quick in their movements, which increased the difficulty of observation, but that the bees themselves were the agents, in making the holes, there can be no reason to doubt.

Highfield, Gainsborough, December 21 F. M. BURTON

Photography Foreshadowed

THE first prophetic allusion to the photographic art, the discovery of which was to take place eighteen centuries later, is perhaps found in the story of the miraculous occurrence told in the life of St. Veronica.

The second instance is about the year 1690; but intermediate instances may probably be found. I extract from the works of Fénelon [1] the following passage from an imaginary voyage in 1690:—"There was no painter in the country, but when anyone wished to have the portrait of a friend, a beautiful landscape, or a *tableau* which represented any other object, he put water in large basins of gold or silver; then placed this water opposite the object he wished to paint. Soon the water congealing became like a looking-glass, in which the image of that object remained ineffaceable; and it was a picture as faithful as the brightest mirrors." One could wish that the author had entered into detail as to the manner "of placing this water opposite the objects he wished to paint."

The third instance is about 1760, that is seventy years later, and seventy-nine years before 1839, the date of Daguerre's discovery. It is reported [2] by Ed. Fournier, who extracted from what he calls "un assez mauvais livre," written by a certain Tiphaigne de la Roche [3], the entire passage, extremely curious, but rather long. This passage contains many details. The "water" of Fénelon is replaced by "a material very subtle, very viscous, and very ready to dry and harden." "They" (certain "elementary spirits") coat with this material a piece of linen, and present it to the objects which they wish to paint," &c.

In the two last examples the pictures formed reproduce the images of the objects, with their natural colours and their forms, so that the objects are seen as if reflected in a mirror. The photographs of the present day are still very far from this ideal perfection, which, however, they will probably never cease to approach without ever being able to reach.

Rotterdam J. A. GROSHANS

Average Annual Temperature at Earth's Surface

HAVING lived for many years both in the southern and northern hemispheres, I have a very strong impression that if means were taken to ascertain, with more or less approximation, the average annual temperature at the earth's surface, by a combination of the daily averages of a sufficient number of places of observation, there would be found a very considerable difference in the yearly values of the said average annual temperatures. But whether, on inquiry, there should prove to be a decided difference or an absolute agreement between these averages, the fact in either case would surely be worth ascertaining, and could not fail to be instructive. It might be objected that it would be impossible to obtain the observations of the daily average temperature from such a number of observatories as would render the desired annual average for the whole earth of any value, but I think this objection overstates the difficulty. Suppose that the subject were taken up by some one of the meteorological authorities in Great Britain, it would not be difficult to obtain from existing daily records, a good average annual value for the temperature of the British Islands. Similarly, an average annual value could be obtained for the temperature, from the daily averages in the various colonies and dependencies of the British empire; and I take it to be certain, that the conductors of the various meteorological observatories all over the empire would cheerfully respond to an invitation to co-operate in such a work. In a similar scientific spirit it is to be hoped that the observatories of all civilised countries would be willing to exchange their observations, and an approximate result could thus be arrived at, possibly in two or three years. Certainly, it might be at first a rough approximation only, but it would be yearly becoming better with the rapid increase of meteorological observatories all over the world. And as it is not too much to hope that, sooner or later, the whole habitable earth will be civilised and covered with observatories, it is certain that the figures ultimately obtained to represent the average annual temperatures at the earth's surface would have the value of scientific approximations of considerable accuracy. If this be so, it cannot be too early to begin these statistics now.

Supposing that these annual averages should exhibit a difference in their yearly values, it is probable that these differences would vary in sympathy with the total sun-spot areas of the years to which they belonged. What could be done for temperature, could be done at the same time for other subjects of meteorological investigation, and it is impossible to anticipate at present what light these tabulated annual averages might be able to throw upon various problems of solar and terrestrial physics.

Balham, December 4 D. TRAILL

[1] Paris, Auguste Desrez, 1837, tome 2me, p 643.

