instead of a low one, as in the case of the V's. The wedges seem to shoot up in front of cyclones and V depressions and to travel along before them. On the front, or east side, the weather is very bright, and the wind is north-west and moderate, while the temperature is that due to excessive radiation. On the rear, or west side, where the barometer begins to fall, the wind turns to south-west, and the sky overcasts in that peculiar manner which first gives a halo, and then gradually becomes black, without true cloud as the cyclone approaches. At the extreme north point of the wedge a shower or thunderstorm is sometimes observed.

Straight Isobars.—In these the pressure is high on one side and low on the other, without any definite cyclone, the isobars running straight across the slope which joins the regions of high and low pressure. Straight isobars are never persistent, and the area which they have occupied is usually traversed by a cyclone of greater or less intensity. For forecasting purposes the indications are for cool, cloudy, unsettled weather, the wind from moderate to fresh, according to the gradients, to be followed soon by rain, as a cyclone forms or comes up.

Cols.—The col consists of a neck of low pressure between two anticyclones. The wind is always light and the weather quiet, but variable in appearance, owing to the local influence of radiation. Though the general position is sometimes nearly stationary, the weather is rather variable, owing to the tendency of the depression which lies on the north-west to develop a secondary in the col. Hence in forecasting, though it is possible to tell what the weather would be in the col at any moment, the future course of the weather is subject to much uncertainty.

Mr. Abercromby devotes a considerable portion of his work to a discussion of weather-types and sequence. With reference to Western Europe, there are at least four well-marked types of weather : 1. The Southerly, in which an anticyclone lies to the east or south-east of Great Britain, while cyclones coming in from the Atlantic either beat up against it or pass towards the north-east. 2. The Westerly, in which the tropical belt of anticyclones is found to the south of Great Britain, and the cyclones which are formed in the central Atlantic pass towards the east or north-east. 3. The Northerly, in which the Atlantic anticyclone stretches far to the west and north-west of Great Britain, roughly covering the Atlantic Ocean. In this case cyclones spring up on the north or east side, and either work around the anticyclone to the south-east, or leave it and travel rapidly towards the east. 4. The Easterly, in which an apparently non-tropical anticyclone appears in the north-east of Europe, rarely extending beyond the coast-line, while the Atlantic anticyclone is occasionally totally absent from the Bay of Biscay. The cyclones then either come in from the Atlantic and pass south-east between the Scandinavian and Atlantic anticyclones, or else, their progress being impeded, they are arrested or deflected by the anticyclone in the north-east of Europe. Sometimes they are formed to the south of the north-east anticyclone, and advance slowly towards the east, or, in very rare instances, towards the west.

Mr. Abercromby next explains the use of various aids to forecasting, and gives some detailed examples of successful and unsuccessful forecasts.

In concluding his work, Mr. Abercromby gives some remarks on forecasting generally, and points out that in many cases of small disturbances the minor features are so local that it is only the general character of the weather which can ever be forecast. Owing to the rapid nature of all meteorological changes, forecasts can never be issued very long in advance. The British forecaster labours under peculiar difficulties from his geographical position. Situated on the most outlying portion of Europe, and in the very track of storms which almost always advance from the westward, he has no intimation of an approaching cyclone till it is actually on him. Mr.

Abercromby's opinion is that, however carefully the relation of weather to isobars may be defined and the nature of their changes described, the judgment which experience alone can give to enable a warning to be issued must ever depend on the professional skill of the forecaster.

RADIANT LIGHT AND HEAT[1]
II.
The Theory of Exchanges

IT was known at a comparatively early period that if a body be placed in an enclosure of constant and uniform temperature, it will ultimately attain the temperature of this enclosure.

To fix our ideas, let us suppose that we have a chamber surrounded on all sides by walls which are kept at the temperature of boiling water ($100°$ C.), and let us further suppose for the sake of simplicity that there is no air in this chamber, so that no heat can be carried about by movable particles of gas. If under these circumstances we put a cold body into the chamber, it will ultimately reach $100°$, at which temperature it will remain. This is a statement of the doctrine of temperature equilibrium ; but this equilibrium may be of two kinds—for it may either be a statical equilibrium, in virtue of which two bodies at the same temperature cease to radiate to each other, or it may be a dynamical equilibrium, in virtue of which each of these bodies independently radiates heat to its neighbour, receiving back, however, just as much heat as it gives out. In either case the ultimate result will be equality of temperature, and the only difference is with regard to the physical machinery by which this is brought about. In the theory of statical equilibrium the behaviour of two bodies of equal temperature with respect to heat may be compared to that of a man with respect to money who is getting neither richer nor poorer, because he is neither giving away nor receiving any money, whereas in the theory of dynamical or movable equilibrium the comparison is with the man who is getting neither richer nor poorer because he is receiving back just as much money as he is giving out.

