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TO PROMOTE THE STUDY OF ELECTROCHEMISTRY, ELECTROMETALLURGY,  
CHEMICAL PHYSICS, METALLOGRAPHY, AND KINDRED SUBJECTS.

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PART I.

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### ON CERTAIN ASPECTS OF THE WORK OF LORD KELVIN.

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*(Presidential Address to the Faraday Society, delivered May 26, 1908, Sir JOSEPH SWAN, F.R.S., PAST PRESIDENT, in the Chair.)*

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.

It is only therefore with some aspects of that work that I propose to deal; and, unconsciously I suppose, I select those aspects which from one or another point of view have specially impressed myself. I shall, however, except from even these all that portion which relates to practical applications; partly because this portion has been dealt with elsewhere, but chiefly because on this occasion it is appropriate to review those portions of the work which are on the border-land of Chemistry and Physics, or else are where Physics runs into Philosophy—those portions, namely, which would have been specially likely to arouse the interest of our godfather Faraday.

But even here I must make large omissions,—for no particular reason except that, even when they are made, the remainder is more than can be dealt with in any reasonable time.

Among the regions of work omitted altogether are those concerned with the tides, with navigation, with the size of atoms, with the age of the earth, with heat-conduction, with electrostatics and electric images, with elasticity, with physical optics, with static magnetism, and with telegraphy.

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 a view to 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. Accordingly they will appear in a forthcoming number of *Nature*.

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. Explanation, 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

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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 any one 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 till 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 textbooks 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 Thermo-dynamics.

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 in pure genius nothing to my mind, 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 "Reflexions 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 thermo-dynamics, 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 thermo-dynamics, 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 three quantities to be expressed are : the kinetic energy of the current itself, the rate of production of heat as investigated by Joule, and the energy corresponding to the storage of charge in a condenser—that is to say, the term  $\frac{1}{2}SV^2$ , or  $\frac{1}{2}\frac{Q^2}{S}$ . Any change in this energy, due to current ( $C = -\frac{dQ}{dt}$ ), must be accounted for by frictional generation of heat and by inertia acceleration. The energy of the latter term was essentially discovered by Lord Kelvin in this Paper (*Phil. Mag.*, Jan., 1853) as an outcome of previous papers in 1851 and 1848 (Arts. xxxv. and liv. of Vol. I. of *Math. and Phys. Papers*). These contain the first statement, so far as I know, of the kinetic energy of a current. He wrote it as  $\frac{1}{2}AC^2$ , and called A “the electro-dynamic capacity of the discharger.” This coefficient A was a most puzzling quantity at the time, and indeed was rejected by many so-called practical electricians, for long afterwards, though it is what we now familiarly speak of and measure as self-inductance. Difficulties felt by Lord Kelvin himself, before introducing it, are indicated on page 490, Vol. I. The whole theory of electric oscillations follows from the energy equation (page 543)—

$$d\left(\frac{1}{2}\frac{Q^2}{S}\right) = RC^2dt + d\left(\frac{1}{2}LC^2\right),$$

or, writing  $x$  for  $Q$ , simply—

$$L\ddot{x} + R\dot{x} + \frac{x}{S} = 0;$$

which, though now we write it glibly enough, was a considerable inspiration in 1853, more than half a century ago ; though the difficulties can hardly be appreciated without a study of the paper and of those which precede it.

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.

#### CONSERVATION OF ENERGY APPLIED TO ELECTRICITY.

A mode in which I have been accustomed to state to students the law of conservation as applied to current electricity, gradually introducing its several terms and thereby covering almost the whole area of the sub-

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ject, so far as fundamentals are concerned, may perhaps be briefly indicated.

Current is defined as  $C = \frac{dQ}{dt}$ ; and the work done when a quantity  $dQ$  is transferred down a step of potential is  $(V - V') dQ$ , or, as it may otherwise be written,  $ECdt$ . That is the energy to be accounted for, and represents the left-hand side of the equation. On the right-hand side we have, as the necessary first term, the frictional generation of heat; and the fact that the opposing force is accurately proportional to the first power of the current (which is Ohm's law), gives us as heat energy  $RCdQ$ , or, as it is commonly written,  $RC^2dt$ ; though of course, strictly speaking, the *square* of a current, just like the square of a time, is really nonsense. All such expressions correspond merely to a convenient grouping of quantities which in common sense and reality are otherwise grouped.