[2] Le Vieux-Neuf, Histoire Ancienne des Inventions et Découvertes Modernes Paris, Dentu, 1859.

[3] Giphantie, à Babylone, MDCCLX., 12°.

ON A MEANS FOR CONVERTING THE HEAT-MOTION POSSESSED BY MATTER AT NORMAL TEMPERATURE INTO WORK

IN a previous article [1] we considered how, by a simple mechanical means, diffusion renders it possible to derive work from matter at normal temperature. As the subject is an important one we propose to develop it somewhat further here.

2. The normal temperature of objects on the earth's surface represents a vast store of energy in the form of molecular motion. The sea (for example) at normal temperature possesses an amount of molecular energy which (by computation), if it were entirely utilised, would be competent to lift it to a height of upwards of seventy miles. The air and the crust of the earth itself possess comparable amounts of energy. It might therefore well be asked beforehand whether it is not possible to transfer some of this intense molecular motion to masses and utilise it. It may be observed that this intense store of energy is being continually dissipated in space in the form of waves (by radiation). The energy possessed by the molecules of matter, however, maintains (as is known) a constant normal value on account of the waves of heat received from the sun, whose mechanical value at the earth's surface (as represented by the results of Herschel and Pouillet) is normally equal to about one-horse power per square yard of surface. Here, therefore, we have a continual store of motion kept up in the molecules of matter on the earth's surface to be wasted in great part in imparting motion to the ether of space. It would certainly look, *à priori*, as if there ought to be some means of utilising this store of motion.

3. The second law of thermodynamics would (as is known) assume that this would not be practicable. This law was propounded simply as what was considered a legitimate inference from the observed behaviour of heat. But a great advance in the knowledge of the nature of heat has been made since that time, and it should be noticed that the law is (admittedly) by no means *theoretically* necessary or requisite to satisfy the principle of the conservation of energy. Indeed a conceivable case opposed to it has been pointed out by Prof. Maxwell, though one not capable of being practically carried out. It was my purpose in the last article to direct attention to a physical process opposed to the law and admitting of practical realisation, in the effects attendant on the diffusion of matter. At the time when this law was enunciated the character of the motion termed "heat" (as illustrated in the now accepted kinetic theory of gases) was unrecognised, and therefore the *mechanism* of the diffusion of gases was not understood. Under these conditions, therefore, it would not be much a point for surprise if increase of knowledge should show the law not to be generally applicable (or not to admit of that general application which is implied by the use of the term "*law*").

4. It may serve greatly to facilitate the following of this subject if we visualise the relations of heat and work more closely. Since "*heat*" is simply a *motion* of small portions of matter (termed "molecules"), and since the

[1] "On the Diffusion of Matter in Relation to the Second Law of Thermodynamics," see NATURE, vol. xvii. p. 31.

transference of this motion to visible masses is called "*work*," so therefore the conversion of heat into work is no more than the transference of motion from small to large portions of matter, *i.e.*, the transference of motion between portions of matter of *different* dimensions. *The mechanical equivalent of heat* therefore simply represents *the equivalence in energy between the motions of portions of matter of different dimensions* (molecules and visible masses). To deny, therefore, that the heat possessed by matter at normal temperature could be converted into work would be to assume that by a certain difference in dimensions the conditions are such that motion can no longer be transferred from the smaller portions of matter to the larger. This would evidently, *à priori*, be by no means a necessary assumption; indeed it would appear, perhaps, rather strange that by no device could such a thing be done.

5. At the first sight one difficulty in the way of utilising this motion that surrounds us on all sides is that the larger scale portions of matter (visible masses) are immersed among the smaller scale portions of matter (molecules) which surround the visible masses on all sides (as the molecules of the surrounding air, &c.), so that a perfect equilibrium of motion exists on all sides; so that it becomes impossible to transfer the motion to the larger scale mass in the one direction or in the other, and we cannot lay hold of each moving molecule individually, on account of its minute size.