Now, we are all of us conversant with frequent examples of individuals of this latter class, but the condition of things in this world is such that we cannot have any permanent example of the former, and similar considerations might convince us that if radiant light and heat be in reality a kind of energy, the theory of a movable or dynamical equilibrium must be much more suitable to such a constitution of things than that of a statical or tensional equilibrium. Historically, however, the question of temperature equilibrium was not decided by considerations regarding energy, our conceptions of which were not then sufficiently advanced to be of much service to those who were engaged in the discussion.

As the subject is one of great theoretical and practical importance, we shall proceed to give a short account of the circumstances attending the origin and development of what is now known familiarly as the *theory of heat exchanges*. About a century ago Prof. Pictet of Geneva made the following experiment:—He took two concave metallic reflectors, and, reversing the ordinary mode of procedure, put ice or a freezing mixture in the focus of the one and a thermometer in that of the other, upon which the temperature of the thermometer was observed to fall. This effect would at once be explained if we could suppose that cold was a substantial entity capable of radiation and reflexion like heat. But it was immediately recognised that such an hypothesis is quite inadmissible, and Prof. Pierre Prevost, also of Geneva, was thus driven to propose for the explanation of this experiment the theory of a movable equilibrium of heat.

It is very evident that such a theory will explain the

fact. For, n accordance with it, bodies of the same temperature continue to radiate heat to one another, and hence the thermometer will radiate heat to the concave reflectors, which we may suppose to be of the same temperature as itself.

This heat will ultimately in great measure be reflected upon the ice or freezing mixture. Now, had this ice been of the same temperature as the other portions of the apparatus, it would have given back to the reflectors, and through them to the thermometer exactly as many heat rays as the latter had given to it.

But the ice being of a lower temperature, does not radiate back as many rays to the thermometer as this instrument gives out to the ice, and the temperature of the thermometer falls in consequence. It will be noticed that the same laws of reflexion and arrangement of mirrors that in the case where a hot body is placed in the one focus would have heated the thermometer in the other will, in the case of a cold body, cool the thermometer in the other ; so that, without resorting to the unlikely assumption that cold is a separate principle, we may explain the above experiment on the supposition that bodies of the same temperature radiate heat to one another, or, in other words, on the hypothesis of a movable equilibrium.

Prevost's first memoir was in 1791, and in 1804 Leslie published his inquiry into the nature and propagation of heat. He there demonstrated the fact that good reflectors of heat, such as metals, were bad radiators. Prevost, in a treatise on radiant heat, published in 1809, showed that Leslie's conclusions followed from his theory, remarking that in a place of uniform temperature a reflector does not alter the distribution of heat, which it would do if it possessed at the same time the power of being a good reflector and a good radiator. Prevost seems to have entertained very correct views upon this subject, inasmuch as he conjectures that a good reflector is a bad radiator because, as it reflects the heat from without, so it also reflects the heat from within. Internal radiation, we shall afterwards see, follows as a consequence from the theory of exchanges.

Some time afterwards Dulong and Petit published their well-known memoir on radiation, which affords evidence of a peculiar kind in favour of the theory of exchanges. To illustrate the bearing of the experiments by Dulong and Petit on this theory, let us imagine that we have a hollow, blackened enclosure which is at the same time a vacuum, and that we have in its centre a large thermometer likewise blackened, the temperature of which is higher than that of the enclosure. We are supposed to be engaged in observing the rate of cooling of this thermometer, or, in other words, the excess of its radiation to the enclosure above that of the enclosure to it. Now let A denote the total radiation of the thermometer, which we may imagine to have the temperature a. Also let B denote that of the enclosure, which we may imagine to have the temperature b. Then $A - B$ will, by the theory of exchanges, represent the rate of cooling of the thermometer. In the next place let the thermometer have the temperature b and radiation B, while the enclosure has the temperature c and radiation C. Here $B - C$ will, by the theory of exchanges, represent the rate of cooling of the thermometer. Finally, let a be the temperature of the thermometer, and c that of the enclosure. Then $A - C$ will, on the theory of exchanges, represent the radiation or rate of cooling of the thermometer. Now $A - C = (A - B) + (B - C)$, that is to say, the rate of cooling in the third case will represent the sum of the two preceding rates *if the theory of exchanges be true*.

