of these samples yielded, with peptone, Bacteria; not so the other two. All three were prepared with the utmost caution respecting atmospheric dust, &c. That, moreover, the positive result could not be caused by an accidental admixture of germs was amply proved by the often repeated control-experiments. It appears, therefore, that, besides the glucose and the peptone, a third substance is needed for generating Bacteria, a body present in the ordinary glucose (starch-sugar), but removed by purification. The nature of this body I have not yet been able to ascertain. But however important, this matter has no direct bearing upon the question of abiogenesis. For that this third unknown body cannot be (as some will probably presume) a germ, my control-experiments and also the above-described experiment, wherein the sugar was boiled with acid, do sufficiently prove.

D. HUIZINGA

Groningen, May 23

Flight of Birds

SOME time since I had occasion to ascend a mountain in the neighbourhood. The wind was blowing over the ridge-like crest of the mountain with a velocity of, I should say, ten or twelve miles an hour, sweeping with increased rapidity through certain transverse gorges cutting the ridge at right angles. In one of these I observed a hawk hovering in search of prey. In the midst of this rapid air current the bird remained apparently *fixed* in space, without fluttering a wing, for at least two minutes. After a time it gently changed its position a few feet with a slight motion of its wings, and then came to rest again as before, remaining apparently as motionless as the rocks around it. From my nearness to it a change of position of an inch would have been clearly visible, and yet except when it seemed to desire to change its point of observation no motion of any kind could be detected. How is this to be accounted for? Does a bird possess the power of giving an extremely rapid tremulous motion to its wings invisible even at a small distance, similar in its nature to the wing vibration of certain insects, which, as any one may have noticed, have a similar power of apparently fixing themselves in space over a flower, for example, notwithstanding a condsiderable amount of motion in the air in which they are suspended?

If any of your correspondents would kindly take the trouble to throw some light on these points they would greatly oblige one who is unfortunately placed out of reach of the ordinary means of reference.

J. GUTHRIE

Graaff Reinet, Cape Colony, April 2

THERMO-ELECTRICITY

THE subject I have chosen is one intimately connected with the names of at least two well-known members of this University—the late Prof. Cumming and Sir William Thomson. It possesses at present peculiar interest for the physicist; for, though a great many general facts and laws connected with it are already experimentally, or otherwise, secured to science—the pioneers have done little more than map the rough outlines of some of the more prominent features of a comparatively new and almost unexplored region. Some of its experimental problems are extremely simple, others seem at present to present all but insuperable difficulties. And it does not appear that any further application of mathematical analysis can be safely, or at least usefully, made until some doubtful points are cleared up experimentally.

The grand idea of the conservation, or indestructibility, of energy:—pointed out by Newton in a short Scholium a couple of centuries ago, so far at least as the progress of experimental science in his time enabled him to extend his statements:—conclusively established for heat at the very end of last century by Rumford and Davy; and extended to all other forms of energy by the splendid researches of Joule:—forms the groundwork of modern physics.

Just as, in the eye of the chemist, every chemical change is merely a re-arrangement of indestructible and unalterable matter; so to the physicist, every physical change is merely a transformation of indestructible energy; and thus the whole aim of natural philosophy, so far at least as we yet know, may be described as the study of the possible transformations of energy, with their conditions and limitations; and of the present forms and distribution of energy in the universe, with their past and future.

It is found by experiment that some forms of energy are more easily or more completely transformable than others, and thus we speak of higher and lower forms, and are introduced to the enormously important consideration of the degradation, or, as it is more commonly called, the dissipation, of energy. The application of mathematical reasoning to the conservation of energy presented no special difficulties which had not, to some extent at least, been overcome in Newton's time: but it was altogether otherwise with the transformations of energy. And it is possible that, had it not been for the wonderfully original processes devised by Carnot in 1824, we might not now have secured more than a small fraction of the immense advances which science has taken during the last thirty years.

For a transformation of heat we must have bodies of different temperatures. Just as water has no "head" unless raised above the sea-level, so heat cannot do work except with the accompaniment of a transference from a hotter to a colder body. Carnot showed that to reason on this subject we must have *cycles* of operations, at the end of which the working substance is restored exactly to its initial state. And he also showed that the test of a *perfect* engine (*i.e.* the best which is, even theoretically, attainable) is simply that it must be *reversible*. By this term we do not mean mere backing, as in the popular use of the word, but something much higher—viz. that, whereas, when working directly, the engine does work during the letting down of heat from a hot to a cold body; when reversed, it shall spend the same amount of work while pumping up the same quantity of heat from the cold body to the hot one. As a reversible engine may be constructed (theoretically at least) with any working substance whatever, and as all reversible engines working under similar circumstances must be equivalent to one another (since each is as good as an engine can be) it is clear that the amount of work derivable from a given amount of heat under given circumstances (*i.e.* the amount of transformation possible) can depend only upon the temperatures of the hot and cold bodies employed. In this sense we speak of Carnot's Function of Temperature, which is as imperishably connected with his name as is the Dynamical Equivalent of Heat with that of Joule.