6. It is an observed fact (and demonstrated theoretically) that portions of matter in motion among themselves tend to acquire the *same* energy of motion (called "temperature" in the case of molecules). In accordance with well-known facts, whenever the energy of this system of small moving portions of matter is greater in one part than in another, *i.e.*, whenever the equilibrium of energy is upset, then we can transfer some of the energy to larger scale masses (convert heat into work). Is there, however, no other means of converting heat into work but through *inequality of energy?* It was pointed out in the last article that *inequality of velocity* (by the mechanism of diffusion) will serve the same purpose. The portions of matter (molecules) which by equal temperature possess equal energy, possess, when their *masses* are unequal, unequal velocities. This inequality of *velocity* can be utilised for work as well as inequality of *energy*.

7. Since size is only *relative*, or there is nothing absolute in size, it will be quite legitimate to suppose molecules magnified up to a larger scale so as to be visible, and we do this as in dealing with the *mechanism* of a process, it is almost impossible to visualise or conceive clearly the results without this condition, and it is our object, on account of its practical bearing, to exhibit the process involved in a clear light. Suppose, therefore, the molecules of two diverse gases (oxygen and hydrogen) to be magnified up to visible dimensions, and as we are not concerned with the shape or form of the molecules, we may simply represent the molecules of the two gases by a number of spheres, those representing hydrogen possessing each one-sixteenth of the mass of those representing oxygen, and also possessing a normal velocity four times as great. This is known to be the fact in the case of the two gases when at the same temperature. We will further suppose the spheres inclosed in the two separate halves of a cylinder with a piston between them. The spheres may either be supposed perfectly elastic or their motion kept up artificially in some way; just as in the case of a gas the motion of its molecules is kept up by the molecular vibrations of the sides of the cylinder.

8. The spheres of the two sets possess *equal* energies of motion, the one set making up in mass for what they want in velocity. The colliding spheres in each compartment will arrange themselves (according to a known principle) so that the number of spheres in unit of volume of each set is the same, and therefore the pressure exerted by their impacts on opposite sides of the piston will produce perfect equilibrium, so that the piston remains immovable. Now the question is, supposing that (as in the case of molecules) we cannot lay hold of each of these spheres separately, is there any means of utilising the *inequality* of velocity for the performance of work? [This is the question we have to make in the case of two gases at the same temperature, whose molecules we cannot grasp, and which possess *unequal* velocities.] If we could by any device get a number of the spheres from one compartment into the other without changing their velocities in the act, then the pressure would evidently rise in one compartment, and we should thus obtain a capacity for work without the performance of work. It is evident that this could be done by making several perforations in the piston, about the size of the spheres themselves, so that the spheres, in impinging against the piston would sometimes happen to encounter the void space of a hole, and thus pass through with unchanged velocity into the opposite compartment. If the spheres of the two sets were moving with *equal* velocities, it is evident that as many on an average would pass through one way as the other, and there would therefore be no disturbance of the equilibrium of pressure, and consequently no work to be derived. But from the fact that the spheres are moving with *unequal* velocities, a different result will ensue. It will be evident that the number of spheres passing through the hole will be proportional to the number of times they strike against the piston, for the chances that a sphere will encounter a hole will be proportional to the number of its impacts against the piston, *i.e.* to the *velocity* of the sphere. So the velocity of the spheres in one compartment being four times that in the other, four times as many lighter spheres pass through one way, as heavier spheres pass through the other. The number of spheres in one compartment will therefore rapidly augment, and thus the *pressure* against the piston will rise, and the piston will be finally driven towards the opposite end of the cylinder, and in this act *energy* will be transferred from the spheres in the one compartment to those in the other; or part of the energy could be transferred to an outside mechanism in a self-acting manner if desired, by simply connecting the piston to the mechanism.

