called a gulf. But in Australasia the breadth and depth of this gulf is rendered more conspicuous by the association of Man with a series of animals absolutely wanting in those higher members of the Mammalian Class which elsewhere minister to his wants, and the use of which is among the first elements of a civilized condition. Alone everywhere, and separate from other beings, Man is most conspicuously alone in those strange and distant lands where his high organization is in contact with nothing nearer to itself than the low marsupial brain.

To those who connect the origin of Man with the theory of Development or Evolution, in any shape or in any form, these peculiar circumstances respecting the fauna of Australasia indicate beyond all doubt that Man is not there indigenous. They stamp him as an immigrant in those regions—a wanderer from other lands. Nor will this conclusion be less assuredly held by those who believe that in some special sense Man has been created. There is something more than an incongruity in supposing that there was a separate Tasmanian Adam. The belief that the creation of Man has been a special work is not inconsistent with the belief that in the time, and in the circumstances, and in the method of this work, it had a definite relation to the previous course and history of Creation—so that Man did not appear until all these lower animals had been born, which were destined to minister to his necessities, and to afford him the means and opportunities for that kind of development which is peculiarly his own. On the contrary, this doctrine of the previous creation of the lower animals, which is, perhaps, more firmly established on the facts of science than any other respecting the origin of Man, is a doctrine fitting closely into the fundamental conceptions which inspire the belief that Man has been produced by operations as exceptional as their result. And so it is, that when we see men inhabiting lands destitute of all the higher Mammalia, which are elsewhere his servants or companions—destitute even of those productions of the vegetable kingdom, which alone repay the cultivation of the soil, we conclude with certainty that he is there a wanderer from some distant lands, where the work of creation had been carried farther, and where the conditions of surrounding Nature were such as to afford him the conditions of a home.

We see, then, that the question asked by Mr. Darwin, in respect to the Fuegians, is a question arising equally in respect to all the races who inhabit regions of the globe, which from any cause present conditions highly unfavorable to Man. Just as Mr. Darwin asked, what could have induced tribes to travel down the American continent to a climate so rigorous as Cape Horn?—just as we have asked, on the same principle, what could have induced men to travel along the same continent in an opposite direction till they reached and settled within the Arctic Circle?—so now we have to ask, what could have induced men to travel from Asia, or from the rich and splendid islands of the Eastern Archipelago, and to take up their abode in Australasia?

In every one of these cases the change has been greatly for the worse. It has been a change not only involving comparative disadvantages, but positive disabilities—affecting the fundamental elements of civilization, and subjecting those who underwent that change to deteriorating influences of the most powerful kind.

It follows from these considerations as a necessary consequence that the present condition of the Australian, or the recent condition of the Tasmanian, cannot possibly be any trustworthy indication of the condition of their ancestors, when they lived in more favored regions. The same argument applies to them which, as we have seen, applies to the Fuegians and the Eskimo. If all these families of Mankind are the descendants of men, who at some former time inhabited countries wholly different in climate, and in productions, and in all the facilities which these afford for the development of the special faculties of the race, it is in the highest degree improbable that a change of habitat so great should have been without a corresponding effect upon those over whom it passed. Nor is it a matter of doubt or mere speculation that this effect must have been in the highest degree unfavorable. The conclusion, therefore, to which we are led is, that such races as those which inhabit Australasia, are indeed the results of development, or of evolution—but of the development of unfavorable conditions, and of the evolution of the natural effects of these. Instead of assuming them to be the nearest living representative of primeval Man we should be more safe in assuming them to represent the widest departure from that earliest condition of our race which, on the theory of Development, must of necessity have been associated at first with the most highly favorable conditions or external Nature.

DOLBEAR ON THE NATURE AND CONSTITUTION OF MATTER.

A Critique.