It was found by Dulong and Petit that this was actually the case, for with $a = 140°$ and $b = 80°$, $A - B$ was found to be 2·17.

Again, with $b = 80$ and $c = 20$, $B - C$ was found to be 1·40.

Finally, with $a = 140$ and $c = 20$, $A - C$ was found to be 3·56. Now this is very nearly equal to 3·57, or the sum of the two preceding rates, so that the evidence deduced from these experiments is decidedly in favour of the theory of exchanges.

In 1848 Provostaye and Desains made a definite advance towards a clearer conception of this theory. It may be stated thus. If we place a thermometer in our hypothetical chamber of constant temperature it is well known that the instrument will give the same indication, in whatever manner we alter the substance of the walls, provided only that their temperature be left the same.

It follows from this that the heat radiated, together with that reflected from any portion of the walls, forms a constant quantity independent of the nature of the substance of which this portion is composed. We thus see that it is not correct to assert that the reflective power of a substance is inversely proportional to its radiative power, the true statement being that in the case of an enclosure of constant temperature such as that we are now considering, the sum of the heat radiated and reflected from any portion is a constant quantity.

It was likewise perceived by Provostaye and Desains that this constant sum, while equal to that of a lampblack radiator, must be unpolarised, since heat from lampblack is unpolarised ; and hence that, since the reflected heat is frequently polarised, the radiated heat must be polarised in an opposite manner, that is to say, in a perpendicular plane, in order that the sum of the two should be virtually unpolarised. Experimentally these observers found this to be the case.

It will thus be seen that the inquiry had now reached a stage at which a perfectly clear conception had been formed of the character with respect to intensity and polarisation of the heat emanating from any portion of the surface of an enclosure of constant temperature.

No attempt had however been made to split up the heterogeneous body of heat into its constituent wavelengths, nor was it perceived that an extension of the argument must necessarily lead to a separate equilibrium for every individual description of heat.

Internal radiation too, as a subject for experiment (if we except the remark made by Prevost), appears to have been overlooked, and its essential connexion with the theory of exchanges does not appear to have been perceived.

In March, 1858, I communicated to the Royal Society of Edinburgh a memoir in which these desiderata were supplied. In this memoir it was shown by a simple process of reasoning that the heat-equilibrium must hold for every individual description of heat, and that as a consequence this would lead to various conclusions, all of which were experimentally verified. The following facts were thus established :—

(1) The radiating power of thin polished plates of different substances was found to vary as their absorbing power : so that the radiation of a plate of rock-salt was only 15 per cent. of the total lamp-black radiation for the same temperature.

(2) It was shown that the radiation from thick plates of diathermous substance is greater than that from thin plates, no such difference being manifested when the substances are athermanous.

(3) It was found that heat radiated by a thin diathermous plate is less transmissible through a screen of the same material than ordinary or lamp-black heat, the difference being very marked in the case of rock-salt.

(4) Lastly, heat from a thick diathermous plate is more easily transmitted through a screen of the same material than that from a thin plate.

All these facts can be explained by a legitimate extension of the theory of exchanges.

Let us recur to our hypothetical chamber, outside the walls of which we may suppose there is a boiling-water arrangement, in virtue of which these walls are kept at

the temperature of 100° C. The inside we shall suppose to be a vacuum. Let us in the first place hang up in this chamber two thermometers, one covered on the outside of its bulb with lamp-black, the other with polished silver. The former of these will absorb all the rays that fall upon it from the walls of the chamber, the latter, on the other hand, will absorb very few of these rays. Ultimately, however, both thermometers will attain the temperature of the walls. Since, therefore, according to the theory of exchanges the equilibrium of temperature is kept up by an equality of absorption and radiation, it is manifest that the radiation from the lamp-black thermometer must be great, because the absorption is great, and the radiation from the silvered thermometer small, because the absorption is small.

It will be noticed that this connexion between the two qualities, absorption and radiation, is deduced from a hypothetical case where everything is at a constant temperature. To prove it experimentally we may without any breach of scientific propriety take out the two thermometers from the enclosure, exposing them to a lower temperature, and noticing their velocity of cooling, when it will be found that the blackened thermometer cools more rapidly than the silvered one.

Or we may allow their radiation to fall upon a thermopile, and to be registered by a galvanometer, when it will be found that the indication of the galvanometer will be much greater for the blackened than for the silvered thermometer.

Let us next hang up in our enclosure a plate of glass and one of polished rock-salt.