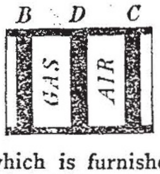
9. Now if precisely the same thing can be done in the case of two gases, it is evident that here the energy being *heat*, we have in the result attendant on the motion of the piston, the transference of heat from one portion of gas to another at normal temperature, *i.e.* the transference of heat in a self-acting manner from a *colder to a hotter portion of matter;* and if desired, a conversion of a part of the heat of the gas (at normal temperature) into *work* by cooling it down below the temperature of the coldest of surrounding objects.

10. In the case of a gas it is clear that we cannot make perforations analogous to the above sufficiently small to suit molecules, but to attack molecules we must have recourse to *molecular* mechanism, or to attempt to handle them like the spheres we must have recourse to mechanism on a suitable scale. We have such a mechanism in a porous diaphragm (such as of pipeclay or plumbago) which represents a piston with *molecular* perforations. Such a diaphragm, if fitted as a piston into a cylinder will exhibit, with the molecules of two separate gases possessing different molecular velocities (such as molecules of oxygen and hydrogen), precisely the same phenomena as those exhibited, simply on a magnified scale in the case of the spheres; or the above description applies word for word. We have by the motion of the porous piston *the conversion of the heat-motion of the gas at normal temperature into work*, the transference of heat automatically from the *colder* portion of gas to the warmer. The second law of thermodynamics only holds when the molecules brought into contact happen to be of the *same*

kind, or, more accurately speaking, of the *same mass*. This latter case is evidently exceptional, and if a case be exceptional the term "*law*" becomes no longer applicable to it.

11. The rates of diffusion of hydrogen and oxygen across the porous diaphragm are known to be as four to one, *i.e.* as the *molecular velocities*. The above illustration of the spheres may serve to exhibit the *physical basis* or cause of this fact in a clear light. The mere statement that the rates of diffusion are inversely as the square roots of the molecular weights of the gases, evidently throws no light on the cause or *physical basis* of the action, which is always the main thing to realise in physical science. The fact that diffusion is in the above ratio to the molecular weight, evidently only happens to be true because the molecular *velocity* is in that same ratio to the molecular weight, otherwise molecular weight has nothing whatever to do with the rate of diffusion. So it will be equally apparent, from the above illustration, that the rate of diffusion of a gas through a porous diaphragm has nothing whatever to do with the *pressure* of the gas, but depends, *cæteris paribus*, on the number of molecules of the gas in unit volume. An increase of the number of molecules in unit volume (by adding to the number of impacts of the molecules against the vessel) increases the pressure, and this is why diffusion *appears* to be dependent on pressure, though evidently *physically* it has nothing to do with it. This serves to explain how, provided the molecular velocities of the gases are considerably diverse, such enormous differences of pressure can take place by diffusion through a porous diaphragm, the pressure having no power whatever to adjust itself through the diaphragm; for the passage of a molecule through the diaphragm simply depends whether, in its normal motion, it happens to encounter a pore or not. The above illustration may also serve to show that the velocity of propagation of any impulse ("wave") by a system of bodies in free collision can only be dependent on the *normal velocity of the bodies*, just as a system of couriers interchanging motion among themselves convey a message at their own rate. So the molecules of a gas interchanging motion among themselves convey an impulse at their own rate, and thus the velocity of sound in a gas can be solely dependent on, and proportional to, *the velocity of the molecules of the gas*, and on nothing else. This must evidently be true on the basis of the kinetic theory, and this theory being now accepted, it would be not unreasonable to expect that in so fundamental a matter as the propagation of sound, an explanation of it on the basis of this theory would be looked to, for a *statical* theory of the propagation of sound appears scarcely to harmonise with the *dynamical* theory of gases. We have alluded to this fact as briefly as possible, having the illustration of the spheres at hand. There may be a liability to lose sight of facts like the above unless due care be taken to realise molecular phenomena by picturing them on a larger scale. The velocity of sound in hydrogen is four times greater than in oxygen, solely because the velocity of the molecules of hydrogen is four times greater than the velocity of the molecules of oxygen—nothing conceivably to do with the molecular weight of the gas, excepting in so far as a less molecular weight determines a higher molecular *velocity*.[1] The rate of propagation of the wave is affected by temperature in so far as the *velocity* of the molecules of the gas (in whose motion the *heat* of the gas consists) is affected by temperature.

