demonstrate that Thomson's "law of the squares" is substantially correct in practice.

In the experiments made on this cable, the resistance of the galvanometer was equal to one mile of the cable. The battery power used averaged from 12 to 36 cells of Daniell's battery, each cell offering a resistance of one-sixth of a mile of the cable.

The first experiment was made upon two wires forming a loop of about 150 miles in length; and when the currents were reversed at the rate of 15.16 per second, the needle came to zero.

The second experiment was made through three wires, that is to say, 225 miles of cable. The speed then obtained was 6.57. Through four wires (*i. e.* double the length of first experiment) 3.78 reversals per second were obtained.

Through six wires, or three times the length of the first experiment, 1.75 per second were obtained, or inversely as the square of the length.

In the foregoing experiments the current was made to pass up one wire and down the second, up the third and down the fourth, and so on; but in experiment No. 5, the current was made to pass through all the six wires, one after the other, in the same direction, the object being to determine, if possible, what amount of retardation was attributable to the magnetization of the iron covering. On the current through the first wire ceasing, a magneto-electric current is produced in the opposite direction to the first magneto-electric current; and consequently, when the wires were so connected that the current went up one wire and down the second, up the third and down the fourth, as in experiment No. 4, the magneto-electric action upon No. 2 wire is counterbalanced by the magneto-electric action upon No. 3, and so on; but in experiment No. 5 the magneto-electric current was in full force on all the wires. The result, however, did not show any appreciable difference in the speed of the wave, as the machine then used could not be governed with sufficient accuracy.

Experiments were made to determine the effect of applying resistance to one end of the cable. For instance, a telegraphic instrument, when applied to the cable, augments the resistance of the circuit; and when a resistance equal to half that of the cable was applied at one end, the rate of the electric wave through it was decreased to three-quarters. When a resistance equal to the whole of the cable was added at one end, so as to double the resistance of the whole circuit, the speed was reduced to about three-fifths; and when resistance double that of the cable was added, the speed was reduced rather more than one-half.

Variations in the electromotive force produced no sensible variation in the speed of the waves.

The second series of experiments were tried upon the Dunwich and Zandvoort cable, after it was submerged, and consequently in a straight line, and not, as in the previous experiment, in a coiled mass; it was therefore exposed to much less magneto-electric induction. The insulation of this cable was very high indeed.

The experiments on this cable, among other results, show that doubling the length of the circuit reduced the speed nearly four times. The experiments on the Mediterranean cable showed that with three times the length, the speed was reduced nearly nine times. With twice the length the speed was reduced nearly four times, or inversely as the square of the distance nearly.

The mean of the experiments through 270 miles of cable are 4.76 revolutions of the wheel per second, or 9.52 reversals of the current per second.

In the experiments through 540 miles, or twice the length of

cable, the speed was 1·326 revolution of the wheel, or 2·65 reversals per second. The reason why they do not follow the law of the squares exactly, is probably to be attributed to the resistance of the battery used on this occasion, and also to the fact of the magneto-electric induction of the iron exterior.

Experiment 9 shows that, on the introduction midway in the circuit of an escape (circuit dérivé), the resistance of which is equal to half the circuit, the first zero is obtained at the rate of 2·78 revolutions per second, or 5·56 reversals per second; the introduction of this escape about doubles the speed of transmission; and thus, by the establishment of a series of escapes judiciously along the cable, the speed may be augmented to a very high degree without weakening the current too much for the purposes of telegraphy. Experiments were then tried with currents of various durations; and the results of these experiments are very important, the highest speed being obtained when the cable was connected to the battery for a very short interval of time and immediately afterwards put to earth. In this way, through the 540 miles, the speed of the wave was increased from 1·326 to 3·7.

In the experiments in which resistances of various amounts were added to one end of the cable, the consequent retardations agree very nearly with the results obtained upon the Mediterranean cable. It was found to be immaterial at which end of the circuit the resistance was added: this, however, can only hold good with highly insulated wires; for it is evident, upon a little consideration of the matter, that, where the line is imperfectly insulated, the resistance added at the sending end will produce more retardation than if applied to the receiving end.

In the experiments on the second zero and second maximum, it is shown that, if the speed required to produce the first zero be taken as unity, double that speed is necessary to produce the second maximum, and four times the speed to get the second zero.

Notwithstanding the difficulties under which these experiments were made (from the necessity of using a machine the rates of whose motion could not be very accurately governed), the results are still sufficiently accurate for all "*practical*" purposes of submarine telegraphy; but such nice points as the retarding influence of the iron covering cannot be inferred with any precision from these experiments. It is certain, however, that in long cables the retarding influence of the external iron covering is so small, compared with the retardation due to electrical induction, that it may be neglected in estimating the speed of the electric wave.

GEOLOGICAL SOCIETY.

[Continued from p. 410.]

March 4, 1863.—Leonard Horner, Esq., V.P., in the Chair.

The following communication was read:—

"On the Permian Rocks of North-eastern Bohemia." By Sir Roderick I. Murchison, K.C.B., F.R.S., F.G.S. &c.

The author, accompanied by Dr. Anton Fritsch, of Prague, made

a transverse section of the rocks exposed by railroad-cuttings between Josefstadt on the S.S.E. and Semil on the N.N.W. These rocks, simply termed Roth-todt-liegende by the Austrian and Saxon geologists, are, however, of very varied mineral characters and of very considerable dimensions. They consist, in ascending order, of, 1st, coarse conglomerate and sandstone, followed by thin courses of schist, with fishes (*Palæonisci*, &c.), and interstratified igneous rocks (basaltic clinkstone, porphyry, &c.); 2nd, alternations of coarse grits and sandstone, with large *Araucarites* and other plants; and 3rd, of bituminous schists, in parts containing coal, with some layers of limestone, copper-slate, &c., and many fossil fishes in bituminous flagstone passing up into red-and-green-spotted sandstones and marls.

This series of rocks, though subject to local undulations, assumes at Liebstadt a steady dip to the S.E., or away from the Riesengebirge; this is well seen on the railway between Liebstadt on the S.E. and Semil on the N.W., which section was described by the author in detail. The igneous rocks, chiefly amygdaloids and porphyries (Melaphyr), occur at various horizons in the series, and are supposed to have been for the most part of contemporaneous formation with the regular aqueous sediments.