The plate of glass will absorb all or nearly all the rays of dark heat that fall upon it from the sides of the enclosure. The plate of rock-salt will, on the other hand, absorb only a few of these rays. A similar argument to that already given will enable us to see that if the theory of exchanges be true, the radiation from a plate of rock-salt must be decidedly less than from one of glass, and this is found to be the case.

Next, let us hang up two plates of rock-salt, a thick one and a thin one. The thick one will absorb more rays than the thin one, and we shall therefore expect it to radiate more. This, too, will be found to hold experimentally, thus proving the fact of internal radiation. On the other hand, we shall observe no sensible difference if we hang up two plates of glass, one thick and one thin, the reason being that the thin plate of glass already absorbs all the heat which falls upon it, so that no increment of absorption, and hence of radiation, can take place by increasing the thickness. We thus see that it is only in the case of diathermanous bodies that the radiation increases with the thickness, while for athermanous bodies there is no such increase.

We are now in a better position for realising what takes place in our hypothetical enclosure.

There is a stream of heat from the walls which falls upon any substance which we may introduce into our chamber. Now this stream is not altered in intensity by altering either the shape or substance of the walls. Suppose, for instance, that they are of polished metal instead of being covered with lamp-black, then, while the heat radiated from them will be less, the reflexion of this heat will be so bandied backwards and forwards between these walls as to swell up the total amount to an equality with the lamp-black radiation, the only difference being that in the lamp-black radiation there is little or no reflexion, while in the other there is much reflexion and comparatively little radiation. Nor will the stream from the walls be altered by hanging up a plate of any substance between them and the body we introduce. For the plate will radiate on its own account just as much heat as it absorbs from the walls, so that the joint radiation of the two will be the same as if the plate were taken away.

Our remarks have hitherto applied only to the total intensity of this stream of radiant heat, and not to its quality—that is to say we have left out of consideration the specific mixture of various kinds of rays differing either in wave-length or in polarisation which go to make up the whole heterogeneous radiation. Now a little reflection will convince us that this specific mixture—this *quality* of the radiation-stream—must, as well as its *quantity* remain the same under any change made in the shape or substance of the enclosure. For suppose that we introduce a thermometer coated with some substance which exercises a selective absorption for certain rays of the stream, and not for others, then a change of quality would mean for this thermometer a change of absorption as truly as if there were a change of quantity. But by the theory of exchanges the absorption must remain the same, being equal to the radiation, and hence this can only be brought about by the quantity and the quality of the radiation-stream remaining each unaltered whatever change be made in the walls of the enclosure, or whatever substance be introduced between these walls and the thermometer. Carrying out this train of thought, we see why, as was proved by Provostaye and Desains, the sum of the radiated and reflected heat from any portion of the walls must be unpolarised, the reason being that the radiated heat from lamp-black is unpolarised and the one radiation must be equal to the other not only in quantity but in quality also. Again, the radiation of any surface or of any plate must be equal to its absorption, both as regards quantity and quality, so that the stream of heat may emerge from the surface or from the plate unaltered both in quality and in quality.

Thus the putting up of a plate between the walls and our coated thermometer will produce no effect, inasmuch as the stream of radiant heat which falls upon the coating will be unaltered both in quantity and quality by the interposition of the plate. We thus see why the radiation from a thin plate of rock salt should be of a quality which renders it much absorbed by a cold plate of the same material, the reason being that a body radiates that kind of heat which it absorbs.

We see, too, why heat from a thin plate of rock salt should be more absorbed by a cold screen of this material than that from a thick plate, inasmuch as the former consists of that kind of heat which is strongly absorbed, even by a thin plate, while the latter contains likewise a number of other rays which are not so strongly absorbed.

The conclusion to be derived from these remarks is that we have in reality a separate equilibrium for every description of heat, an equilibrium which is independent of the shape of the enclosure and of the substances of which it is composed. Furthermore, the stream of radiant heat may be supposed to circulate in the interior of a substance such as glass, water, or even metal, the radiation of each particle which it meets being exactly equal to its absorption, so that the stream proceeds through the interior, being virtually the same at one part of its path as at another. Again, it can be shown that it is essential to equilibrium that in the interior of a substance this stream of heat should be proportional to the *square of the refractive index*. That is to say, in an enclosure containing glass whose refractive index is 1·5 the stream of radiant heat in the heart of the glass will be 2·25 times greater than that proceeding through a vacuum ; we cannot, however, tell what takes place in the heart of a crystal. It also appears that, for an enclosure of given temperature, the stream of a given kind of heat has a definite value, the amount of this increasing as the temperature increases.