Parenthetically also the  $R$ , which was originally denominated by Kelvin "the galvanic resistance of the discharger," and commonly called resistance, is really only a *resistance coefficient*; it is the factor by which to multiply the current in order to give the frictional or irreversible opposition E.M.F.

So far the equation runs, simply,  $ECdt = RC^2dt$ ; and Ohm's law is an assertion that, at constant temperature,  $R$  is constant—*i.e.*, is not a function of current.

The next term that may be attended to is the inertia term, depending on electric acceleration—a term which is zero so long as the current is steady or obeying the first law of motion, but which is of great importance whenever the current strength, and therefore the magnetic field enclosed by it, varies. If by any means the number of effective lines of force encircled by the current is increased by  $dN$ , the corresponding increase of energy is  $CdN$ , and this therefore is the next term, and represents the energy of magnetic induction. So the equation now is  $ECdt = RC^2dt + CdN$ , and yields at once the fundamental dynamo and motor equation—

$$\frac{dN}{dt} = E - RC = \text{generated or back E.M.F.}$$

But as it stands the term  $CdN$  is very comprehensive, and may be separated out into self-induction, mutual induction, and induction by outside magnets:—

In self-induction the  $N$  is proportional to the current; it is equal to current multiplied by an appropriate coefficient  $L$ , dependent on the geometry of the circuit and on the material inside it. The self-induction term may therefore be written  $LCdC$ ; though there are cases, as in a commutated armature, when the  $L$  varies too, in which case we shall have an additional term  $C^2dL$ , and  $dL/dt$  will be a spurious or apparent resistance.

For mutual induction the corresponding energy term will be  $Cd(MC')$ .

For any number of magnetic poles the energy will be  $Cd(m\omega)$ , where  $\omega$  is the solid angle which the circuit subtends from each.

All these and other varieties are special cases of the term  $CdN$ .

So the equation becomes, in one important and most simple case—the case of any circuit whatever in which the current is not necessarily steady or is not yet fully established—

$$ECdt = RC^2dt + LCdC.$$

When there is no battery  $L \frac{dC}{dt} + RC = 0$ , or, if there is a discharging condenser in the circuit,  $= \frac{1}{S} \int_t^0 Cdt$ .

Now we come to a peculiarly Kelvin term, when the current decomposes

a liquid, giving rise to a quantity of decomposed material, the thermal value of whose reaction per gramme is  $\theta$ . Accordingly we shall have as the next energy term  $\Sigma(J \cdot \theta dm) = \Sigma(J\epsilon\theta)dQ = \Sigma(J\epsilon\theta)Cdt$ , where  $\epsilon$ , or  $dm/dQ$ , is the electrochemical equivalent of the substance; giving the well-known expression for polarisation E.M.F.,  $J\epsilon\theta$ , which dates actually from 1851.

Next there is the possible passage of a current across the junction of two materials to be considered; for *there* must exist the contact force which is responsible for the Peltier effect. It is true that Lord Kelvin identified this with a mere temperature-variation of the so-called Volta contact-force, whereas I regard that as not so much physical as chemical, and represented by the previous term instead of by this thermo-electric term. But undoubtedly there is a contact force at the junction of two metals, and immensely more at the junction of a metal and an insulator; though I do not suppose that any metal-metal force has the value which he was inclined to attach to it. If the temperature is uniform, the forces balance; but if not, we must add an energy term,  $\Sigma(\Pi)dQ$ .

Next comes the E.M.F. caused by the passage of a current along a slope of temperature—a phenomenon discovered by Lord Kelvin himself, first theoretically and then experimentally, and expressed by him as exhibiting a curious analogy with specific heat. The term for a simple circuit of two metals, with junctions at temperatures  $T_1$  and  $T_2$ , may be written  $dQ \int_1^2 (\sigma_a - \sigma_b) dT$ , where each  $\sigma$  is the Thomsonian specific heat-capacity of electricity in a substance.

The result deducible from all this is that a current generated by any E.M.F.  $E$ , applied to a circuit, is in general opposed, first by the inevitable frictional or ohmic resistance, and next by a number of possible opposition or polarisation forces which can be enumerated as follows:—

- (1) a self-induced back-E.M.F. which vanishes when the current is steady;
- (2) a magnetically-induced E.M.F., due to moving or varying magnets, or to reaction from other electric circuits;
- (3) a chemical polarisation force proper, at electrodes in liquids;
- (4) a Peltier force at every junction of two conductors;
- (5) a Thomson force, in all conductors with a gradient of temperature;
- (6) and the current is also opposed by reaction from any mechanical work done, as by an electric motor or any other contrivance developing energy  $W$  of any kind.