Alluding to the animal remains, as enumerated by Geinitz, the author stated that he was disposed to view the group as having chiefly an estuarine character, the various sauroid fishes and the coarse conglomerates leading to that inference; at the same time he admits that portions of it were probably freshwater and terrestrial accumulations. After pointing out the chief localities of the large fossil stems of the *Araucarites* and other plants, allusion was made to the opinion of Göppert and Geinitz, that the fauna of this group is, as a whole, distinct from that of the carboniferous age. He shows that the thickness of the whole of these rocks in Northern Bohemia is very considerable, as (at Erlbach) in the adjacent country of Saxony, the inferior half only of these deposits, or the lower Roth-liegende, has actually been sunk through by a shaft, in search of coal, to the depth of 2300 feet, as brought to his notice by Professor Keilhau.

In referring to the general relations of these rocks, he suggests that, as they vary very considerably in different regions, they are best defined by the word Permian, which, according to its original definition by himself and his associates in Russia, simply means that such rocks lie between the upper coal, on which they rest unconformably, and the lowest portion of the Trias, by which they are covered.

It was observed that, in proceeding from north to south (in Eastern Germany), the Zechstein thins out; and seeing the vast dimensions which the group assumes where true Zechstein is no longer traceable, the author suggests that some of the higher members of the Bohemian Roth-liegende may represent that limestone *in time*. The term Dyas, recently applied to the whole Permian group by Geinitz, is objected to, since it is based on the theory that the lower portion of the Permian is exclusively of freshwater origin, as con-

trasted with the superjacent marine Zechstein, and also because the geographical term Permian, involving no theory, had previously been widely adopted, and even used by Geinitz himself.

Sir Roderick having expressed his great obligations to Dr. Geinitz, to whose excellent work ('Dyas') he made many references, and to the name of which only he objected, concluded by presenting to the Society a very large collection of rock specimens of the Lower Permian of Saxony.

March 18.—John Carrick Moore, Esq., F.G.S., in the Chair.

The following communications were read:—

1. "On the Correlation of the several Subdivisions of the Inferior Oolite of the Middle and South of England." By Harvey B. Holl, M.D., F.G.S.

The order of succession of the subdivisions of the Inferior Oolite observed in passing from the southern side of the Mendips to the typical section of that formation at Leckhampton, with the lithological characters of the strata, were described in this paper. The classification of the members of the Inferior Oolite employed by Mr. Hull in the Memoirs of the Geological Survey was adopted by the author; and it was shown that in proceeding from Bath northwards the two upper subdivisions may be seen to rise, the Building Freestone at the same time becoming thicker, while at Aveling the Oolite Marl is first seen interposed between the Lower Ragstone and the Lower Freestone, and at Nailsworth the former is separated from the Oolite Marl by the Upper Freestone, all these beds becoming thicker towards Cheltenham, and thinner in the opposite direction, towards Bath. Dr. Holl concluded with some remarks on the strata exhibited in the Rolling Bank Quarry, and on the geographical distribution in England of the members of the Inferior Oolite.

2. "On the occurrence of large quantities of Drifted Wood in the Oxford Clay, near Peterborough." By Henry Porter, M.D., F.G.S.

The Oxford Clay in the neighbourhood of Peterborough having been exposed in clay-pits, the author was enabled to carry on some investigations regarding the fossils which there occur in it; he found the formation to be extremely rich in organic remains, and, besides containing many species of Ammonites and other Mollusca, which he enumerated, to include large quantities of drifted wood, the fragments bearing on their surface the impressions of Ammonites.

3. "On a new Macrurous Crustacean from the Lias of Lyme Regis." By Henry Woodward, Esq., F.Z.S. Communicated by Prof. Morris, F.G.S.

A very perfect specimen of a Crustacean, obtained from the Lias of Lyme Regis by Mr. Harrison of Charmouth, was described in this paper as the type of a new genus. The nearest living analogues were stated to belong to the fossorial group *Thalassinidæ*, from which it differs chiefly in its much less rudimentary abdomen, and the length of its chelæ. Amongst fossil forms, this Crustacean, which

was named *Scapheus ancylochelis*, approached most closely to *Mega-cheirus longimanus*, from the Solenhofen limestone, species of which genus occur also in the Oxford Clay of Wiltshire and the Oolite and Lias of Germany.

April 1.—Professor A. C. Ramsay, President,
in the Chair.

The following communication was read:—

“On recent Changes in the Delta of the Ganges.” By James Fergusson, Esq., F.R.G.S. Communicated by the President.

Before describing the local phenomena of the Ganges, the author explained, first, the laws that govern the extent of the oscillations in reaches of rivers, either laterally or in the direction of their course; secondly, the causes operating to raise the banks of rivers flowing through very flat plains above the level of the country at a little distance from their margins; and lastly, the immense relative thickness of the early deposits in deltas over those of later periods, when the conditions of the river had come more nearly in *equilibrium*.

Mr. Fergusson then proceeded to point out that in historical times the Brahmapootra and Ganges, on entering the plains of Bengal—passing Goalparah and Rajmahal respectively—ran originally to the sea in a nearly due north and south course, parallel to one another. This symmetry was first disturbed by the upheaval of the Modopore jungle, north of Dacca, by which the Brahmapootra was diverted in a south-eastern direction into the depression known as the Sylhet Jheels, which were the result of the upheaval just described. He then further explained how the river, having filled up these Jheels, had returned to its former bed within the limits of the present century.

The paper then described the effect this change had already produced in reopening the rivers of the western half of the Delta, and showed that, if it were maintained, it would have the effect of so raising the eastern half as to restore the course of the two great rivers very nearly to the position they occupied before the disturbance above alluded to.

The next point adverted to was the gradual retrocession of all the mouths of the tributaries of the Ganges, in consequence of the tilting back of the plain, by the gradual rise of the Deltaic plains.

Mr. Fergusson then stated that he conceived we had sufficient historical indications to show that within the last 5000 years the plain of Bengal has been nearly in the same condition that the valley at Assam now is—a jungly swamp, with only a few habitable spots here and there on the banks of the larger rivers.

The last phenomenon alluded to was the “Swatch of low ground” in the Bay of Bengal. This was ascribed to the action of the tides, which, being accelerated on either shore of the bay, acquired a rotatory motion at the sand-heads, and, meeting in the centre of the bay, scooped or swept out this depression in the centre, and by this action prevented the growth of the Delta seaward to the extent that would otherwise take place.