We are, however, ignorant of the exact function of the temperature which expresses the value of this stream, but we know that this value increases more rapidly for the more refrangible rays of the spectrum than for those of greater wave-length and smaller refrangibility.

We now come to consider the luminous rays, and here

the wondrous power of the eye can aid us to an extent far surpassing that of the most delicate pile and galvanometer for the dark rays.

Wollaston and Fraunhofer were the first to show that in the solar spectrum numerous dark bands occur, which indicate the absence of certain definite kinds of light.

Sir David Brewster afterwards showed that similar bands make their appearance when the spectrum is made to pass through nitrous acid gas, and it was thus rendered probable that the bands which appear in the solar spectrum were due to absorption likewise.

Brewster, J. Herschel, Talbot, Wheatstone, and W. A. Miller were amongst the first to make observations upon the luminous spectrum obtained by heating various substances, and it was soon perceived that such spectra consist of bright lines on a dark background, and thus appear to be a reversal of the solar spectrum, which consists of dark lines on a bright background. Fraunhofer was the first to notice a coincidence in spectral position between the dark double line D occurring in the solar spectrum and the bright yellow flame produced by incandescent sodium. Swan afterwards showed that the correspondence between the two black lines and the two bright lines is very exact, and that a very small quantity of salt is sufficient to call forth the bright lines. Ångström (*Phil. Mag.*, May, 1855), referring to a conjecture of Euler that a body absorbs all the series of oscillations which it can itself assume, expresses his conviction that the same body, when heated so as to become luminous, must emit the very rays which at ordinary temperatures are absorbed, and that the explanation of the dark lines in the solar spectrum embraces that of luminous lines in the electric spectrum. Probably, however, the first to give definite expression to this conception was Prof. Stokes, who, about the year 1850, commented on an experiment recently made by Foucault. This observer had found that, when a voltaic arc formed between charcoal poles was placed in the path of a beam of solar light, the double line D is thereby rendered considerably darker. If, on the other hand, the sun and the arc jut out the one beyond the other, the line D appears darker than usual in the solar light, and stands out bright in the electric spectrum. Thus the arc, remarks Foucault, presents us with a medium which emits the rays D on its own account, and which at the same time absorbs them when they come from another quarter.

The explanation given by Stokes of this experiment assumes that the vapour of sodium must possess, by its molecular structure, a tendency to vibrate in periods corresponding to the degrees of refrangibility of the double line D.

Hence the presence of sodium in a source of light must tend to originate light of that quality. On the other hand, vapour of sodium in an atmosphere around a source must have a great tendency to absorb light from the source of the precise quality in question.

In the atmosphere around the sun, therefore, there must be present vapour of sodium, which, according to the mechanical explanation thus suggested, being particularly opaque for light of that quality, prevents such of it as is emitted from the sun from penetrating to any considerable distance through the surrounding atmosphere.

It appears, from the historical sketch here given, that two independent lines of research were progressing towards the same conclusion. The one of these had for its basis the theory of exchanges, and endeavoured theoretically and experimentally to render this theory complete. The other was founded upon spectroscopic investigation, and endeavoured to apply to light an analogy deduced from sound, believing that, just as a string or tuning-fork when *at rest takes up* that *note* it gives out when *struck*, so a molecule when *cold absorbs* that *ray* which it gives out when *hot*.

In October, 1859, Prof. Kirchhoff of Heidelberg made a communication to the Berlin Academy on the subject of Fraunhofer's lines. His observations were made on this occasion by an examination of the spectrum of coloured flames made by Bunsen and himself, and he derived from them the following conclusions:—He concluded that coloured flames in the spectrum of which bright sharp lines present themselves so weaken rays of the colour of these lines, when such rays pass through the flames, that, in place of the bright lines, dark ones appear as soon as there is brought behind the flame a source of light of sufficient intensity in the spectrum of which these lines are otherwise wanting. He concluded further that the dark lines of the solar spectrum which are not evoked by the atmosphere of the earth exist in consequence of the presence in the incandescent atmosphere of the sun of those substances which in the spectrum of a flame produce bright lines in the same place.

Carrying out this train of thought, Kirchhoff, about the end of 1859, shows that as a mathematical consequence of the theory of exchanges, a definite relation must subsist between the radiating and absorbing power of bodies for individual descriptions of light and heat.

