would be no particular difficulty in maintaining any thesis if results could be treated in this way.

Mr. Fletcher's work has been partly on the above lines. He obtained a "solution of excreta" by growing plants in water culture, and then used this solution as a medium for plant growth. It proved to be toxic, and the conclusion is drawn that the plant first used excreted some poisonous body. The experiment, however, is not a very good one. It is well known by those who have worked with water cultures that bacterial decompositions are liable to take place in the solution, producing substances injurious to plants; precautions always have to be taken to prevent development of bacteria. It does not appear that any such precautions were taken by Mr. Fletcher, indeed, the conditions under which he worked seem to have been favourable to bacterial development; well water was used, and the "solution of excreta" was allowed to evaporate at ordinary temperature until sufficiently concentrated for the second part of the experiment. There is no evidence that the toxic substance was excreted by the plant; it might equally well have been a bacterial product.

In another set of experiments crops were grown in rows side by side, and three lots of measurements were taken:—(1) the yield in the outside row, bordering on the bare ground; (2) the yield in the middle row; (3) the yield in a row bordering on another crop. The first is the highest, the second shows the effect of the plant on others of the same kind, and the third shows the effect on others of a different kind. The falling off in yield in the second and third cases is regarded by Mr. Fletcher as proof of a toxic excretion; it is generally explained as due to lack of water or food, and no satisfactory evidence is adduced against this view; indeed, Mr. Fletcher states that the reductions in crop are less marked under a more evenly distributed rainfall. We cannot consider that the question of root excretion has been materially advanced in any of these publications.

E. J. R.

ACID-RESISTING ALLOYS.

A PAPER was read at a recent meeting of the Faraday Society by Mr. Ad. Jouve describing the remarkable resistive character of ferro-silicon and other silicon alloys. Attention was directed to the fact well known to analysts that no methods of analysis for this substance, based upon the use of acids, with the exception of hydrofluoric acid, are employed for ferro-silicons, because ferro-silicon containing more than 20 per cent. of silicon is insoluble in acids. This protective property of metalloid is being made use of in producing acid-resisting vessels. Ferro-silicons, however, are not the only substances which possess this property; almost any alloy of a metal with this metalloid will behave in the same way to a greater or lesser degree, according to the nature of the metal. Calcium-silicide is, for example, unaffected by acid, whereas calcium itself acts vigorously upon water.

As showing the resistance of these alloys, which are called "Métillures," to acids, the following example is interesting:—Nitric acid, even as a vapour such as is obtained at the exit of a bisulphate retort or when mixed with nitrous acid, does not affect them at all. A striking example of this is given by a pipe which has been submitted for nearly five years to the daily passage of 660 lb. of nitric acid vapour at temperatures varying from 150° to 200° C. without its loss in weight exceeding a few decigrams in a total weight of a score of kilograms. This loss occurred quite at the beginning of the period, and was probably due to a few impurities remaining on the inner surface of the pipe after fusion.

Sulphuric and hydrochloric acid appear to have still less effect, and pipes of ferro-silicon have been used for carrying and condensing gaseous hydrochloric acid. Acetic acid and the mixture produced by treating calcium acetate are also without action. Seeing the extremely high price of platinum, which is the most stable of all industrial metals, it would appear probable that the advent of these new resisting alloys will become of very considerable importance. The chief drawback to their use is in the brittleness and weight of the alloy, the vessels made of it being generally rather thick.

CERTAIN ASPECTS OF THE WORK OF LORD KELVIN.[1]

WHEN a man of the first magnitude works continually at a single group of subjects from an age preceding twenty to an age exceeding eighty, the circumstance is so exceptional and the output so enormous that no ordinary summary or criticism can do it justice.

I shall not aim at any chronological sequence, and, in fact, propose to begin with those later physico-philosophic views which seemed to determine the direction of his thoughts and the attitude of his mind to nascent and contemporary discoveries in recent years.

For this aspect, even if difficult to treat of, is one which a biographer is bound in some fashion or another not to shirk; and, although myself unable to regard it with full sympathy, I am confident that my point of view is neither presumptuous nor disrespectful.

Kinetic Theory of Solidity.