LXXIV. *Intelligence and Miscellaneous Articles.*

ON THE OSCULATING TWISTED CUBIC TO A CURVE OF DOUBLE CURVATURE. BY SIR WILLIAM ROWAN HAMILTON, LL.D., ETC.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT may interest some readers of the *Philosophical Magazine* to know that *one* of the applications which, in my forthcoming work (*Elements of Quaternions*), have been made of my calculus to geometry relates to the determination of the *osculating twisted cubic* (or *gauche curve* of the third degree) to any proposed curve of double curvature.

Another application relates to the *locus of the osculating circle*, which is at the same time the *envelope of the osculating sphere*, to any such curve in space.

If permitted, I shall be happy to give some short *sketch* of each of those *two* investigations, in the *Philosophical Magazine*: and, in the mean time, I have given an outline of *one* of them to the Royal Irish Academy, at their general meeting.

ON THE INDUCTIVE CAPACITY OF INSULATING BODIES.

BY M. GAUGAIN.

The new theory of *induction*, propounded by Mr. Faraday twenty years ago, has given rise to numerous controversies; and it is not generally allowed that the inductive capacity is a special property, distinct from ordinary conductivity. But the problem of the determination of inductive capacities, viewed in the manner I am about to point out, is a purely experimental question, and can be treated quite independently of any theoretical idea.

According to Faraday's views, the inductive capacity consists in the greater or less facility with which neutral electricity is decomposed and recomposed in the interior of the same molecule, while conductivity consists in the greater or less facility with which the electricity of one molecule is transmitted to the adjacent ones. But holding to these notions, it would appear difficult, if not impossible, to distinguish the effects of the inductive capacity from those of ordinary conductivity; and to isolate the first, Mr. Faraday has found no other means than that of rapidly charging and discharging the condensers which he used. But it seemed to me that, by working in this manner, a new principle is virtually introduced, which consists in assuming that the inductive capacity may be completely manifested in an inappreciable time, while conductivity always requires, to develop its effects, more or less of time. Taking this character as a definition, the problem of the determination of the inductive capacities may be stated in precise terms.

As I have pointed out in a previous note (*Phil. Mag.* vol. xxiv. p. 495), the charge of a given condenser put in communication with a source of electricity depends on the time during which contact is established (at all events when the dielectric is a solid body). Hence the charge ought to have a maximum and a minimum value; and the investigation of these two limits constitutes two perfectly

definite problems. But it follows from preceding considerations, that the lower limit of these charges is precisely what is called the *inductive capacity*.

In the above-mentioned note I have occupied myself in determining the higher limit of the charge; in the present one my principal concern is with the lower limit. In both cases I have followed the same method of experimentation. I constructed a series of fulminating panes of the same dimensions with different dielectrics; I charged them successively by putting them for a given time in communication with a source of electricity of constant tension, and in each case measured the quantity of electricity condensed by the inducing armature by what I have called the method of gauging.

Working in this way, I have found that the quantity of electricity condensed diminishes with the duration of the charge, even when only small intervals of time are considered. Thus the following numbers were obtained for the charges of a condenser made of commercial stearic acid, the charge of an air-condenser of the same size being taken as unity:—

Duration of the charge.	Quantity of electricity condensed.
A fraction of a second.....	1·3
2 seconds.....	1·8
40 seconds.....	2·7
Several hours.....	7·0

Hence if the conductibility and inductive capacity are two distinct properties, it will be necessary, in order to eliminate the first, to work more rapidly than I have done. Physicists who have worked on the inductive capacity have not usually noted the time during which their condensers were charged. Faraday merely mentions that he worked rapidly; but I think that at least three or four seconds would be needed to execute the manipulation which his method requires; and it is indubitable that during this interval conductibility may manifest its action, since, in the example cited, the quantity of electricity condensed where the duration of the charge is 2 seconds is four times as much as with a charge whose duration I estimate at $\frac{1}{100}$ th of a second.

I have not yet reduced the duration below this fraction of a second; but I consider it certain that in this small interval of time conductibility may exert a certain action, and I am led to believe that for stearic acid the true value of the lower limit of the charges is unity, which amounts to saying that stearic acid has exactly the same inductive capacity as air.

All the insulating substances I have worked with have not comported themselves like stearic acid, and some of the results obtained appear at first to furnish a decisive argument in favour of Faraday's theory. Thus, with two fulminating panes of the same size, one of sulphur and one of stearic acid, I found that the latter exceeds the former when the duration of the charge is some seconds, but that the former decidedly preponderates over the latter when the duration of the charge is reduced to $\frac{1}{100}$ th of a second. At first sight it seems impossible to explain this inversion without introducing some pro-

perty distinct from conductivity; but yet it is easy to see that the assumption of this new property may be dispensed with, if it be assumed that at least one of the dielectrics offers a certain heterogeneity of composition or of structure. If we conceive, on the one part, a homogeneous dielectric endowed with a feebly, and on the other hand a dielectric composed of a perfectly insulating mass, in the midst of which are fragments of another perfectly conducting substance, it is easy to see that the second dielectric might exceed the first when the duration of the charges was sufficiently small, and that, on the contrary, the homogeneous dielectric might preponderate when the charges are sufficiently prolonged.

This influence of heterogeneity I have been able to demonstrate by means of two cakes of flour of sulphur of the same size, one containing a small quantity of water, and the other a somewhat larger one of olive oil. Compared in the same manner as solid discs, I found that the disc containing water gave a charge almost double that of the other when the duration was only a fraction of a second, but that when the duration was some minutes, the cake impregnated with oil gave the greater charge.

Is it now possible to attribute to sulphur such a heterogeneous structure as I describe? The researches of MM. Regnault, Ch. Deville, and Berthelot have shown us that this body may undergo various modifications; and it appears natural to assume that its electrical properties may vary at the same time as its other properties. With specimens of amorphous sulphur and of octahedral sulphur I constructed pulverulent cakes which I investigated in the usual manner, and I found that the quantity accumulated by the octahedral sulphur-condenser increases with the duration of the charge, and for even very small durations is greater than the quantity accumulated by an air-condenser of the same dimensions. The charge of a condenser constructed of amorphous sulphur does not vary with the time, and is exactly equal to the charge of an air-condenser of the same dimensions.

In short, all my investigations on these subjects lead me to believe that they would all be affected in the same way by electrical influence if they could be completely deprived of their conductivity. Hence I think Mr. Faraday's theory must be modified in one point. According to the views of this illustrious physicist, induction and conduction are only the two extreme terms of the same mode of propagation, which in all cases is effected by the intervention of the material molecules. But it seemed to me quite established by my preceding researches, that the mathematical laws of transmission are the same in the cases of induction and of conduction; but it does not therefore follow that these two modes of propagation take place in the same medium. It appears probable, on the contrary, that electricity, like heat, may be propagated by the intervention of the æther as well as by the intervention of ponderable matter; and I am led to believe that influence or induction takes place by the first of these two ways, while conduction employs the second.—*Comptes Rendus*, April 20, 1863.