It will be noticed in this historical statement that I made my first experiments on dark heat; afterwards I proceeded to the subject of light. Meanwhile, however, Kirchhoff had independently been led to experiment in this direction, and, although his memoir slightly preceded mine in publication, I shall now give the experiments which I was led to make, more especially as they are very similar to those of Kirchhoff. In February, 1860, I communicated to the Royal Society of London a paper in which I showed that the light radiated by coloured glasses is intense, in proportion to their depth of colour, transparent glass giving out very little light. I also showed that the radiation from red glass has a greenish tint, while that from green glass has a reddish tint. It was likewise shown that polished metal gives out less light than tarnished metal and that when a piece of black and white porcelain is heated in the fire the black parts give out much more light than the white, thereby producing a curious reversal of the pattern.

Finally, in a paper communicated in May of the same year, it was shown that tourmaline, which absorbs in excess the rays of light polarised in a plane parallel to the axis of the crystal, also radiates, when heated, this kind of light in excess, but that when it is viewed against an illuminated background of the same temperature as itself, this peculiarity disappears. All these facts are a natural consequence of a movable equilibrium of temperature holding separately for every variety of heat, the word " variety " embracing any difference either in wave-length or polarisation which is the cause of unequal absorption.

The theory of exchanges, as here exhibited, has been founded upon the fact that in an enclosure of constant temperature all bodies will ultimately attain the temperature of the walls of the enclosure. This is the experimental foundation upon which our structure has been built, and we have not attempted to work under it or to find whether in its turn it be not founded upon some principle of a still deeper and more fundamental nature. We shall now briefly indicate that such is the case, and that this law of ultimate equality of temperature is a consequence of the theory of energy in which we are told that no work can possibly be got out of heat which is all at the same temperature. For if the ultimate result in our enclosure should be a variety of temperatures, then it would be possible to utilise this temperature-difference and convert heat into work, so that there would practically result a case of perpetual motion. Now, it is one of the most fundamental axioms of physical science that such a motion is impossible.

I have endeavoured to make use of this method of viewing the problem, in order to point out what forms

the natural limit to our conception of a movable heat-equilibrium. Suppose, for instance, that we have a large spherical chamber of the temperature of 100° C., and that this chamber is removed from all gravitating influence, so that a solid spherical body, also of the temperature of 100° C., may rotate on its axis in the centre of this chamber without requiring the support of an axle. The chamber may likewise be supposed to be void of air, so that there is nothing but the ether to bring the revolving body to rest. Now, if a sort of diaphragm or rim be introduced into the chamber, as in Fig. 9, the result will
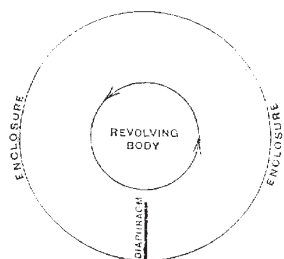

FIG. 9.

be that the particles of the enclosure to the left of the diaphragm will only receive heat from that portion of the revolving body which is approaching them, while those to the right of the diaphragm will only receive heat from those portions of the same body which are receding from them.

But the wave-length of light is altered in one way by a body which is approaching us, and in another way by a body which is receding from us, so that the particles to the left of the diaphragm will, in reality, receive a different kind of radiation from those to the right. Here, then, we have something which upsets the temperature equilibrium, and we may even conceive that the particles to the left of the diaphragm will absorb more heat, and therefore become hotter than those to the right. If so, we shall have the possibility of creating work out of this difference of temperature, or, in other words, of starting a kind of perpetual motion.

We thus begin to see that, somehow, the revolving body must lose as much energy as we gain by means of these differences of temperature, for otherwise we should have the transmutation of heat originally of the same temperature into work, which we cannot admit. But this means that a revolving body placed under these circumstances must gradually part with its energy of visible motion, although it is not in contact with anything else than the ethereal medium.

Before concluding this branch of my subject let me say a few words about phosphorescence and fluorescence.

It is well known that certain substances remain luminous—that is to say, continue to emit light for some time after they have been exposed to the light of the sun or of some other powerfully luminous body. Such substances are said to be *phosphorescent*.

It is likewise known that other substances, more especially certain liquids, emit light in a peculiar way while the luminous source acts upon them, but do not enjoy this property for an appreciable time after it has been withdrawn. Such bodies are said to be *fluorescent*.