Building upon this work of Carnot, Sir W. Thomson gave the first *absolute* definition of temperature—that is a definition independent of the properties of any particular substance. Perhaps there is no term in the whole range of science whose meaning is correctly known to so few even of scientific men, as this common word temperature. It would not, I think, be an exaggeration to say that there are not six books yet published in which it is given with even an approach to accuracy. The form in which the definition ultimately came from the hands of Joule and Thomson enables us to state as follows the laws of transformation of energy from the heat form.

1. A given quantity of heat has a definite transformation equivalent.
2. But only a fraction of this heat can be transformed by means even of a perfect engine: and this fraction is DEFINED as the ratio of the range through which the heat actually falls to that through which it might fall—were it possible to obtain and employ bodies absolutely deprived of heat.

This definition has two great advantages. 1st, The utmost amount of work to be got from heat under any circumstances of temperature is determined by precisely the same law as that assigning the work to be had from

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* Abstract of the Rede Lecture delivered in the Senate House, Cambridge, May 23, 1873.

water under similar circumstances of level. In this case the sea-level corresponds to what is called the Absolute Zero of temperature. [It is well to observe here that it is the potential energy of the water, not the quantity of water itself, which corresponds in this analogy to the quantity of heat. In this simple remark we have all that is necessary to correct Carnot's reasoning in so far as it was rendered erroneous by his assumption of the materiality (and consequent indestructibility) of heat.] 2nd, Temperatures thus defined correspond, as Thomson and Joule have shown by elaborate experiments, very closely indeed with those given by the air-thermometer—the absolute zero being about 274° of the Centigrade scale below the freezing point of water. I have made this digression as I shall have frequently to use the word temperature, and I shall always employ it in the sense just explained.

The subject of Thermo-electricity of course includes all electric effects depending on heat, but in this lecture I shall confine myself to the production by heat of currents in a circuit of two metals.

The transformation of heat into the energy of current electricity was first observed by Seebeck in 1820 or 1821. His paper on the subject (Berlin Ac., 1822-3, or Pogg. vi.) is particularly interesting, as he gives the whole history of his attempts to obtain a voltaic current from a circuit of two metals without a liquid, and the steps by which he was led to see that heat was the active agent in producing the currents he eventually obtained. In this paper Seebeck gave the relative order of a great number of metals and alloys in the so-called thermo-electric series, and showed that several *changes of order* occurred among them as the temperature was gradually raised.

In a note attached to this paper, Seebeck recognises that in this further discovery he was anticipated by Cumming (who seems, in fact, to have made an independent discovery of Thermo-electricity). Cumming showed that when wires of copper, gold, &c., were gradually heated with iron, the deflection rose to a maximum, then fell off, and was *reversed* at a red heat.

[Seebeck's original experiment and Cumming's extension of it were exhibited.]

You see that, keeping one of the copper-iron junctions at the temperature of the room and gradually heating the other, I produce a current which increases in intensity more and more slowly till it reaches a maximum, then falls off faster and faster till at last it vanishes and thereafter sets in the *opposite* direction. We are still far below the melting point of copper, yet further heating up to that point produces but little additional effect. The reason of this will be apparent from some facts to be described towards the end of the lecture. At the moment of maximum current the two metals are thermo-electrically *Neutral* to one another.—The temperature in the present case is about 280° C.

Seebeck pointed out that bismuth and antimony (to the choice of which he had been led by a very curious set of arguments) were very far removed from one another in the series, and therefore gave large effects for small differences of temperature. This is still taken advantage of in the Thermo-electric Pile, which, when combined with a sufficiently delicate galvanometer, is even now by far the most delicate thermometer we possess. It has recently enabled astronomers to detect and measure the heat which reaches us from the moon, and even from the brighter fixed stars. In the skilful hands of Forbes and Melloni this instrument was the effective agent in demonstrating the identity of thermal and luminous radiations—a step which, as regards the simplification of science, is as important as the discovery of magneto-electricity; and which was completed by Forbes when he succeeded in polarising radiant heat.