Now, I confess that for some years before his death Lord Kelvin's attitude to fundamental physical or philosophical questions was somewhat of a puzzle to me. He seemed to be abandoning ground which he himself had opened up to explorers, and discouraging others from advancing in directions where he himself had pioneered. As a matter of fact, I was uncertain whether his position was even consistent and logically tenable or not; and at the British Association meeting at Leicester, during a discussion on the constitution of the atom in Section A, I had an opportunity of respectfully and deferentially challenging him on this subject. He responded, as always, in the kindest manner, and with great and almost exceptional lucidity indicated what had now become his position. I would not be understood as implying that he carried conviction, or led me to regard that position as a desirable one to occupy; but he showed it to be a consistent and logical one, which he had every right to occupy if he chose, and on which, therefore, it must be left for posterity, or at least for effluxion of time and progress of discovery, to pass anything in the nature of ultimate judgment.

I was much interested in this pronouncement, and before leaving Leicester jotted down a few notes concerning it, with the view of publishing them in his lifetime, in order that he might, if he chose, add to, or subtract from, or modify the statement. Other things prevented rapid publication, however, and accordingly it is too late for one of the objects in view, but still the notes are worth publication as suggesting genuine antithetical or alternative views of the universe. (They have now appeared in Nature for July 2, 1908.)

It may seem as if the real antithesis was between the postulates of a connecting medium, on the one hand, and of action at a distance across empty space, on the other, and as if Lord Kelvin were in favour of the latter view. I do not, however, think it would be fair to attach to him that responsibility. I think it was more a matter of practical politics, with him, than a philosophical conception. I think he would have liked to see an explanation in terms of a connecting medium, if it could have been managed; but, after spending some years in the attempt, he abandoned it either as too difficult or as hopeless, and constrained himself to be satisfied with unexplained forces between masses of matter acting according to specified laws; the question of the medium or mechanism through which they acted being left out of account as unnecessary from the point of view of practical dynamical calculation and consistent reasoning.

He did speak at times, however, as if immediate action across empty space would be logically satisfactory to him, and quite good enough as an explanation; the only question being, was it the true one? To me I confess that any such philosophic scheme must necessarily be a cold and merely descriptive account of material activity—that it must necessarily fail to go to the heart of the matter or to constitute what may more reasonably be called "explanation." The conception of forces acting according to a specified law of distance is capable of yielding dynamical results truly, but not of explaining them. Ex-

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[1] Abridged from the presidential address to the Faraday Society, delivered by Sir Oliver Lodge, F.R.S., on May 26.

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planation, however, is never ultimate; so it may be that the process contemplated, and in his last years energetically worked at, by Lord Kelvin is an intermediate stepping-stone, which must be taken in order to cross to some more stable resting-place beyond; just as has happened in the case of gravitation.

The above is an attempt fairly to represent what I conceive must have been in the mind of our great leader, and it was a kind of pronouncement which I hoped to draw from him by the publication above mentioned. If he had been living it would have been presumptuous to try and state more concerning his views than he himself had indicated; and still it is to be hoped that anyone acquainted with his mind on this matter will make the necessary corrections.

ENERGY.

If we now proceed to ask what great generalisation will for ever be associated with Lord Kelvin's name, and in future ages stand out as his greatest achievement, it is not easy amid the wealth of material to focus it clearly. A few days ago I myself should not have been certain, if suddenly catechised, what my answer would be to such a question. But in preparing this address, and reading once more some of his early papers, I find nothing greater than what emanated from him in and about the year 1851, when he was immersed in the doctrine of energy. I do not mean, of course, any single year exactly, but about that period of his life; for in the records of that time are to be found, I think, his greatest and strongest memoirs.

The keenness and penetration of his mind at that epoch must have been something astounding. With all his mathematical powers alert, with tremendous natural genius, and extraordinarily vivid interest in phenomena of all kinds, he seized the facts concerning energy as they emanated from Carnot and from Joule, and with them in his mind, more powerfully and persistently than even Helmholtz, he brooded over the whole domain of physics until he elicited therefrom a series of the most beautiful and striking discoveries—discoveries which, as they have gained in familiarity, have perhaps lost something in charm, by constant iteration in text-books and college lectures, but which, in their freshness, well repay an attentive perusal; though their form is far inferior to their substance.