This is all quantitatively expressed by cancelling  $dQ$  from the energy equation above built up, and writing it thus:—

$$E = RC + LdC/dt + dN/dt + \Sigma(J\epsilon\theta) + \Sigma(J\Pi) + \Sigma J\sigma dT + dW/dQ.$$

#### 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 till 1856 (see pages 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



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heat of electricity," to an unexpected degree; and thermo-electric 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.

## THERMO-ELECTRICITY AND GAS THEORY.

The splendid way in which the second law of thermo-dynamics was applied to the phenomena of thermo-electricity, so as to establish the laws of a thermo-electric 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 thermo-electric 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 innumerable 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 page 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 under 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 any one 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



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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 page 502, Vol. I.).

Then, again, many people seem to think that absolute measurement has necessarily something to do with the metric system, and with the expression of quantities in the undoubtedly convenient c.g.s. units. On the other hand it has been urged by others that nothing expressed in arbitrary and perishable units, like centimetres, grammes, and the like, could be really absolute; but that a unit independent of convention and humanity and terrestrial considerations, one really eternal and the same throughout the universe, ought to be employed,—such, for instance, as the wave-length of a specified kind of light as the unit of length, the mass of a certain kind of atom as the unit of mass, and the period of some fundamental movement as the unit of time.

Undoubtedly the determination of any conventional unit in terms of these natural units has a distinct value; inasmuch as it enables the conventional unit to be reproduced or understood at any epoch in the world's history, or on any other planet. But it has nothing to do with the principle of absolute measure; nor is it necessary to employ these universal units for practical purposes. The connection between them and the conventional unit is a matter of careful determination, to be performed from time to time; and it is usually expressed in a phrase such as "measurement of the wave-length of light in metres," or "determination of the mass of an atom in grammes," rather than by the converse mode of expression, which to some would seem more appropriate. The fact is, however, that anything will do for a unit of absolute measure, so long as it is definite and constant,—a foot or a yard, is as good as anything else—and it is a great pity that the inventors of the metric system did not employ one of these old historical units instead of a brand new one supposed to be connected with the size of the earth—a comparatively unimportant and irrelevant magnitude. They could hardly have realised that the relation between any two units arbitrarily chosen must necessarily be incommensurable, and that historical continuity was far more valuable than approximate round numbers for relations not frequently wanted. Perhaps they did not realise, either, that old units based on the dimensions of the human body were psychologically sound.

The real essence of absolute measure is that every quantity shall be thoroughly and completely specified, so as to be of the right dimensions without any factor being omitted or understood. Thus, for instance, a foot-pound is not an absolute unit of work, unless the intensity of gravity be included as a factor; nor can a self-induction be expressed as so many earth-quadrants, without the introduction of the factor  $\mu$ . More than this. Any equation which is only true when the quantities are interpreted in a certain way—as, for instance, if length is expressed in metres, weight in tons, and the like—as

is done in so many engineering equations—avoids all the advantages of absolute measure : these expressions are not really equations at all, but arithmetical dodges or shorthand statements for immediately practical convenience. For such purposes they are often useful, though their constant employment tends to stunt the theoretical faculties, and seems to hinder perception of the advantages of the more important kind of equation, which, being in absolute measure, is true in every system of units whatever,—an equation in which the symbols represent the quantities themselves, and not some mere numerical relation in terms of a conventional unit.

Take a very simple example. If from a point at a height  $h$  above a sphere of radius  $r$  a tangent be drawn to the sphere, of length  $b$ , then it is absolutely true to say that  $h(2r + h) = b^2$  (for this is Euc. iii., 36) ; and this equation, applied to the Earth, gives the distance of the horizon in terms of the height of an observer above the sea. But for immediately practical use it would be convenient to throw it into some arithmetical form, such as this:  $1\frac{1}{4}$  times the square root of the height, in feet, will give the distance of the horizon, in miles.

Similarly, the velocity of free fall under gravity, in feet per second, is eight times the square root of the height, in feet ; and the *time* of fall is a quarter of the same square root.

These are all handy conventional rules, available for Englishmen ; whereas the fundamental equations on which they are based are true in every system of units, and true not only all over the world, but, when properly interpreted, throughout the universe. The interpretation of special cases, in handy form for practical purposes, is entirely legitimate and useful ; objection can only be raised when people urge that there is no other kind of equation. In these arithmetical contrivances units must be carefully expressed and adhered to. In every kind of absolute equation it is unnecessary to specify units at all : such an equation expresses a relation between the quantities themselves, irrespective of every arbitrary system of measurement.