There appeared in "SCIENCE" a series of three papers[1] by Professor A. E. Dolbear which contain such new and somewhat startling ideas on the nature and constitution of matter that an interesting controversy was to be expected. Nearly six months have, however, passed without any objections having been raised to any of the Professor's statements, some of which seem to me quite strange and of rather peculiar mathematics withal. I now, with no little hesitation enter a protest against some of these statements. The subject of the constitution of matter is so intricate, so complicated, beset with so many difficulties on the one hand, while on the other our means of dealing with it are so inadequate, our methods of investigation so imperfect that, as Maxwell says, all we can do is to make hypotheses and see how far our facts and phenomena bear them out. This being so, I believe that whenever a particularly bold hypothesis is made and conclusions are drawn therefrom by anyone without having made a most careful comparison with all the principal phenomena of matter, the humblest student of this fascinating department of physical science has a right to command a most vigorous halt, and to examine whether he who assumes to guide is himself sufficiently acquainted with the intricacies and windings of the road not to lead his followers into the dismal swamps of metaphysical vagaries. I therefore claim for myself that right, lest what I have to say might be construed as too presumptuous.

In my review I shall, in the main, touch upon and discuss the points I desire to examine, in the order in which they occur in the Professor's papers. To begin, then, with the first paper, Section II, I shall devote a little attention to the equation $E' = \varepsilon \frac{mv^2}{2}$ which the Professor says expresses the total energy of an atom. It seems an altogether gratuitous assumption to give to the expression for the total energy of an atom the same form that Clausius gives for the total energy of a molecule. In the molecule we have the motion of translation and also the motion or motions of its parts relative to its centre of mass; but of the atom we cannot make the same assertion. Clausius was justified, by mathematical deductions from experimental data, to assume that the total energy of the molecule is proportional to the energy of agitation; but that does by no means justify the assumption that the same form of function also expresses the total energy of the atom, for here all experimental data are wanting. We may, however, reasonably conclude that the form of this function for the atom must differ somewhat from that for the molecule, as the motions of the atom must, of necessity, be much more intricate and complex

[1] "On Some Needed Changes and Additions to Physical Nomenclature," Vol. I., p. 238; "On Matter as a Form of Energy," Vol. II., p. 49, and "On the Amplitude of Vibration of Atoms," Vol. II., p. 146.

than those of the molecule. Granting the correctness of the expression for argument's, sake I must confess that I do not understand how the Professor gets the expression $E' - E = \varepsilon$ given under 3, in his "Table of Forms of Energy." If ε in the expression $E' = \varepsilon \frac{m v^2}{2}$ is anything it certainly must be the ratio $\frac{E'}{E}$ where $E = \frac{m v^2}{2}$ is the energy of agitation of an atom. By subtraction we obtain $E' - E = \varepsilon \frac{m v^2}{2} - \frac{m v^2}{2} = \frac{m v^2}{2} (\varepsilon - 1)$ and not ε as the Professor would lead us to believe. While I regard it simply a gratuitous assumption to give the expression for the total energy of an atom, and that for the total energy of a molecule the same form—because we have no experimental evidence whatever to justify us to believe that the conditions of the atom resemble those of the molecule—I believe that the equation $E' = \varepsilon \frac{m v^2}{2}$ in which ε is *internal* energy is utterly incorrect. ε in this expression is not at all analogous to β in $\frac{1}{2} \beta m v^2$ the expression for the total energy of a molecule as given by Maxwell. Here β is the numerical ratio of the total energy to the energy of agitation, an abstract, while ε is internal energy, a concrete. Here let me ask what is energy times energy. The form $E' - E = \varepsilon$ is undoubtedly correct. From this by substitution we get $E' = \frac{m v^2}{2} + \varepsilon$ and not $\varepsilon \frac{m v^2}{2}$.

The statement "Latent heat, specific heat, and specific inductive capacity, are all involved in (that factor?) ε," is certainly not correct. Latent heat is work performed upon some body, and is, according to Clausius, partly internal and partly external. The external work is performed upon surrounding material systems. The internal work is, in general, composed of two parts—one expended upon the molecules in expanding the body from one state of aggregation to another, the other part is expended upon the parts of the molecule. It is only this last portion which can affect the atom as such, and which can in any way be involved in ε. Similarly we find that specific heat is also work performed, and that, too, of a complex nature. Specific inductive capacity seems to me to belong to an altogether different class of phenomena.