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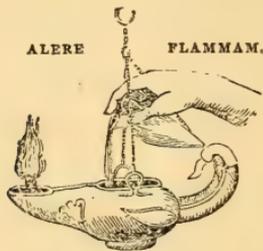
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AUG 21 1863.

INCREASE OF TEMPERATURE DOWNWARDS IN THE EARTH.

$OX = x$

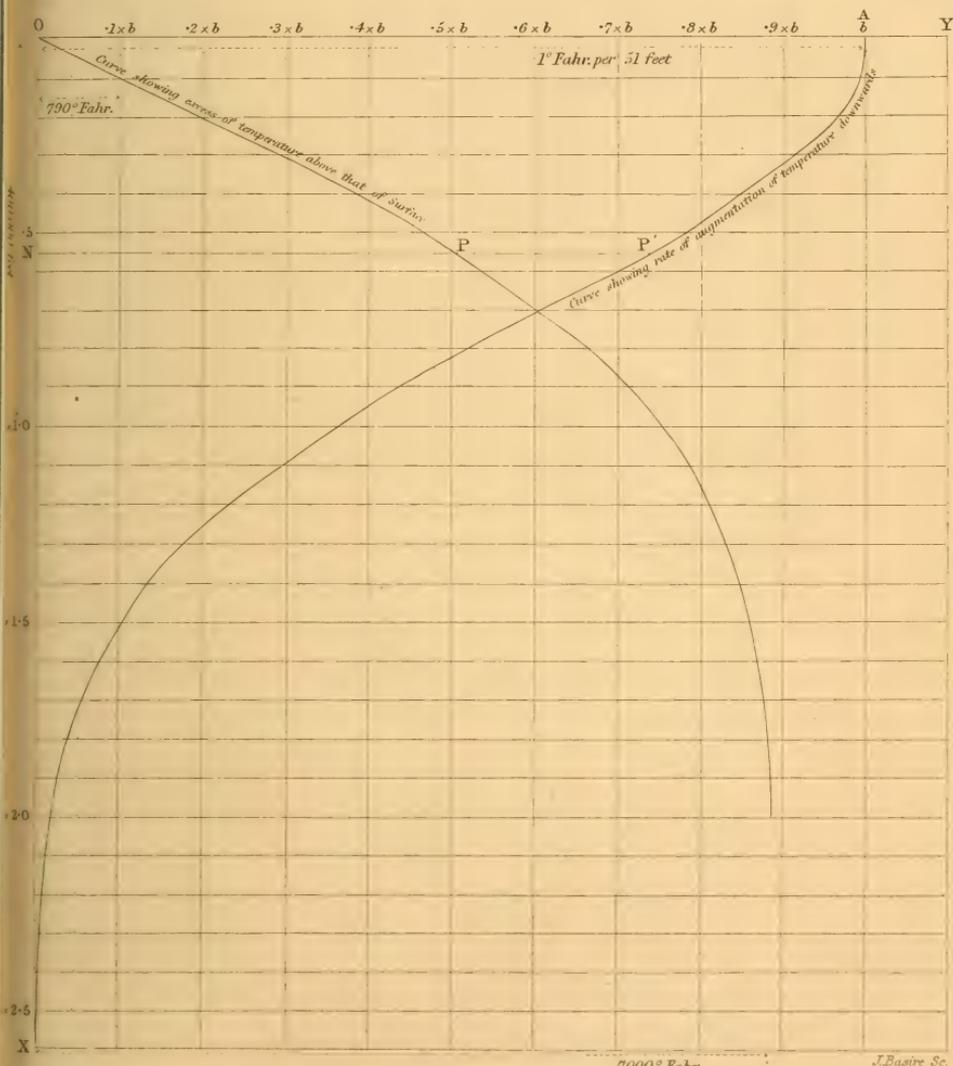
$XP' = b e^{-\frac{x^2}{a^2}} = y'$

$XP = \text{area ONP'A} + a = \frac{1}{a} \int_0^x y' dx$

$a = 2\sqrt{Kt}$

$\frac{dy}{dx} = \frac{V \cdot NP'}{a \cdot b \frac{1}{2} \sqrt{\pi}}$

$v - v_0 = V \cdot \frac{NP}{b \frac{1}{2} \sqrt{\pi}}$



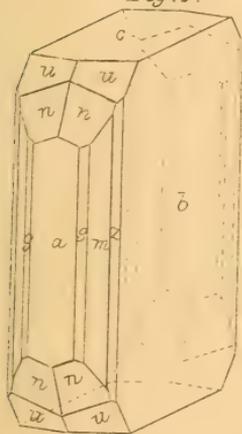
888 x 9 = 8 1/2 sqrt(pi)

J. Basire Sc.



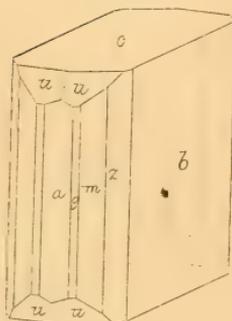
Columbite.

Fig. 3.



Phil. Mag. Ser. 4. Vol. 25. Pl. II.

Fig. 4.



Connellite.

Fig. 1.

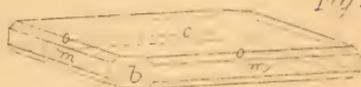


Fig. 2.



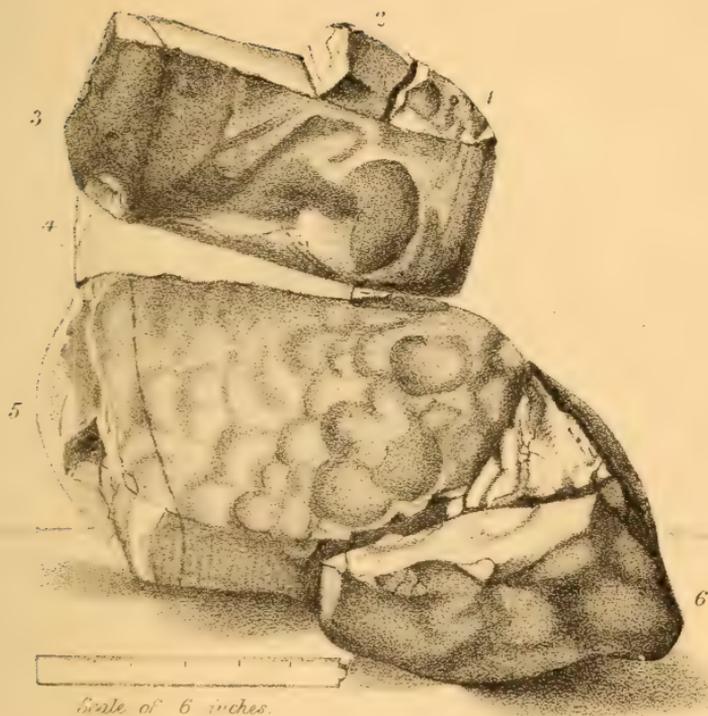
Lanthanite.