So I expect that the answer of posterity, to the question above mooted, will be that his most immortal work is the development and application of the doctrine of the conservation of energy, together with the comprehension and elaboration of the laws of thermodynamics.

Later he became more immersed in the work of the world, managed a great deal of practical business, and made many inventions of surpassing ingenuity; but although all this later work is the best known to the general public —if, indeed, any scientific work can be said to be really known to that body—yet for pure genius, to my mind, nothing since Newton comes up to his achievement in the fifth and sixth decades of the last century, especially from 1848 to 1856.

The comprehensive recognition, and extraordinary application to physics, of Carnot's brilliant "Reflections on the Motive Power of Fire," or, as we should now say, *On the efficiency of heat engines,* must have been largely due to Lord Kelvin's influence, and to the clear and enthusiastic way in which he took up and developed the subject. It is singular that this discovery of the second law of thermodynamics, which came historically first, created a real difficulty and obstruction in the recognition of the truth of what is now called the first law; and Joule's work would not only have been rejected by the Royal Society, as it was, but would have met with a total lack of recognition, or even disdain, had it not been for Lord Kelvin's perception of its value at a meeting of Section A of the British Association in 1847. In fact, the development of the whole subject of thermodynamics, though extensively carried out by Clausius and others, must have received strong initiative from him.

But it was not the mere recognition of the true nature of heat as a form of energy—so that when work was done by a fall of temperature the heat removed was less than the heat supplied, thereby breaking down the hydraulic analogy—but it was the way in which, both by Lord Kelvin and Helmholtz, the conservation of energy was applied all over the ground of physics, and especially so as to incorporate electrical phenomena with the rest, in one scheme, that was most remarkable.

Of all the memoirs dealing with the conservation of energy as applied to electricity, perhaps the most striking, though one of the simplest, is Lord Kelvin's early paper on transient currents, or the discharge of an electric capacity; wherein he gives the whole theory of electric oscillations, in so far as they can be treated without recognising the radiation which accompanies them—a discovery reserved for Maxwell. . . .

The extraordinary magnitude of the giants in physical science, especially in mathematical physics, during the Victorian era, and, indeed, throughout the nineteenth century, will probably be recognised more fully by posterity than by us. It will be many generations, probably many centuries, before the general and literary world can receive any adequate impression on the subject, or begin to understand their methods and their more recondite results.

SPECIFIC HEAT OF ELECTRICITY AND VOLTA FORCE.

It seems to me an amazing piece of insight which led Lord Kelvin at that date, 1851, to attribute to electricity, even hypothetically and only for convenience, something akin to real specific heat. The fact was really discovered in 1851, though he did not verify it experimentally until 1856 (see pp. 246 and 319, &c., of that monument of human power, vol. i. of "Math. and Phys. Papers"). The modern theory of electrons—which are now supposed to be flowing in great crowds through a conducting metal, and which, by their irregular motions, must account for some of the heat energy of a substance, in addition to the much larger portion corresponding to the motion of the atoms—seems to justify this curious expression, "specific heat of electricity," to an unexpected degree; and thermoelectric phenomena may be stated in terms of a definite pressure of these mobile and detached electrons in any given substance, after the fashion of the pressure of a gas or the osmotic pressure of a salt dissolved in a liquid.

There is, indeed, no obvious reason for denying that the Volta force might be expressed in this way too, were it not that a perfectly valid *vera causa* for this effect is to be found at the surface of the metals, where they are in contact with air or other chemically potential material; and that the magnitude of the effect, so calculated, from electrochemical and thermal data, agrees with observation in absolute as well as in relative value. These and other facts lead me to maintain that Volta force is an incipient display of potential but not actual chemical activity, at the bounding surface of a metal and a dielectric. But I ought to say that Lord Kelvin differed from this view in 1884, and that he still might not agree with all that is implied in this summary statement.

THERMOELECTRICITY AND GAS THEORY.