In regard to the ether the Professor makes some very curious statements. He says that he knows nothing of the specific properties of the ether, yet in the same sentence is the statement "ether is not matter," as if this were a generally accepted view. If the ether is not matter, what is it? There are two ways of looking at matter—the subjective or metaphysical, and the objective or physical. Metaphysically defined matter is anything which has extension or occupies space. For the physical definition I quote Maxwell[2]: "Hence, as we have said, we are acquainted with matter only as that which may have energy communicated to it from other matter, and which may, in its turn, communicate energy to other matter." Again, he says: "Energy cannot exist except in connection with matter." Whether, then, we accept the metaphysician's definition or the physicist's, we must regard ether as matter; for it certainly has extension and occupies space, and it certainly receives from other matter, transmits and imparts to other matter energy. That Maxwell regarded ether as matter, appears from the following quotation, taken from the same work and page as the preceding: "Hence, . . . we conclude that the *matter* which transmits light is disseminated through the whole of the visible universe." The italics are mine. Professor Dolbear, furthermore, tacitly assumes ether to have mass, as will appear hereafter.

Again, the Professor says: "Furthermore, as atoms differ in mass so will their rates of vibration differ when they possess the same absolute amount of energy. Velocity, in this case, will be equal to amplitude *a b*, the space point *c* passes over during one vibration. If m and m' be two atoms of different masses having equal energy of vibration, then $E = \frac{m v^2}{2} = \frac{m' v'^2}{2}$ and $\frac{m}{m'} = \frac{v'^2}{v^2}$ that is the square of their velocities is inversely as their masses, so that wave-length in the ether will vary as the mass of the atom." This is certainly very curious logic and mathematics. The statement may be true, and the investigations of Lecoq de Boisbaudran even furnish some evidence in its favor, but the mathematical proof offered by the Professor does not justify any such conclusion. v and v' are, according to his own statement, amplitudes of vibration; when, then, the atoms of different masses have equal energy, the proportion $\frac{m}{m'} = \frac{v'^2}{v^2}$ simply proves that the squares of the amplitudes of vibration are inversely as the masses. In what manner the rate of vibration and wave-length in ether follows from this relation of mass to amplitude the Professor does not make clear. In order to make the above conclusion of Professor Dolbear correct, we must have the further condition, $\frac{v'^2}{v^2} = \frac{n'}{n}$ where n and n' are the relative number of vibrations of m and m' in equal times. One of the most fundamental equations of motion is unquestionably $v = \frac{s}{t}$. Hence, as the amplitude $a\, b$ is a space passed over in a given time, we can make it equal to v only by making t unity. Similarly we can make the amplitude of m' equal to v' only by making t unity. If now we wish to compare the velocities and masses of the two atoms we can certainly not use different units of time to determine those velocities; and we get, according to the Professor's statement, the self-contradictory result that two atoms, which make each one vibration in equal times yet have different rates of vibration. To make the problem more general let us take two atoms of masses m and m'. Let them make respectively n and n' vibrations of amplitudes, a and a' in unit of time. The time of one vibration of m will be $\frac{1}{n}$ and of m', $\frac{1}{n'}$. Substituting these values successively for t, and a and a' successively for s in the equation of motion, we have

$$v = \frac{a}{\frac{1}{n}} = a n \text{ and } v' = \frac{a'}{\frac{1}{n'}} = a' n' \text{ combining } \frac{v}{v'} = \frac{a n}{a' n'},$$

or the velocities are proportional to the products of the amplitudes by the number of vibrations in unit time. Combining this with the Professor's proportion we have $\frac{m}{m'} = \frac{a'^2 n'^2}{a^2 n^2}$.

To obtain from this the relation $\frac{m}{m'} = \frac{\lambda}{\lambda'}$, λ and λ' being wave-lengths, we must fulfil the condition $\frac{a'^2 n'^2}{a^2 n^2} = \frac{n'}{n}$ or $\frac{n}{n'} = \frac{a'^2}{a^2}$. If, then, two atoms of the masses m and m' have equal energy, and the relation $\frac{n}{n'} = \frac{a'^2}{a^2}$ holds n and n', being the respective number of vibrations in unit time, and a and a' corresponding amplitudes, the relation $\frac{\lambda}{\lambda'} = \frac{m}{m'}$ in which λ and λ' are wave-lengths will follow. For we will then have, as above shown, $\frac{m}{m'} = \frac{n'}{n}$. We also have $\lambda = \frac{v}{n}$ and $\lambda' = \frac{v}{n'}$. From these we obtain $\frac{\lambda}{\lambda'} = \frac{n'}{n}$ and, hence, $\frac{m}{m} = \frac{\lambda}{\lambda'}$.