12. As an illustration of a simple form of apparatus adapted for converting normal temperature heat into work, and admitting of continuous actuation, the following rough sketch may serve:—Let the annexed diagram represent a cylinder containing three pistons, B, D, C, the



central one, D, of which is furnished with any porous diaphragm (such as of plumbago, or porous earthenware). Let any light gas (hydrogen being the most effective) be supposed introduced into one-half of the cylinder, some heavier gas (or air) filling the other half. All three pistons are supposed (first) fixed. Then, as is known, diffusion commences through the porous diaphragm, everything remaining necessarily at normal temperature so long as the pistons are fixed and no work is done. The rapidly moving molecules of the light gas pass in greater numbers through the pores of the diaphragm than those of the heavy gas (or air), so that the pressure rises in the compartment originally filled with air. As soon as the pressure has attained a maximum, the central piston is automatically released, and is thus driven by the excess of pressure towards the opposite end of the cylinder, the portion of gas which does the work being chilled and the heat transferred in the form of work to the outside machinery with which the central piston is connected. A certain part of the heat goes to the portion of gas *towards* which the piston is driven, heat thus passing from a colder to a hotter body (for as soon as the portion of gas commences to be chilled, it is already the colder). Simultaneously with the stroke of the central piston, a convenient automatic arrangement connected with the machinery oscillates the two end pistons inwards and outwards, expelling in the inward stroke (through convenient openings) the diffused mixture of gas and air, and by the outward stroke drawing in a fresh supply. Of course the valves suitable for this are not given, as it is only our purpose to sketch the *principle* of such an apparatus as a scientific point, and having no regard to any question of commercial value or not. Clearly the power derived would depend on the specific gravity of the gas used, and would be proportional (*cæteris paribus*) to the area of the piston. Coal gas would give a less power than hydrogen. A diffused mixture of gas and air is necessary for gas engines, the mixture being exploded in them. It is clear that it would be possible, by means of an apparatus of the above character, to derive power in the act of mixing the gas and air previous to exploding the mixture. The gaseous mixture, after passing through the apparatus, could be stored in some reservoir or receptacle, so as to recover (before combustion in the gas engine) from surrounding objects the heat which it lost by conversion into work in the diffusion engine. By this procedure it may be observed that the heat converted into work is derived from *the normal store of heat possessed by surrounding objects*, and their store is finally made good by the sun, which latter may therefore be regarded as the ultimate source of the energy derived.

13. In view of the numerous porous structures existing in the animal and vegetable world (*porosity* being a distinguishing characteristic of animal and vegetable organisms), also taking into consideration the prevalence of gases of different molecular weights, notably oxygen and carbonic acid (which are known to be intimately connected with animal and vegetable processes); the conclusion would seem warranted, and even necessary, that work on the above principle must take place widely in nature, and thus part of the store of energy accumulated in materials on the earth's surface by the sun, is made to fulfil a useful end, instead of being dissipated uselessly in space.

S. TOLVER PRESTON

[1] It is evident that though the velocity of the wave is proportional to the velocity of the molecules, the *absolute* velocity of the wave must be to a certain fixed degree less than that of the molecules; for the molecules in their normal motions are moving more or less *obliquely* to the path of the wave. This I have pointed out in a paper, published in the *Philosophical Magazine* for June, 1877, where the true mathematical relation for the velocity has been determined by Prof. Maxwell, and is there given.