But when we come to look at this question from the point of view of transformation of energy, we have to ask *where* is the absorption, and *where* the letting-down of heat, to which the development of the current considered as a rise of energy is due. Very remarkably, an experiment of Peltier supplies us with at least part of the answer. Peltier showed that, given a metallic junction which when heated would give a current in a certain direction, then provided a battery were interposed in that circuit (initially at a uniform temperature) so as to send a current in that direction, the passage of the current *cooled* the junction, while a reversal of the current heated it. This, considering the circumstances under which it was made, and the deductions since drawn from it, is one of the most extraordinary experimental discoveries ever made. Water was frozen, in an experiment by Lenz, by means of the Peltier effect.

Here then is a reversible heat effect, and to it we may reasonably assume that the laws of thermodynamics may be applied; although from the very nature of the experiment the reversible effect must always be accompanied by non-reversible ones, such as dissipation by heat-conduction, and by heat generated in consequence of the resistance of the circuit. The latter of these is in general small in thermo-electric researches, but the former may have large values.

It is known from the beautiful experiments of Magnus that no thermo-electric current can be produced by unequal heating in a homogeneous circuit, whatever be the variations of section—a negative result of the highest importance. Sir W. Thomson, to whom we are indebted for the first and the most complete application of thermodynamics to our subject, showed that the existence of a neutral point necessitates the existence of some other reversible effect besides that of Peltier. And even if the circuit varied in section, the result of Magnus, just referred to, showed that this could only be of the nature of a convection of heat by the current between portions of the same metal at different temperatures. Thomson's reasoning is of the very simplest character, as follows:—Suppose the temperature of the hotter junction to be that of the neutral point, there is no absorption or evolution of heat there; yet there is evolution of heat at the colder junction, and (by resistance) throughout the whole circuit. The energy which supplies this must be that of the heat in one or both of the separate metals; but reasoning of this kind, though it proves that there must be such an effect, leaves to be decided by direct experiment what is the nature and amount of this effect in each of the metals separately. By an elaborate series of ingenious experiments Thomson directly proved the existence of a current convection of heat, and (curiously enough) of opposite signs in the first two metals (iron and copper) which he examined. In his own words, "Vitreous Electricity carries heat with it in an unequally heated copper conductor, and Resinous Electricity carries heat with it in an unequally heated iron conductor." This statement is not very easy to follow. It may perhaps be more intelligible in the form:—In copper a current of positive electricity tends to equalise the temperature of the point it is passing at any instant with that of the point of the conductor which it has just left, *i.e.*, when it passes from cold to hot it tends to cool the whole conductor; when from hot to cold, to heat it, thus behaving like a real liquid in an irregularly heated tube. The effects in iron are the opposite; and Thomson therefore speaks of the specific heat of electricity as being thus positive in copper and negative in iron. He gives a very remarkable analogy from the motion of water in an endless tube (with horizontal and vertical branches), produced by differences of density, due to differences of temperature. Here the maximum density of water plays a prominent part. Neumann has recently attempted, by means of the laws of motion of fluids, and the unequal expansibility of different metals, to give a physical explanation of thermo-electric currents. But, not to speak of the fact that positive electricity is by him considered as a real fluid, there are the fatal objections that his method makes no provision for the explanation of the Peltier, or of the Thomson, effect; and therefore cannot be looked upon as having any useful relation to the subject. Similar remarks apply to the attempt of Avenarius to account for thermo-electric currents by the variation with temperature of the electrostatic difference of potentials at the points of contact of different metals.

By employing the thermo-electric pile instead of the thermometers used by Thomson, Le Roux has lately measured the amount of the specific heat of electricity in various metals, and has shown that it is very small, or altogether absent, in lead. Strangely enough, though he has verified Thomson's results, he does not wholly accept the theoretical reasoning which led to their prediction and discovery.

One of Thomson's happiest suggestions connected with this subject is the construction of what he calls a thermo-electric diagram. In its earliest form this consisted merely of parallel columns, each containing the names of a number of metals arranged in their proper thermo-electric order for some particular temperature. Lines drawn connecting the positions of the name of any one metal in these successive columns indicate how it changes its place among the other metals as the temperature is raised. Thomson points out clearly what should be aimed at in perfecting the diagram, but he left it merely as a preliminary sketch. The importance of the idea, however, is very great; for, as we shall see, the diagram when carefully constructed gives us not merely the relative positions of the metals at various temperatures, with the temperatures of their neutral points, but also gives graphic representations of the specific heat of electricity in each metal in terms of the temperature, the amount of the Peltier effect, and the electromotive force (and its direction) for a circuit of any two metals with given temperatures of the junctions. In short, the study of the whole subject may be reduced to the careful drawing by experiment of the thermo-electric diagram, and the verification of Thomson's thermo-dynamic theory will then be effected by a direct determination either of Peltier effects or of specific heat of electricity at various temperatures, and their comparison with the corresponding indications of the diagram.