Fig. 5.



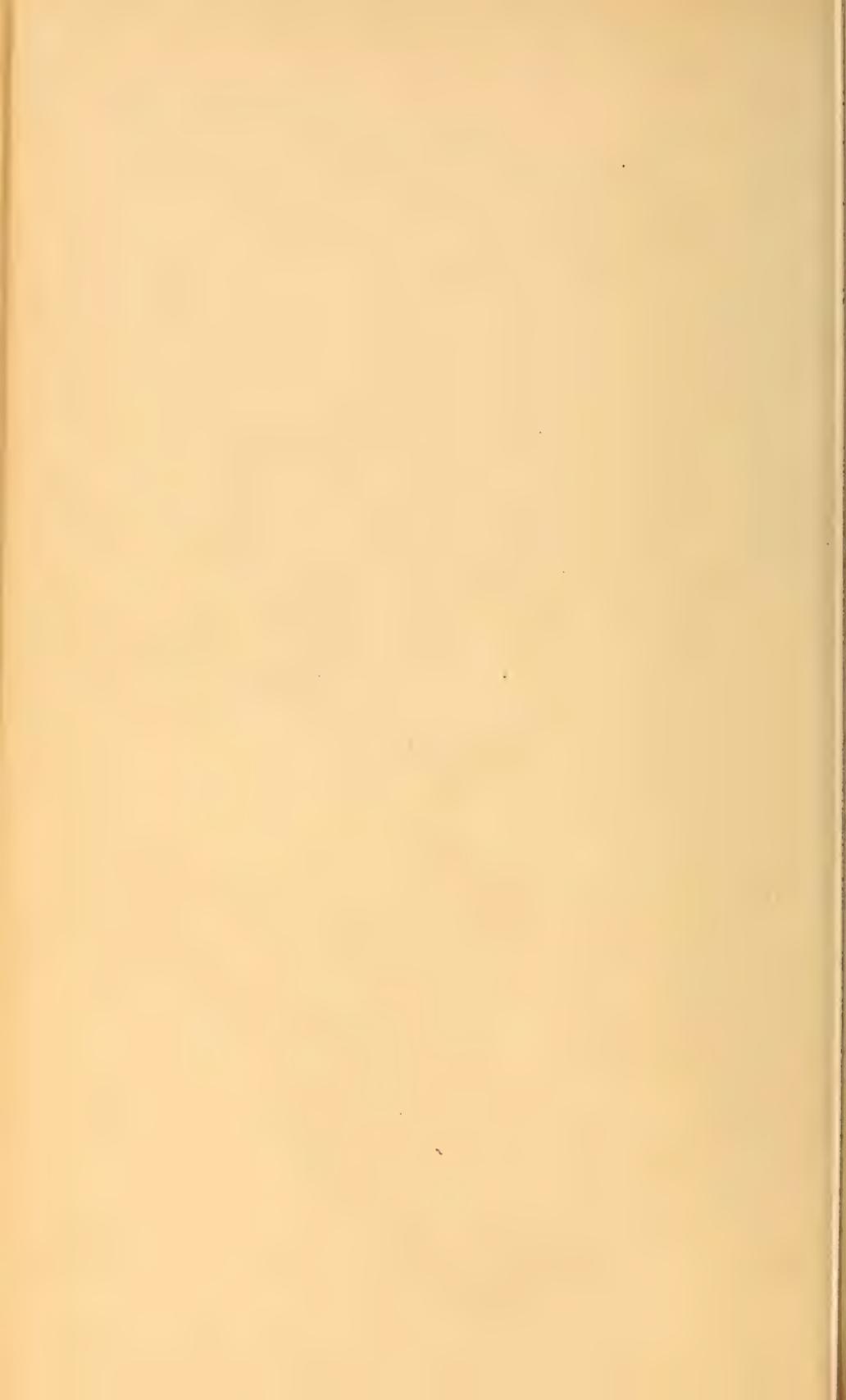


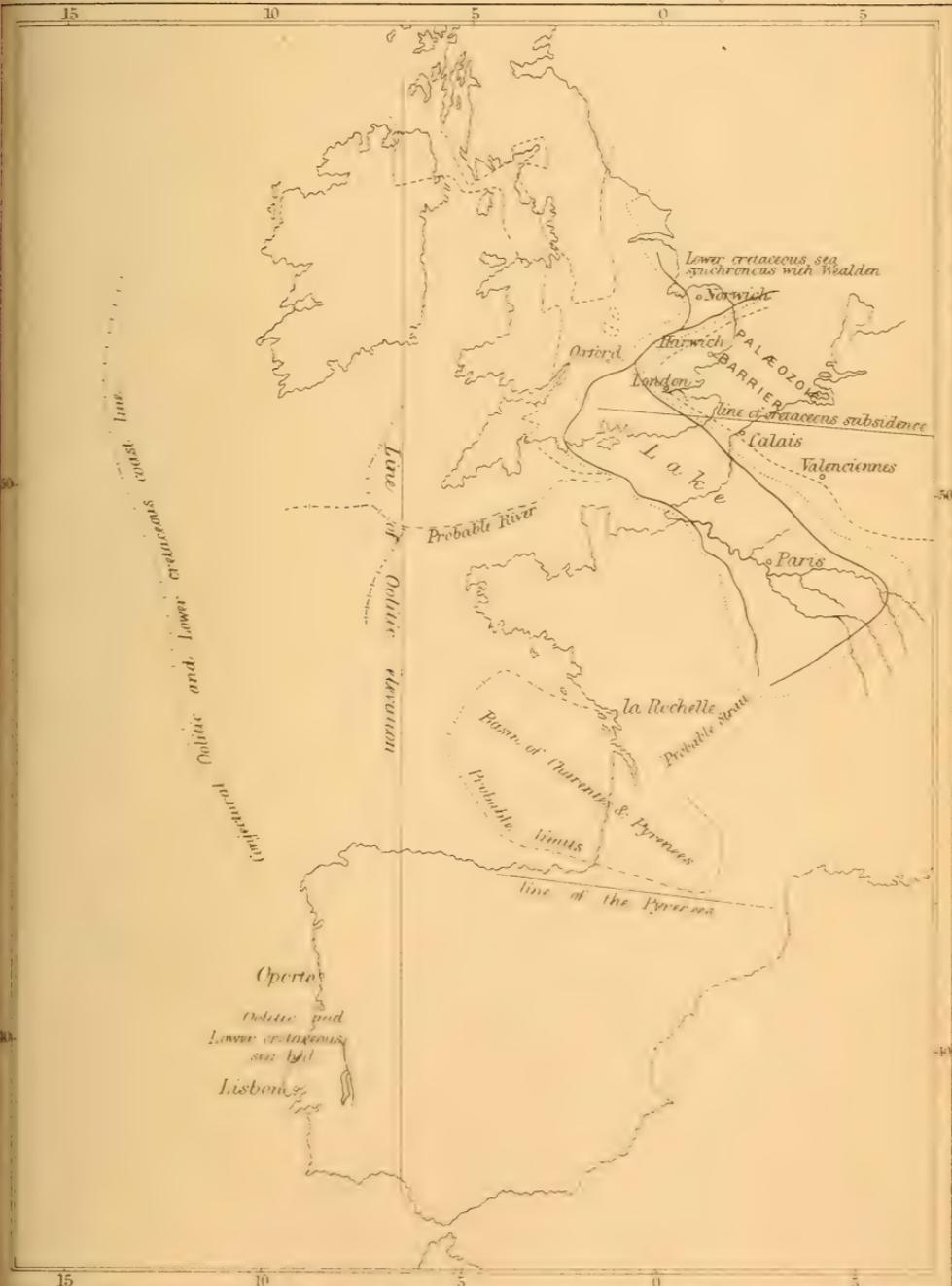




Scale of 6 inches.

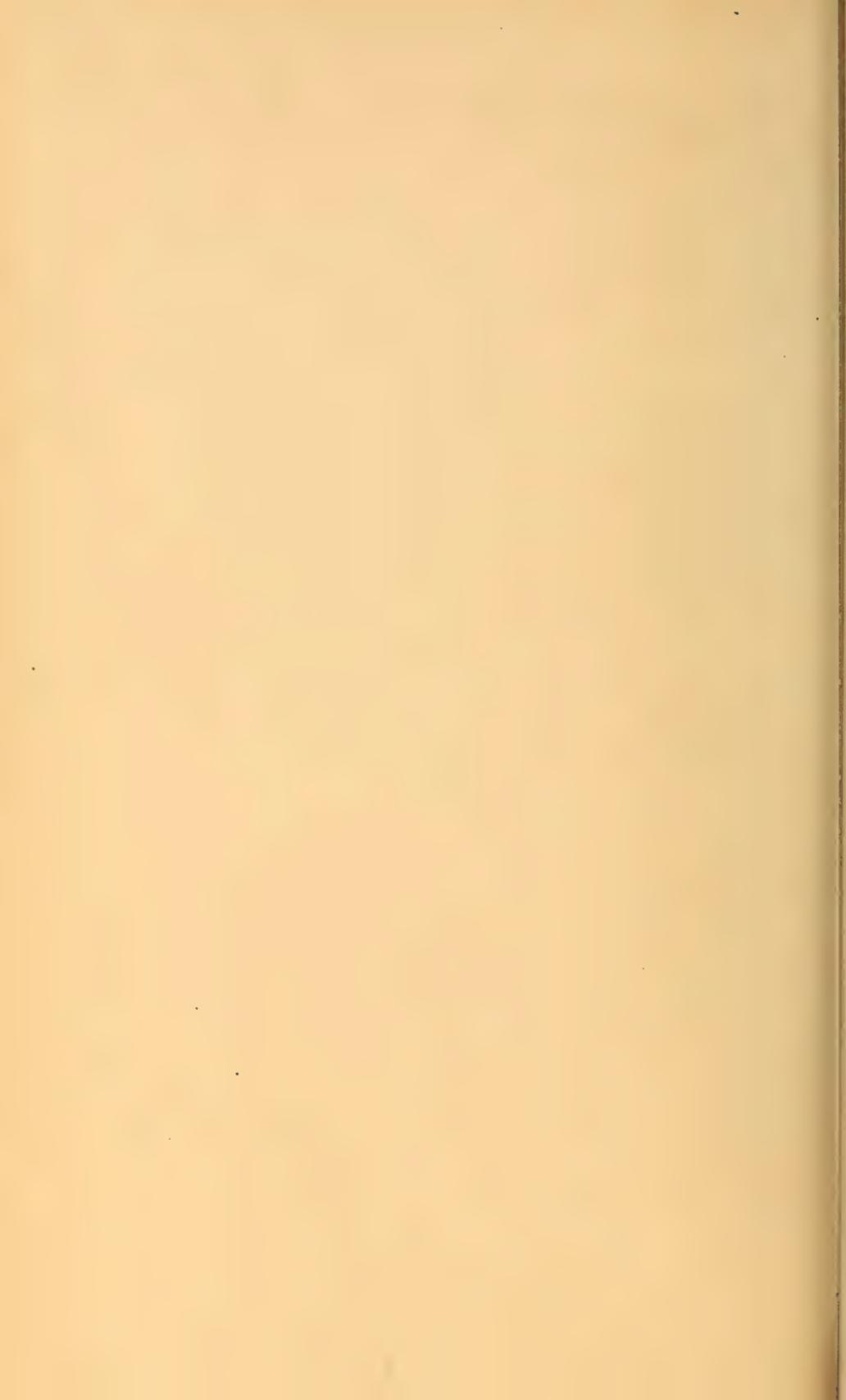






- - - - - Outline of shore of Trias
 - - - - - of Upper Oolite.
 ————— of Lake and Neocomian sea.
 - - - - - Conjectural Outlines.

J. Beane del.



Malachite.

Fig. 1.

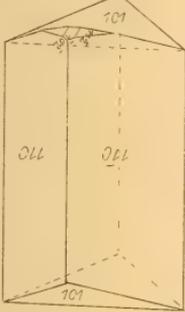


Fig. 2.