It is manifest that the difference between phosphorescence and fluorescence is one of time, the bodies implied by the first term continuing to give out light for some time after the exciting source is withdrawn, while those implied by the second do not retain this property for an appreciable time after the withdrawal of the luminous source. Prof. Stokes, who has done much to advance this subject, has shown that the exciting cause of phosphorescence and fluorescence is more especially the rays of high refrangibility—even rays beyond the violet of the

visible spectrum. On the other hand, the rays which the body gives out are generally of a lower refrangibility than the exciting rays. Hence invisible rays may, by means of a phosphorescent or fluorescent body on which they fall, render themselves visible. This phrase, however, is perhaps not strictly correct, inasmuch as, before becoming visible, they have been changed into other rays of lower refrangibility.

The object of introducing this subject here is rather, however, to discuss its bearing upon the theory of exchanges than to treat it as a separate branch of inquiry; and I may commence by remarking that at first sight it seems to contradict the general law that the quantity and quality of the light and heat given out by a body depend upon its temperature, and upon this only. Thus, a thermometer at 100° C. is supposed to radiate from the surface of its bulb heat which will be the same in quantity and quality whether the instrument has been heated by the sun's rays or by plunging it into boiling water. Now in such a body as luminous paint we have the light which we usually associate with a high temperature given out long after the sun has ceased to shine upon it, and when we know its real temperature to be that of the bodies around it. Do phosphorescent bodies form, therefore, an exception to the general law which represents the quality of the radiant heat as a function of the temperature?

I think we shall find, on examination, that in this general law it is taken for granted that no chemical change is taking place in the body in question, and no other molecular change than that implied in the cooling of the body. In a chemical action we have generally the transmutation of chemical energy into heat, and in molecular action we have generally the transmutation of molecular energy into heat likewise. That is to say, the body undergoing these changes becomes heated, and so gives out light and heat peculiar to the temperature to which it has been raised. But there seems to be no reason why molecular energy should not be somehow changed at once into radiant light and heat. In this case there would no doubt be an apparent breaking of the law above mentioned, which associates a certain temperature with a certain quantity and quality of radiant heat, but the exception would be only apparent. for, as we have stated, the law presupposes that no molecular change of this nature is taking place.

In like manner our argument regarding an enclosure of a constant temperature and the theory of exchanges in general, while it allows of the greatest possible variety of substance and form in the enclosure, virtually assumes that no chemical or molecular change is going on amongst the substances introduced. We are, in fine, supposed to be dealing with radiant energy and absorbed heat, and with no other form of energy, and indeed we have just seen that if we have a body in visible motion in the enclosure, the equilibrium no longer holds.

Thus we get rid of the difficulty by rejecting the bodies in question as not fulfilling strictly our requirements. No doubt the phenomena of phosphorescence and fluorescence are comparatively trivial exceptions, but we may imagine an enclosure in which all the substances are at the temperature of 100°, while some one substance is gradually changing its molecular state, until at length we have a violent explosion accompanied with light and heat. Here the result is so obvious that we have no hesitation in recognising such a body as an exception not contemplated by the theory of exchanges. We are persuaded that phosphorescent bodies are equally an exception, the only difference being that the character of this exception is not nearly so pronounced.

It has been pointed out by Prof. Tait that the conclusions of the theory of exchanges are only statistically true. That is to say, if we take a sensible time, such as a second, and a sensible quantity of any substance, such as a milligramme, then in an enclosure of constant temperature the absorption of

that matter during one second is equal to its radiation during the same time, and this holds for all kinds of heat. On the other hand, if we take a single molecule and a billionth of a second, we cannot affirm the same equality. This is no doubt correct ; in fact, if the equality between radiation and absorption were to hold for the smallest conceivable mass and the smallest conceivable increment of time, our equilibrium would in reality be a tensional one instead of being movable or dynamical. I shall conclude by repeating the words of Tait (" Heat," p. 253) :—" It is vain, at least in the present state of science, to look for a truly *rigorous* investigation of the relation between radiating, absorbing, and reflecting powers. In all the professedly rigorous investigations which have been given the careful reader will detect one or more steps which are to be justified only by the statistical process of averages."   BALFOUR STEWART

(To be continued.)