#### 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 thermo-dynamics, 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.*). 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

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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, whose internal energy is  $E$ ,—

$$T = \left( p + \frac{dE}{dv} \right) \frac{dT}{dp} - \frac{dE}{dp} \cdot \frac{dT}{dv} \quad \dots \dots \dots (A).*$$

For an ordinary gas  $\frac{dE}{dv} = K + c \frac{dT}{dv}$ , where  $K$  is Laplace's cohesion constant ; and  $\frac{dE}{dp} = c \frac{dT}{dp}$  ; 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 whose molecules 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 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 actually 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 (Sept., 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

\* See *Encyclopædia Britannica*, article "Heat."

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

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

$$(p + K)(v - b) = RT,$$

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

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

or—

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

or—

$$T = \frac{1}{a} \left( 1 + \frac{K}{p} \right) \dots \dots \dots (5);$$

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 Waal's  $A\rho^2$ .

To get  $K$  we must perform a definite operation, say a sudden expansion  $\delta v$ , 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  $\delta T$ . It would be zero if the gas were perfect. If imperfect, the energy lost is  $K\delta v$ .

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  $\delta H$ ; the cooling will then be due only to internal work  $K\delta v$ ; and the heat change can be expressed as  $mc'\delta T$ , when eddies have subsided.

Thus we get—

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

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

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

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

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

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  $\delta p$ , and carrying with it ultimately the defect of heat  $\delta H$ , measured anyhow; for then—

$$T = \frac{1}{a} \left( 1 + \frac{\delta H}{v\delta p} \right) \dots \dots \dots (7')$$

But the expression for the absolute expansion term—

$$aT = \frac{p + K}{p} \dots \dots \dots (5')$$

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—

$$0.2375 \times 0.001293 \times (42 \times 10^6) = 0.001294 \times 10^6 \text{ ergs per c.c., for dry air.}$$

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  $0.208^\circ$ ; so that—

$$\frac{\theta}{\Pi} = \frac{0.208}{10^6 \text{ dynes per sq. cm.}}$$

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  $c'\rho\theta/\pi$  come out right as a pure number (being plainly a ratio of two energies when  $\rho$  is written  $m/v$ ); and the correction factor for air equals—

$$1 + 0.001294 \times 0.208 = 1.00027.$$

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)—

$$\frac{1.00027}{0.0036706} = 273.17.$$

#### INSTRUMENTS.

Of Lord Kelvin's many instrumental inventions I shall say nothing, beyond calling attention to the strongly mathematical basis on which they are founded. This fact was conspicuous to me long ago in connection with the infancy of the quadrant-electrometer—an instrument now older than most physicists, and so familiar that its unobviousness at first can hardly be appreciated; but the treatment of an electrometer as a variable condenser, which naturally led to the design of one flat surface moving over two other flat surfaces in the same plane, was, I judge, a distinctly mathematical, not an experimental, idea. It furnished at once a first approximation to the *moment* acting upon the upper flat surface, grotesquely called a "needle," and showed that the expression contained two factors—the difference of potential between the two quadrants, and the difference between their average potential and the potential of the needle above them; the needle being thereby urged to move so that the potential energy of the system should become a minimum.

#### ELECTRICAL THEORY OF MATTER.

On the great modern region of physics centering 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.*"



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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 whose major consequences 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.

Various interesting groupings can be easily calculated as stable patterns, and they tend to become more stable if they revolve regularly round the centre, or if sustained by a central group or particle. But although the calculable distribution and properties of such groups are profoundly suggestive and interesting, the behaviour of atoms of positive electricity with even a single electron inside each became instructive in the hands of Lord Kelvin.

His conception of the simplest possible electrical atom is that of a comparatively large positive uniform globe, neutralised by an equal charge of negative as a speck at the centre. Two such atoms, of course, exert no force on each other from a distance; but if they are of unequal size, and if the smaller one penetrates the bigger until the centre of the smaller is included in the circumference of the bigger, then each electron begins to be displaced, and, oddly enough, both are displaced in the same direction. If the penetration continues, both electrons enter the smaller sphere, and there adjust themselves and remain; the smaller one having robbed the bigger one of its charge, so that when they separate again the smaller one has become negative and the larger one positive. This fact is naturally supposed to correspond to some features in frictional electricity, or electrification by contact between substances of different kinds.