Fig. 5.

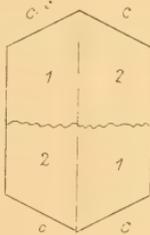


Fig. 3.

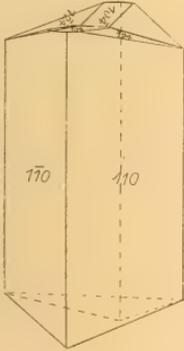


Fig. 4.

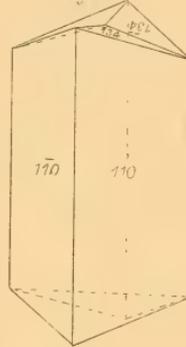


Fig. 8.



Fig. 6.

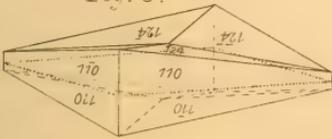


Fig. 7.



Fig. 9.

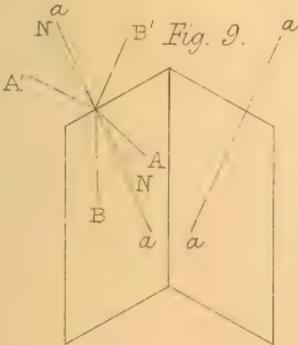
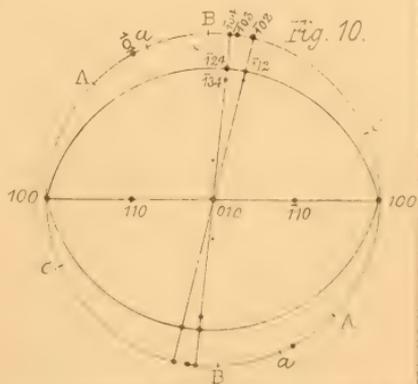
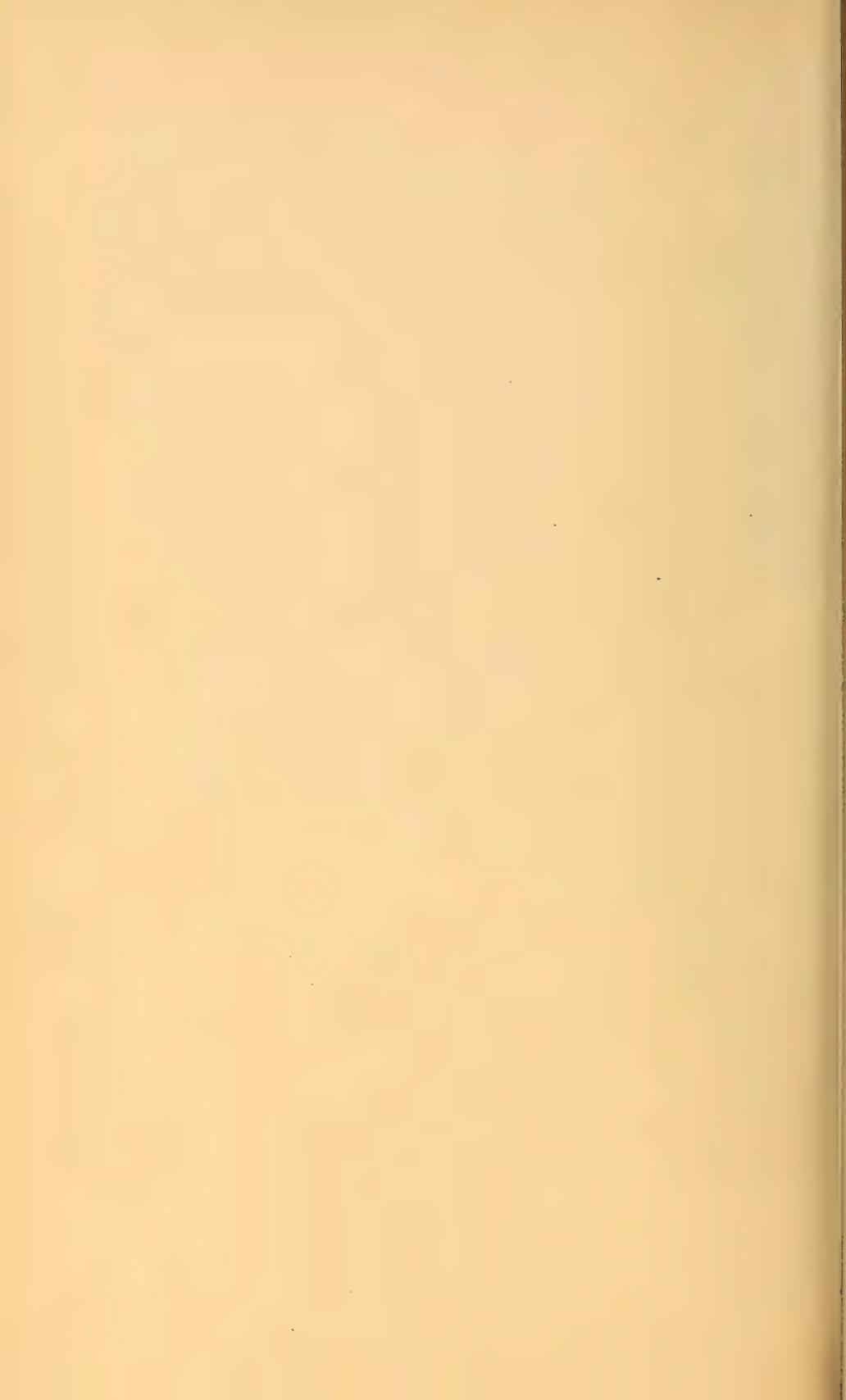


Fig. 10.





Gold.

Fig. 1.

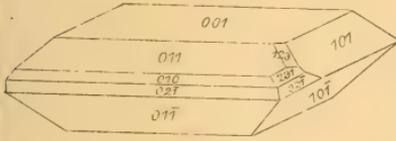
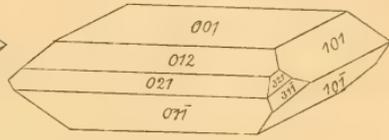


Fig. 2.



Euclaitz.

Fig. 3.

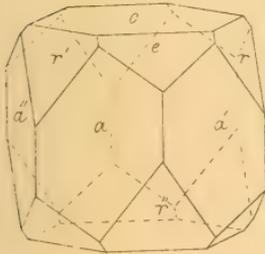


Fig. 4.