The splendid way in which the second law of thermodynamics was applied to the phenomena of thermoelectricity, so as to establish the laws of a thermoelectric circuit, is too well known to demand notice here. The chief features of it are to be found on p. 249 of Lord Kelvin's "Math. and Phys. Papers," vol. i.; but the enterprise was, I think, to some extent attended by good fortune, such as often rewards those who do not hesitate to risk something in the development of a theory, leaving it to be corrected, if necessary, by the future. (J. R. Mayer's theoretical estimate of J is another illustration.) It so happens that the thermoelectric theory has demanded very little correction, in spite of the intrinsic uncertainty attending application of the second law to an operation which had one irreversible feature about it—which might have been more relevant than it turns out to be—viz., heat conduction.

As an example of the opposite tendency, however, in Lord Kelvin's mind—for it was a mind which at times was extremely cautious—I think I may instance the difficulty he felt about the Boltzmann-Maxwell theory of the distribution of molecular energy. He always seemed to be troubled with a persistent difficulty about the innu-

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merable degrees of freedom possessed by a molecule, and was unwilling to accept the position that many of these degrees of freedom were out of the running, so to speak—were beside the mark, for the purposes of gaseous theory, inasmuch as it was only those which affected, and were affected by, *collisions* that really mattered. Anything like organised motion, such as that of the planets, is out of the running, of course, and so is any internal motion of the parts of an atom which collisions do not produce or lessen or in any way affect.

It may be said that some collisions, like those which result in chemical combinations, do shake the parts of an atom—as is known by the emission of light. That is quite true, but then these collisions are exceptional, and, moreover, energy so transferred is speedily radiated away. The Boltzmann-Maxwell theory only applies to that which remains a permanently constituent portion of the heat energy of the substance—that is to say, the energy effective in producing pressure and the other manifestations of temperature—the unorganised random collision energy; it is this alone which need ultimately distribute itself equally among the parameters, through the agency of innumerable encounters. It is probable, however, that Lord Kelvin would not concur in the simplicity of this statement; he continued to be impressed by outstanding difficulties.

DISSIPATION OF ENERGY.

Of Lord Kelvin's work in connection with the dissipation of energy I shall not say much. I fancy that he himself, and certainly some of his disciples, have been at times inclined to attribute to the law of degradation more ultimate and cosmic importance than properly belongs to it. Its significance is limited to the validity of the terms "heat" and "temperature"; and if for any reason those terms cease to have a practical meaning, then the dissipation of energy also ceases to be inevitable. The theory, as originally stated by its author, was formulated as an axiom beginning, "It is impossible by means of inanimate material agency," &c., which at once conveys a suggestion that by some other means it may be possible. The different availabilities of energy of various kinds must be essentially a human and temporary conception, useful and convenient for practical purposes, but not ultimate or cosmic. What devices there are for thrusting aside the inevitableness of dissipation, and so evading the goal of ultimate stagnation, I do not know; they have not yet been discovered by us; but there is nothing inconceivable about them. Maxwell's "demons" is one attempt in that direction; nitrifying bacteria have been suggested as another. It is not at all certain what the influence of "life" may be; and all these agencies have to be eliminated if the uncompromising dissipation of energy doctrine is to be accepted. It was not originally stated in quite uncompromising form (see p. 514 of vol. i.).

The *conservation* of energy is a very different thing; that applies to every form, and is a comprehensive law; but the dissipation of energy has no meaning in circumstances when "heat" and "temperature" are obsolete terms,—that is to say, when what we now consider to be unorganised and intractable molecular motions can be dealt with in an individual and organised way. Ultimately and absolutely no operation need be irreversible. Irreversibility means only that things have got temporarily beyond our control, as a fire does sometimes.

ABSOLUTE MEASUREMENT.

To Lord Kelvin, more than to anyone else, we owe the realisation of the system of absolute measurement applied to such intractable quantities as are found in electricity and magnetism; and if the world decides to call its commercial electrical energy unit—now commonly spoken of, in insular fashion, as a Board of Trade unit, or B.T.U.—by the universally known and appreciated name of "a Kelvin," such a procedure will be entirely appropriate.

Counting, or the enumeration of discrete quantities, is a very easy and natural operation; but measurement, in the sense of expressing the warmth of a day, or the brightness of a light, or the strength of a current, or the field of a magnet, or the resistance of a wire, or the transparency of a window, or the elasticity of a metal, or the conducting power of a gas, in numerical fashion, is not by any means a simple thing; it usually needs great ingenuity, and sometimes can hardly be done.