Lord Kelvin further goes on to calculate what would happen when one neutral atom, with an electron at centre, is approached by a small positive sphere without any neutralising electron charge. Displacement of the electron now begins long before contact; and when they approach within a certain distance, namely 1.89 times the larger radius distance between their centres, the electron in the bigger one is displaced a distance 0.63 times the radius from the centre; and there its equilibrium becomes unstable, so that it jumps across the intervening space "like a cork jumping out of a bottle" and enters the smaller sphere. It will then shoot through the smaller sphere and oscillate "perhaps ten or twenty times [or perhaps a million]," and ultimately settle down to rest at some place—not the centre—depending on the ultimate distance of the larger atom; which, however, has no power under any circumstances of regaining its charge from the smaller one. It is always the smaller that robs the bigger. This, indeed, is only a particular case of the general importance of "concentration" in the electrical theory. The mass of an electron depends upon its smallness—that is, upon its potential as well as upon its charge—and could be made enormous if it were assumed small enough; though, as we well know, to account for its actual mass a definite smallness is appropriate.

It is confessedly unlikely that an atom should have only one electron, but that which it is fairly easy (not too easy) to calculate for this simple case would happen in modified fashion in more complex cases, when the electrons were more numerous; and a vast number of possibilities suggest themselves, such as with ingenuity can be made to account for a great variety of the fundamental facts of chemistry. Ingenuity such as is needed for this difficult work, which at present is only in its infancy, has been conspicuously displayed in various well-known writings of Professor J. J. Thomson.

Meanwhile to return to Lord Kelvin.

Where there are more than one or two electrons in the atom, constituted as supposed above, they distribute themselves in stable form as follows—

Three form an equilateral triangle.

Four are at the corners of a regular tetrahedron.

Five may permit four to be in a plane, at the corners of a square, with one centrally perpendicular to the plane of the square, though more certainly stable if three are in a plane and two are on its central perpendicular, one on each side.

Six are stable at the corners of a regular octahedron ; they are also possibly stable in a plane if one is at the centre and five at the corners of a pentagon.

Eight can be at the corners of a cube, but with insecure stability, unless there are some at the centre too.

Twenty, at the corners of a regular dodecahedron, would either have to be *constrained* to lie on a spherical surface, or else would have to be well sustained by repulsive forces from the interior.

The calculation for space-distribution becomes complicated, and most attention has been paid to distribution in a plane ; wherein the criteria for stability, as depending either on central repulsion, or on centrifugal force due to regular rotation, has been admirably worked out by J. J. Thomson in a methodical and very instructive way.

A typically instructive and simple case is that of four electrons revolving round the centre. So long as they are revolving above a certain angular speed, they are stable when at the corners of a square ; but if the speed falls below a critical value this grouping becomes unstable, and they fall, with a convulsion and emission of energy, into a statical position of equilibrium at the corners of a tetrahedron.

It is in a direction typified by this simple example that we are looking for an explanation of some part of the phenomenon of spontaneous radio-activity.

#### 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. And as partly said before, I must omit all reference to his work on the size of atoms, on capillarity, and on the tides.

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. I shall refer specially to his Paper estimating the amount of matter in the sensible universe, on the basis of accounting for the stellar velocities met with, and the darkness of the stellar sky. In this Paper he argues that the average density of matter in the universe must ultimately be infinitesimal ; he reckons what it is in the space at present accessible to our aided senses ; and he conceives of this cosmos as formed by a clash of atoms. There has now appeared a posthumous Paper of his on a similar sort of subject, which looks as if it had been a preliminary arithmetical calculation preparatory to his paper on Gravitational Clustering. In this paper the 6,000 trillion tons of the earth's mass are supposed to be distributed regularly in cubic order, as cubic tons at rest throughout a vast space, and are then allowed to approach, under mechanical and easily reckoned conditions, until they simultaneously clash. This

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sort of procedure is plainly preliminary to an attempt to visualise the formation of a cosmos by a regulated and definite variety of the concourse of atoms.

At the Glasgow Meeting of the British Association in 1901 Lord Kelvin gave a most interesting abstract of his cosmic papers in the *Philosophical Magazine* for the same year; and parts of this are so characteristic of his mode of thought, and yet so comparatively easy to read, that I would call general attention to it. It is reproduced as Appendix D to the volume of his Baltimore Lectures, and may be abstracted further thus—

He begins by discriminating matter from ether, without denying that ether is material. [He chooses to do this by speaking of “gravitational matter”; but it is much simpler to employ the two terms “matter” and “ether” and to keep them distinct.]