The diagram is constructed so that abscissæ represent absolute temperatures, and the difference of the ordinates of the lines for any two metals at a given temperature is the electromotive force of a circuit of these metals, one of the junctions being half a degree above, the other half a degree below, the given temperature.

It will be seen by what follows that nothing but direct measurement of the value of the specific heat of electricity at various temperatures can give us the actual form of the line representing any particular metal; but if the line for any one metal be assumed, those of all others follow from it by the process of differences of ordinates just described. So that it is well to begin by assuming the axis of abscissæ as the line for a particular metal (say lead, in consequence of Le Roux's result); and if, at any future time, this should be found to require change, a complex shearing motion of the diagram parallel to the axis of ordinates will put all the lines simultaneously into their proper form.

Thomson's theoretical investigation may be put in a very simple form as follows:—Let us suppose an arrangement of two metallic wires, one end of each of which is heated, their cold ends being united, and in which the circuit can be closed by a sliding piece or ring, always so placed as to join points of the two metals which are at the same temperature t. Let E be the electromotive force in the circuit, Π the Peltier effect, and σ_1, σ_2 the specific heats of electricity in the two metals. Then, if the sliding piece be moved from points at temperature t to others at $t + \delta t$, the first law of thermodynamics gives by inspection the equation

$$\delta E = J\,(\delta\Pi + \overline{\sigma_1 - \sigma_2}\,\delta t),$$

and the second law gives

$$0 = \delta\left(\frac{\Pi}{t}\right) + \frac{\sigma_1 - \sigma_2}{t}\,\delta t.$$

These equations show at once that, if there were no electric convection of heat, or if it were of equal amount in the two metals, the Peltier effect would always be proportional to the absolute temperature; and the electromotive force would be proportional to the difference of temperatures of the junctions; so that there could not be a neutral point in any case. In fact, the lines in the diagram for all metals would be parallel: and, on the former of the two hypotheses, parallel to the axis of abscissæ.

Eliminating $\sigma_1 - \sigma_2$ between the equations, we have

$$\delta E = J\frac{\Pi}{t}\,\delta t.$$

Now, by the construction of the diagram, $\frac{dE}{dt}$ is the difference of the ordinates of the lines for the two metals at temperature t. Hence, *whatever be the form of the lines for two metals*, the Peltier effect at a junction at temperature t is always proportional to the area of the rectangle whose base is the difference of the ordinates, and whose opposite side is part of the axis of ordinates corresponding to absolute zero of temperature. This area becomes less and less as we approach the neutral point, and changes sign (i.e., *is turned over*) after we pass it; the current being supposed to go from the same one of the two metals to the other in each case.

The electromotive force itself, being the integral of $\frac{dE}{dt}$ between the limits of temperature, is proportional to the area intercepted between the lines of the two metals, and ordinates drawn to correspond to the temperatures of the junctions respectively.

Again, the second of the preceding equations shows us that the difference of specific heats in the two metals is proportional to the absolute temperature and to the difference of the tangents of the inclinations of the lines for the metals to the axis of abscissæ. If we assume this axis to be the line of a metal in which the electric convection of heat is wholly absent, the measure of this convection in any other metal is simply the product of the absolute temperature into the tangent of inclination of its line to the axis. Thus, if the thermo-electric line for a metal be straight, electric convection is in it always proportional to the absolute temperature; and it is positive or negative according as the line goes off to infinity in the first or in the fourth quadrant. If the lines for any two metals be straight, and if one junction be kept at a constant temperature, the electromotive force will be a parabolic function of the temperature of the other junction—the vertex of the parabola being at the temperature of the neutral point of the two metals, and its axis being parallel to the axis of ordinates.

For the benefit of such of my audience as are not familiar with mathematical terms, I may give an illustration which is numerically exact. Let time stand for temperature, years corresponding say to degrees. Let the ordinate of one of the metals represent a man's income, that of the other his expenditure. The difference of these ordinates represents the rate of increase of his capital or accumulated savings, which here stands for electromotive force. As long as income exceeds expenditure, the capital increases; when income and expenditure are equal (*i.e.*) at a "neutral point," capital remains stationary, indicating, in this case, a maximum value; for in succeding years expenditure exceeds income, and capital is drawn upon.

P. G. TAIT

(*To be continued.*)