The invention of suitable units, and the mode of expressing currents and electromotive forces and resistances in such units, is very far from being an obvious notion; and even now the full meaning of the idea of absolute measurement is not in all quarters quite clear. In the first instance it was not always quite clear, I venture to say, even in the mind of Lord Kelvin himself; and a certain partial incompleteness was almost necessary in order to reduce electric and magnetic quantities to simple mechanics. For, as a matter of fact, they cannot be reduced to simple mechanics, or, at least, have not yet been so reduced; and it was by partially blinding ourselves to that fact that the ideas of the ohm, the ampere, and the volt were attained. We used to be told that resistance was a velocity, and that electrostatic capacity was a length, also that self-induction was a length, and so on. But, of course, resistance is not a velocity, nor is self-induction or capacity a length. Nevertheless, had it not been for this partially erroneous simplification, the introduction of any system of electric measurement would probably have been seriously delayed. Incidentally, it may be noted that the magnetic method of measuring resistance, or "determining the ohm," was devised by Weber. Kelvin's first method was based upon Joule's law (see p. 502, vol. i.).

ABSOLUTE TEMPERATURE.

One of the remarkable achievements of Lord Kelvin has been the conception and determination of absolute temperature. The idea of an absolute temperature—that is to say, of a temperature reckoned from a real and actual zero, not a conventional one, and specified so as to be independent of the properties of any particular substance—follows rather naturally from the second law of thermodynamics, and from the fact that the efficiency of a perfect or reversible heat engine is independent of the properties of the working substance—being dependent only on the temperatures at which heat is supplied and withdrawn. Absolute temperature is, in fact, the reciprocal of Carnot's function, as Kelvin showed in 1848 (p. 100, vol. i., "Math. and Phys. Papers"). And the absolute zero is the temperature at which the working substance has exhausted all its heat in doing work, so that there is none to yield up as waste—the temperature, in fact, at which a condenser or "cold body" becomes unnecessary.

On a thermal diagram a *scale* of temperature can easily be drawn, as the rungs of a ladder between two adiabatic lines, such that the area of each space is the same; and in order to find the number of rungs, with a given-sized degree, it becomes a matter of experiment to determine the total heat obtainable from an isothermal operation performed on the substance to which the adiabatic lines belong. The measurement necessary can be made upon any substance—steam or anything else—but it must be dependent on an actual operation (say an expansion)—not a closed cycle of operations—and on a measurement of the change of energy therefrom resulting.

Lord Kelvin gives as the general expression for the absolute temperature of any substance whatever, the internal energy of which is E,

$$\mathrm{T}=\left(p+\frac{d\mathrm{E}}{dv}\right)\frac{d\mathrm{T}}{dp}-\frac{d\mathrm{E}}{dp}\cdot\frac{d\mathrm{T}}{dv} \quad . \quad . \quad . \quad (\mathrm{A}).^{1}$$

For an ordinary gas $\frac{d\mathrm{E}}{dv}=\mathrm{K}+c\frac{d\mathrm{T}}{dv}$, where K is Laplace's cohesion constant; and $\frac{d\mathrm{E}}{dp}=c\frac{d\mathrm{T}}{dp}$; so this expression (A) agrees with what we obtain below as equation (5).

The actual determination, as hitherto experimentally made, of the zero of absolute temperature, below which it will be for ever impossible to cool bodies—since at that temperature they possess no heat, and, therefore, cannot have any more removed—may be said to depend (not necessarily or theoretically, but actually as the simplest method in practice) on the conception of a perfect gas in the first place;—that is, one of the molecules of which act upon each other and upon the surrounding walls solely by bombardment, there being no cohesion whatever between the molecules. The temperature at which the pressure of such a

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[1] See "Encyclopædia Britannica," article "Heat."

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gas becomes zero must be simply the temperature of absolute molecular rest, and, therefore, will be the absolute zero. From the properties of such a gas its absolute temperature could at once be experimentally determined, if only such a gas were available for experiment; for it would come out as the reciprocal of its coefficient of expansion. But as a perfect gas is not available, an imperfect gas has to be employed, and a correction made for the amount of its imperfection; the amount of this correction being deduced by reasoning based on its behaviour when subjected to an irreversible operation. For instance, it may be allowed suddenly to expand adiabatically in such a way as to do no external work, and, therefore, not to cool itself if it were perfect, provided time is allowed for all eddies and streaming motions to subside; and we may then observe the actual consumption of heat or fall of temperature really produced—which would be proportional to the cohesion multiplied by the change of volume. The change of temperature so observed is the chief term in a correction to be applied to the reciprocal of the observed coefficient-of-expansion-under-constant-volume of the imperfect gas.