[2] "Matter and Motion," p. 93.

Whether or not the relation $\frac{n}{n'} = \frac{a'^2}{a^2}$ holds in any particular case can, it would seem, be determined only by experiment. So, too, the fact of the equal absolute energy of vibration of two atoms. Our experimental methods are, however, as yet far from competent to deal with either question, and until they are it is certainly premature to build up speculative hypotheses.

Every student of molecular science knows how great is the temptation to build hypotheses which are to account for all the physical and chemical relations of matter. We can read between the lines of nearly all our recent writers in this department of science their secret belief that chemical phenomena are probably but a complex phase of mechanical phenomena, and that all matter is probably one. Nor are facts justifying such views altogether wanting. Probably no chemist would be bold enough to say in how far such phenomena as, for instance, the solution of ammonia, carbon dioxide, and many other gases in water are of purely chemical and how far of purely physical nature. There are many other phenomena in which similar difficulty would be felt. The phenomena of adhesion and cohesion are such that it does not require a very great stretch of the imagination to suppose that they may be but different phases of what we call chemical union. But to pass from such general and indefinite speculations to suppositions in regard to the mechanical conditions which will account for all these phenomena and all the properties of matter upon purely mechanical principles is a long and, indeed, a bold stride. As the temptation to make this attempt is great, so ought our caution to be great in making the attempt. Professor Dolbear's immediate predecessor in this attempt is Professor Norton. His hypothesis of two atmospheres, one attractive, the other repellant, surrounding each atom, is too artificial, and in being in opposition to the "Kinetic Theory of Gases," is probably too much out of sympathy with the tendency of modern thought to make many converts. Not so, however, with Professor Dolbear's speculations. Their great fundamental simplicity, as well as their thoroughly Kinetic nature, make them dangerous to healthy progress in molecular science unless they can maintain their right of being by accounting for at least the chief and fundamental phenomena of matter. I shall now attempt to apply the touch-stone to them. In Section IV. of his first paper Professor Dolbear advances an hypothesis of chemical union founded on the analogy to a vibrating body which, as is well known by reducing the average density of the atmosphere, causes light bodies to cling to it by atmospheric pressure. We are told that precisely the same conditions exist in the ether near a vibrating atom; that the average density of the surrounding ether is lessened, and that by extraneous pressure another atom vibrating synchronously with the first would attach itself thereto, and the molecule would be formed, etc., etc. I would like to ask how Prof. Dolbear can consistently speak of the density of ether, which, he says, is not matter. Now, in this idea of density there is implicitly the idea of mass, for density, as every one knows, is the mass or amount of matter in unit volume. But, disregarding this inconsistency, it is certainly very bold induction, if induction it can be called, to attribute chemical union to a lessening of density of ether due to atomic vibrations because a vibrating tuning-fork attracts light bodies when brought sufficiently near. In the professor's hypothesis the atoms (vortex-rings) vibrate about a circle as figure of equilibrium, and consequently have four points of maximum displacement or minimum density of the ether. As a consequence of this, each atom must attract other atoms capable of attaching themselves to it at four points. To judge from his diagrams, the Professor believes that atoms unite only in two-dimensional space, *i.e.*, that the centres of all the atoms lie in the same plane. Such a distribution of the atoms would render any closed structure such as a saturated molecule an impossibility, for the peripheral atoms would constantly attract further atoms as long as they vibrate, and other atoms vibrating synchronically with them are present. If, on the other hand, the atoms are arranged in tri-dimensional space, having their centres in planes, say, at right angles to one another, the simplest molecule and the only really stable one would have to contain six atoms whose planes of rotation form the faces of a cube. A further possible supposition is that the atoms would arrange themselves in parallel planes with their centres in a line at right angles to these planes. The first of these suppositions, as already indicated, would not allow the formation of saturated molecules, and it would seem that all chemical union, as we know it, could not exist, for it would evidently be altogether a matter of chance how atoms grouped themselves in regard to numbers, so that we could not always obtain like results of union under precisely like conditions. The second supposition is also inconsistent with chemical facts, for we have molecules of two, three, four and five atoms, as well as others containing hundreds. The third supposition is also untenable, for from Helmholtz's mathematical investigations and Tait's experiments we know that two vortex-rings, when