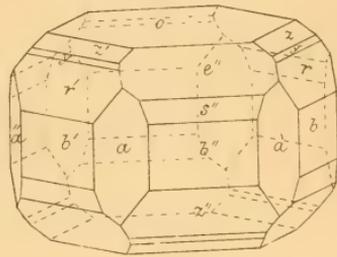


Fig. 5.

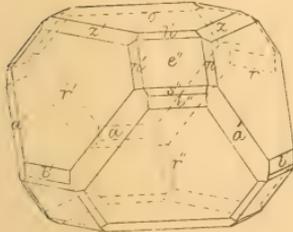


Fig. 6.

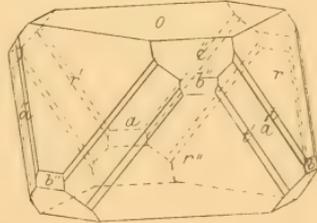
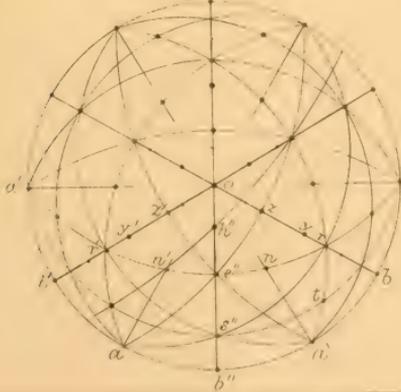


Fig. 7.

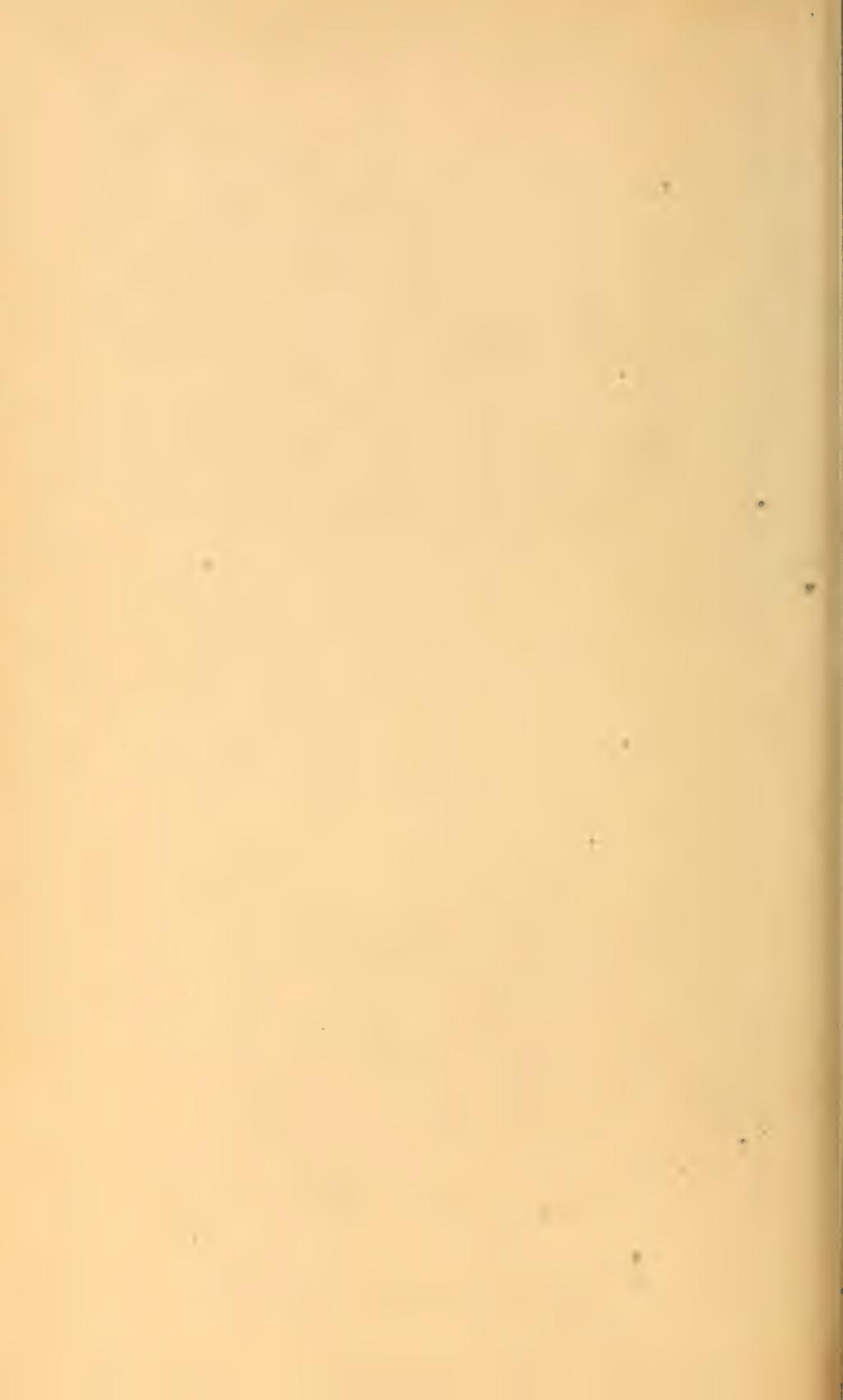






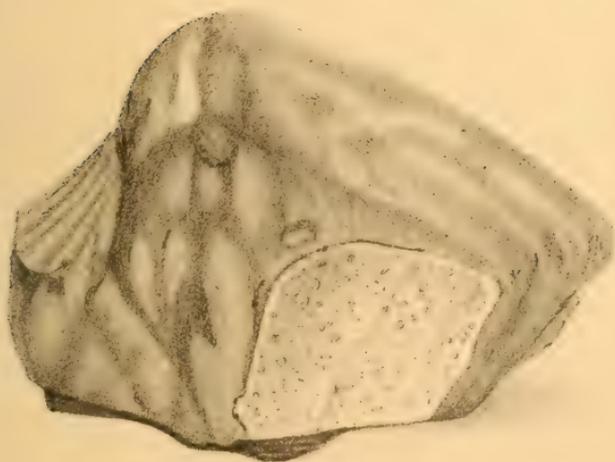
6 inches

The great Parnallee Aerolite in the British Museum.
(weight 127 lbs.)





Six Inches



Facile imp.

The Durala Aerolite in the British Museum.

4155¹²⁵





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