The experiment as first made by Gay-Lussac, September, 1806, and later independently and more exactly by Joule, of allowing a gas to double its volume inside a closed vessel, by opening a connection between a full and an empty portion of a vessel, was manifestly an interesting and suggestive experiment, and a check or verification of Mayer's hypothesis that the mechanical equivalent of heat could be obtained by equating the heat supplied and the work extracted from expanding air; but the full meaning and bearing of such an experiment is by no means obvious, and it is remarkable that it should lead to a determination of the zero of absolute temperature. For this purpose it has to be repeated in a more refined form—the oozing of gas as a steady stream from high pressure to low through a porous plug—and a determination made of the change of temperature resulting, when all eddies and organised kinds of motion have subsided, and when everything has become heat again, except what was lost in internal work.

It is well known now that the practical liquefaction of gases depends on this very effect; for, of course, without some cohesion between the molecules liquefaction would be quite impossible. The essence of liquefaction is the automatic subdivision of the contents of a vessel into two sharply bounded regions of different density, and the retaining of them in this condition for a time by internal molecular forces.

ABSOLUTE TEMPERATURE.

The elementary argument about the notion of absolute temperature in terms of a perfect gas can be put thus:—

A perfect gas is one the molecules of which act on each other, and on the walls of the containing vessel, solely by bombardment. Simple mechanics shows that such a substance exerts a pressure—

$$p=\frac{1}{3}\rho u^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and whenever it expands all the work done is against external pressure.

The heat in such a body is solely the energy of its irregular or unorganised molecular motion—including rotation as well as translation; and the temperature of such a body can be defined as simply proportional to the heat, or equal to the heat divided by a capacity-constant mc.

If the gas has to expand against external pressure, more heat must be supplied to allow for the external work done, $\int p dv$; the capacity being now called mc' if the pressure is constant. Consequently, if the gas be heated at constant pressure, from absolute zero up to the temperature T, the heat required can be expressed as—

$$\mathrm{H}=mc\mathrm{T}+pv=mc'\mathrm{T};$$

wherefore—

$$p=\rho(c'-c)\mathrm{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

which may be called the characteristic equation of the substance.

Comparing this with the first equation, we see that—

$$u^2=3(c'-c)\mathrm{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which constitutes a definition of absolute temperature in terms of the characteristic constant $c'-c$; the "3" having reference to the three dimensions of space.

Actually to determine T we can employ equation (2), and can get rid of the constant, say, by measuring the increase of pressure when the gas is heated at constant volume. This gives—

$$\frac{dp}{p}=\frac{d\mathrm{T}}{\mathrm{T}};$$

or—

$$\mathrm{T}=p\frac{d\mathrm{T}}{dp}=\frac{1}{a} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

the reciprocal of the coefficient of expansion.

In other words, the expansibility of a perfect gas is simply the reciprocal of its absolute temperature.

This is consistent with the form of characteristic equation which allows for molecular bulk, though not for molecular forces—namely, $p(v-b)=\mathrm{R}\mathrm{T}$.

For a slightly imperfect gas there is the cohesion or molecular-attraction term to be attended to as well, and its characteristic equation is—

$$(p+\mathrm{K})(v-b)=\mathrm{R}\mathrm{T},$$

K being a function of volume only. For constant-volume warming this gives—

$$\frac{dp}{p+\mathrm{K}}=\frac{d\mathrm{T}}{\mathrm{T}},$$

or—

$$\mathrm{T}=p\frac{d\mathrm{T}}{dp}+\mathrm{K}\frac{d\mathrm{T}}{dp}$$

or—

$$\mathrm{T}=\frac{1}{a}\left(1+\frac{\mathrm{K}}{p}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where a is the coefficient of expansion as measured on a constant-volume thermometer; showing that a correction factor not far from unity must be applied, depending on the incipient cohesion or inter-molecular attraction, represented by Laplace's K or van der Waals's $\mathrm{A}\rho^2$.