THE LIFE OF AQUATIC ANIMALS AT HIGH PRESSURE [1]

THE magnificent expeditions of the *Talisman* and the *Travailleur* have called the attention of naturalists and physicists to the conditions of life at the bottom of the sea. A learned physiologist, Dr. Regnard, has conceived the happy idea of studying experimentally these condition of life at high pressure. With apparatus designed by M. Cailletet, he has subjected aquatic animals to enormous pressure, such as prevails in the depths of the ocean, and has examined the results when those inhabiting the surface are suddenly placed at great depths. Since his first experiments Dr. Regnard has invented an ingenious method by which he can see, notwithstanding the great pressure, what goes on inside the apparatus.
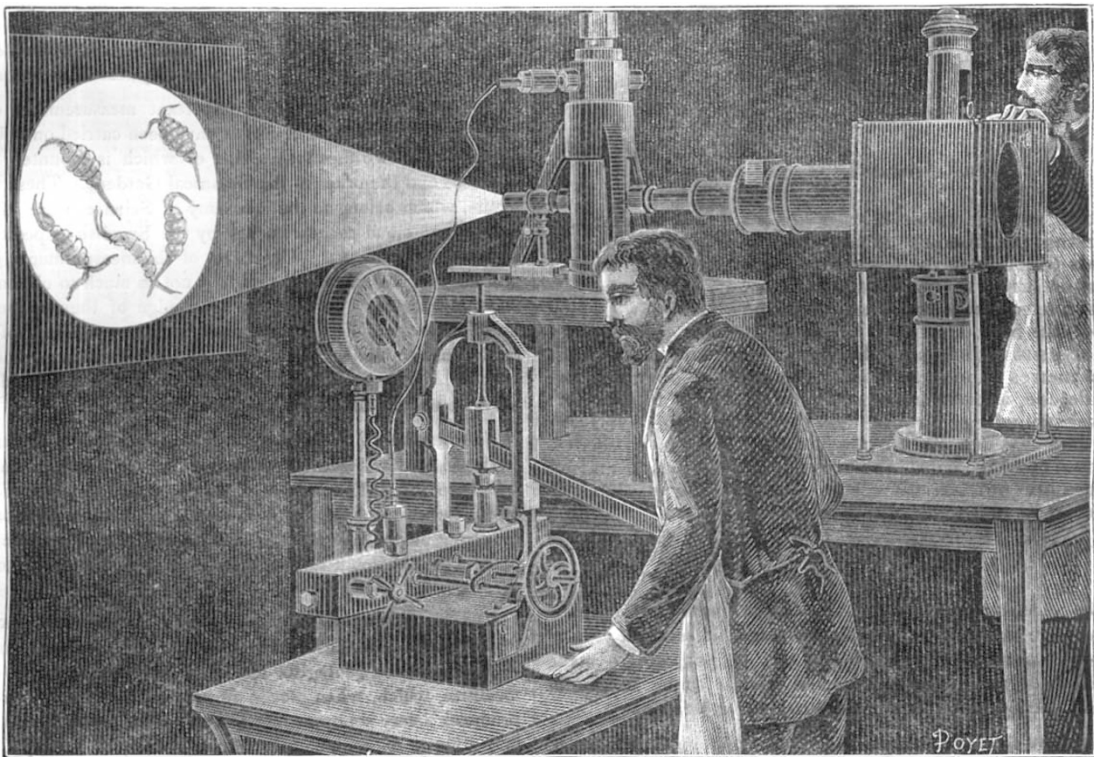

FIG 1.—General View of Dr. Regnard's Apparatus.

Hitherto the operator simply placed the animals on which he experimented in the iron block of the Cailletet pump, and subjected them to the pressure corresponding to a given depth ; he then released them, sometimes very slowly (after several days), sometimes rapidly and even instantly. He examined then, physiologically and microscopically, the lesions produced. But all the intermediate stages between the entrance of the animals and the time they were taken out escaped the observer. But now the apparatus in Fig. 1 allows him to follow each minute the effects. The following is Dr. Regnard's description of his apparatus to the Academy of Sciences :—

Two holes are pierced through and through across the lower part of the Cailletet block, M (Fig. 2). In these two holes, placed in a straight line, are inserted two tubes in *r* and *r'*. These are hollow, and in each of them is solidly fixed a cone of quartz, B, the extremity of which joins the edges of the hole which is pierced in the screw nut E. A ray of light thrown by the orifice *r* will thus traverse the apparatus and emerge at *r'*. Experiments have shown that a similar apparatus will resist easily a pressure of 650 atmospheres, which represents that of the greatest depths that have been dredged—about 6500 metres. Across one of the quartz cones are sent the concentrated rays of an electric lamp. These rays cross the block full of water, and emerge on the opposite side, where they are received by an achromatic object-glass which projects them on to a screen. The observer therefore works at a distance from the apparatus, where he is sheltered from all danger (Fig. 1). This arrangement has another advantage. The orifice pierced at *r* is hardly half a centimetre in diameter, and one can experiment with animalculæ so small as to be scarcely perceptible

[1] From *La Nature*