He then takes his favourite sphere, of radius  $3 \times 10^{16}$  kilometres—being the distance at which a star has a parallax of one-thousandth of a second—and supposes distributed uniformly through it the mass which he images actually to exist within it, namely, one thousand million times the sun's mass. The main reason for postulating such a mass as that, is that by gravitational attraction it would give, in process of time, something like the stellar velocities which are actually observed; whereas a mass ten times as great would generate velocities much greater than any known stellar velocity. Actual stellar velocities could be generated under favourable conditions by such a mass, even though it were uniform. In so far as it was non-uniform—as of course it really is—the velocities generated would tend to be in places greater.

The density of matter in the above visible universe amounts to only  $1.6 \times 10^{-23}$  grammes per cubic centimetre; and if the whole mass were reduced to globules 2 centimetres in diameter, and of the sun's mean density, each globule would have 360 million cubic kilometres of space surrounding it; and the visual obstruction to an eye at the centre of such a set of globes, to light coming from outside, would only be  $\frac{1}{37}$ .

He then makes an important digression on the brightness of the starlit sky, and reckons that in order to make up with such disks anything like the solar brightness—to make, say, 3.87 per cent. of the whole sky bright with stars—the radius of a sphere thus full of matter at this density must be enlarged to  $3 \times 10^{27}$  kilometres. But then, with this radius, light would take  $3 \times 10^{14}$  years to travel from circumference to centre; and the life of any luminary is not likely to be greater than one hundred million years—it is probably less. So, since the time taken by light to travel from the outlying stars to the centre of such a gigantic sphere is three million times the life of any star considered as a luminous body, it follows that the greater part of the stars in such a globe as that would be dark, their luminous epoch being a mere episode in their total history. So that even this extension of the cosmos would fail to illuminate the sky.

He next reduces all the matter in the supposed universe to atoms, distributed uniformly throughout the originally assumed space with radius  $3 \times 10^{16}$  kilometres, and reckons what happens when they begin and continue to fall together, starting with the density of  $1.6 \times 10^{-23}$ , and shrinking by mutual gravitation, without any ethereal friction or other disturbance. In that case, he says, they would “take nearly 17 million years to reach 0.0161 of the density of water, and about two hours longer to shrink to infinite density at its centre.” [This is a quite characteristic sentence.] At the time when it reached the above-specified fraction of the density of water the velocity of the particles would be comparable with that of light, and in a moderate number of seconds “the whole surface of our condensing globe of gas or

vapour or crowd of atoms would begin to glow, shedding light inwards and outwards."

And he then concludes thus :—

"To come to reality, according to the most probable judgment present knowledge allows us to form; suppose at many millions, or thousands of millions, or millions of millions of years ago, all the matter in the universe to have been atoms very nearly at rest or quite at rest, more densely distributed in some places than in others, of infinitely small average density through the whole of infinite space. In regions where the density was then greater than in neighbouring regions the density would become greater still; in places of less density the density would become less; and large regions would quickly become void, or nearly void, of atoms. These large void regions would extend so as to completely surround regions of greater density. In some part or parts of each cluster of atoms, thus isolated, condensation would go on by motions in all directions not generally convergent to points, and with no perceptible mutual influence between the atoms until the density becomes something like  $10^{-6}$  of our ordinary atmospheric density, when mutual influence by collisions would begin to become practically effective. Each collision would give rise to a train of waves in ether. These waves would carry away energy, spreading it out through the void ether of infinite space. The loss of energy thus taken away from the atoms would reduce large condensing clusters to the condition of gas in equilibrium under the influence of its own gravity only, or rotating like our sun or moving at moderate speeds as in spiral nebulae, &c. Gravitational condensation would at first produce rise of temperature, followed later by cooling and ultimately freezing, giving solid bodies, collisions between which would produce meteoric stones such as we see them. We cannot regard as probable that these lumps of broken-looking solid matter (something like the broken stones used on our macadamised roads) are primitive forms in which matter was created. Hence we are forced, in this twentieth century, to views regarding the atomic origin of all things closely resembling those presented by Democritus, Epicurus, and their majestic Roman poetic expositor, Lucretius."

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. But Lord Kelvin clearly thought otherwise; so with this notable aspect of his life-work I conclude my address.