To get K we must perform a definite operation, say a sudden expansion δv, under adiabatic conditions, allowing no external work to be done; and we must observe the resulting absorption of heat, say by noticing the small change of temperature $\delta\mathrm{T}$. It would be zero if the gas were perfect. If imperfect, the energy lost is $\mathrm{K}\delta v$.

To ensure that no external work is done, the operation must be performed in a rigid vessel, and a steady stream of gas will carry off the defect of heat $\delta\mathrm{H}$. The cooling will then be due only to internal work $\mathrm{K}\delta v$; and the heat change can be expressed as $mc'\delta\mathrm{T}$, when eddies have subsided.

Thus we get—

$$\mathrm{K}\delta v=\delta\mathrm{H}=mc'\delta\mathrm{T}=v\rho c'\delta\mathrm{T};$$

but now instead of δv we may write $-\frac{v}{p}\delta p$, since the temperature is nearly constant, so that—

$$\mathrm{K}=-\rho c' p\frac{\delta\mathrm{T}}{\delta p} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Hence, denoting by θ the small observed change of temperature corresponding to the change of pressure Π, and substituting (6) in equation (5), we get finally as an expression for the absolute temperature of the gas experimented on—

$$\mathrm{T}=\frac{1}{a}\left(1-c'\rho\frac{\theta}{\Pi}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Perhaps the equation looks still clearer if we write it in terms of the volume of air v streaming through the porous plug, down the difference of pressure δp, and carrying with it ultimately the defect of heat $\delta\mathrm{H}$, measured anyhow; for then—

$$\mathrm{T}=\frac{1}{a}\left(1+\frac{\delta\mathrm{H}}{v\delta p}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7^1)$$

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But the expression for the absolute expansion term—

$$aT = \frac{p + K}{p} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5^1)$$

is also a very simple one.

To interpret equation (7) numerically—

The quantity $c'\rho$ will be recognised as *the atomic heat*, which is nearly the same constant for all ordinary gases, and equal in c.g.s. energy units to—

$$0{\cdot}2375 \times 0{\cdot}001293 \times (42 \times 10^6) = 0{\cdot}001294 \times 10^6 \text{ ergs per c.c. for dry air.}$$

The actual change of temperature per atmosphere, observed as the final result of the irreversible Joule and Thomson expansion, was, for air, a lowering of about a fifth of a degree, or more exactly 0°·208; so that—

$$\frac{\theta}{\Pi} = \frac{0{\cdot}208}{10^6 \text{ dynes per sq. cm.}}$$

Hence, since *ergs per c.c.* are the same as *dynes per sq. cm.*, the value of what we have just reckoned as the dimensions of the whole term $c'\rho\theta/\Pi$ comes out right as a pure number (being plainly a ratio of two energies when ρ is written m/v); and the correction factor for air equals—

$$1 + 0{\cdot}001294 \times 0{\cdot}208 = 1{\cdot}00027.$$

At zero Centigrade the expansibility of air was measured by Regnault as 0·0036706. Wherefore the absolute temperature corresponding to zero Centigrade is, in accordance with equation (7)—

$$\frac{1{\cdot}00027}{0{\cdot}0036706} = 273{\cdot}17.$$

ELECTRICAL THEORY OF MATTER.

On the great modern region of physics centring round an electrical theory of matter, Lord Kelvin's mind was somewhat conservative; as perhaps it was in electricity generally, whenever results could not be obtained by straightforward dynamics or by energy calculations. In other directions he only advanced under protest, as it were, towards the goal at which others were enthusiastically working. Nevertheless, we owe to him some pioneering work even in this branch.

Comparatively modern speculation and calculation on the structure of an atom are contained in a remarkable paper by Lord Kelvin, published in the *Phil. Mag.* for 1901 under the curious title "Æpinus Atomised." It is reproduced in the volume of Baltimore lectures as Appendix E. It was probably the first attempt to work out the statics of an atom, according to a simple conception the major consequences of which can be traced with comparative ease, viz., that of a spherical portion of uniform positive electricity in which minute negative charges are sown like specks; being attracted towards the centre of the sphere according to the law of direct distance, and repelling each other according to the inverse square law.

COSMIC CALCULATIONS.

Of the work of Lord Kelvin in elasticity, I shall here say nothing beyond the remark that his kinetic view of elasticity often seems to me one of the most suggestive and ultimately pregnant of all his theories.

His papers on celestial dynamics are very remarkable and lucid, though we may not feel that they represent the last word on the question; any more than the last word has been said as to the age of the sun or of the earth. The fact that after a lifetime of immersion in all the intricacies of natural philosophy Lord Kelvin still postulated an origin or beginning for the material universe—a beginning when it was essentially different, not only locally but universally, from its present condition—and that he endeavoured to conceive what it might then have been like, in those early times—is a notable circumstance and one of general interest. To me there appears no reason for calling those times "early" rather than "late"; nor would I suppose a beginning or ending at all, either for space or for what is in space, other than such beginnings or endings as we might detect, or may hope to detect, somewhere, even now.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

PROF. WÜLFING has been appointed to the chair of mineralogy in the University of Heidelberg. We notice also that the same university has just celebrated the fiftieth anniversary of the doctorate of Prof. Georg Quincke, professor of physics in the University.

THE correspondence between the Colonial Secretary (Mr. J. C. Smuts) and the council of the Transvaal University College relating to the organisation of higher education in the Transvaal has just been issued as a Blue-book (T.G.—24—1908). The question of a Transvaal university is not considered yet to be ripe, the proposals at present being for the establishment of a university college. If the recommendations of the committee appointed by the Colonial Secretary be carried out, the Transvaal University College will be a federation, under one council, of three institutes. The technical courses would be assigned to the Johannesburg branch, the literary and science courses to Pretoria, and the agricultural work would be centred at Frankenwald. It has been decided to proceed at once to carry out the scheme so far as it relates to the allocation of the various departments of work and study to the Pretoria and Johannesburg branches respectively. For the Frankenwald branch it is hoped that 200,000*l.* will be available from the Beit bequest, but this part of the scheme is deferred. Certain questions relating to the constitution of the reorganised college are also held in abeyance. It is obvious that the three branches will have but a slender bond of union; but after reading their report we are inclined to accept the view of the committee, that the difficulties in the way of finding any one place where the branches can be developed side by side are insurmountable.

THE *British Medical Journal* for August 15 gives its readers a lengthy report of the discussion by the British Medical Association at Sheffield on the education of the medical student. The speakers included Profs. Starling, Armstrong, Sherrington, Sims Woodhead and Osler, Sir Felix Semon, Dr. Dawson Turner, Dr. Buist, and Dr. Russell Wells. The discussion formed part of the proceedings of the Section of Physiology, but the list of speakers guaranteed adequate handling of their theme in respect of scientific as well as clinical aim. It appeared to be widely held that (1) the period devoted to preliminary and intermediate study should be curtailed; (2) closer consideration should be paid during the intermediate course to the practical needs of the future medical man—*e.g.* biological studies should have a physiological rather than a morphological bias; (3) more clinical study is required in the later periods of the training, especially practice of diagnosis; (4) there should be fewer lectures and more demonstrations. The leading article in the same number of the journal is devoted to a consideration of this discussion jointly with the new regulations for the medical curriculum recently promulgated by the University of London. The journal approves the decision of the University to extend the final part of the curriculum to thirty-six months. We may point out that we are still behind the foremost Continental countries in our estimate of the time required to train a qualified medical practitioner.

A WELL printed and illustrated pamphlet has been issued by the British Education Section of the Franco-British Exhibition under the title "A Short History of National Education in Great Britain and Ireland." In the article which appeared in NATURE for August 13 attention was given to the manner in which the exhibition, both as a whole and in detail, illustrates national progress, whether such progress be viewed from the pedagogic or from the administrative aspect. The booklet now before us deals with the latter aspect, and its author—Mr. T. L. Humberstone—gives a broad and clear outline of his subject. Too little is said of private schools, but the history of public provision for education during the last three centuries is made clear. The awakening of England and Wales during the last century to their responsibility for educating their citizens is traced with judgment, and mention is made of the latest development of this sense of responsibility shown by the medical inspection of school children. The value of this production is much in excess of its price—it is published by Messrs. King at 3*d.*

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