they move axially in the same direction alternately, pass through each other one expanding, the other contracting, while when moving axially in opposite directions they both expand moving slower and slower, but never meet. This is, according to Tait, about all we know experimentally or mathematically in regard to the action of one vortex ring upon another. It is certainly a little strange that Prof. Dolbear, in framing his hypothesis, completely ignores these known facts, and relies on a far-fetched analogy. Serious as are these difficulties, they are by no means the most serious. If experimental evidence is worth anything, we must believe that elementary molecules, with a few exceptions, consist of two atoms, which are, as far as we can judge, exactly alike. Furthermore, we find that in all chemical reactions we can deal with nothing less than the molecule; we know and can deal with the atom only as a part of a molecule, and not as an independent existence. When chemical union takes place between two elements, there is simply an interchange of atoms between the molecules. The difference between the molecules of an element, and those of a compound, is simply this, that the atoms of elementary molecules are all alike, while those of a compound molecule are unlike. I repeat all these fundamental and well-known chemical facts and deductions, to show how singularly inadequate Prof. Dolbear's hypothesis is to account for even the most simple chemical facts. According to his hypothesis, the atoms whose rates of vibration are most exactly alike, must form the most stable molecules. Consequently, the atoms of an element must cling more firmly together than can those of two different elements, and chemical union between the elements becomes impossible. Did the atoms of elements exist as individuals, and not as parts of molecules simply, synchronism of vibrations might be a possible supposition to account for chemical union; but as the case stands, we must reject any such hypothesis as precluding all combination between atoms of different elements. Setting aside even this difficulty, how are we to account by synchronous vibrations for the liberation of energy in the form of heat and light, which accompanies most chemical unions. These forms of energy are, according to the Professor himself, altogether due to vibrations of the atoms and these same vibrations cause the union. Now, how can they both cause the union and be produced by it? Does this not look a little like *perpetuum mobile?* Had the Professor tried to explain adhesion and cohesion by molecular vibration his position would undoubtedly be much stronger. We know that molecules are complex and that there must be motion of their parts relative to the centre of mass of the molecule. As there is no good reason for supposing the motions of these parts or atoms to be rather in one plane than another, we must admit the possibility of motion in all planes. The vibrations would, however, probably be in three planes at right angles to one another in all molecules of more than three atoms; and would, consequently, have six points of maximum displacement and minimum density of the surrounding ether. Molecules of two and three atoms might possibly vibrate in two or only one plane. As molecules are not vortex-rings, though possibly groups of vortex-rings, the analogy to a vibrating tuning fork becomes much closer than in the case of a vibrating vortex-ring, and we are much more justified in trying to make application of the hypothesis. Prof. Dolbear's analogy thus modified can, I think, be made a very fair working hypothesis to explain adhesion, cohesion and even crystallization. The phenomena of surface tension of liquids and capillary action find a reasonably fair explanation upon this hypothesis, and possibly also those of osmosis, dialysis and occlusion. But even here such an hypothesis meets with many difficulties and we must exercise extreme caution, and must gather further experimental evidence before committing ourselves to its acceptance.

In his second paper the Professor tells us that the vortex-ring theory assumes that matter is a *form of energy*, etc. Never having been so fortunate as to have had access to Sir William Thomson's original memoir, I know his celebrated hypothesis only through interpretations of others. From these interpretations I have always supposed that this hypothesis assumes that all matter is essentially one; and that the elements, as we know them, are portions of this common matter imbued with vortex-motion, thus forming vortex-rings variously knotted, whose energy is non-interchangeable with other forms of energy provided the vortex-rings are formed and exist in a perfect or frictionless fluid. If the fluid is not quite perfect, not quite frictionless, the vortex-rings must gradually be destroyed and their energy must be transformed. The uniform material substratum, if I understand the hypothesis correctly, consists of smaller and simpler vortex-rings which are also the particles or atoms of the ether. If, then, I comprehend the positions, the non-transformability of the energy of the vortex atoms and also their permanence, *i. e.* the persistence of our elements depend upon the perfect fluidity of the ether. Whether the ether is perfectly frictionless or not science is, I think, hardly ready to answer. To call "*matter a form of energy* not interchangeable with other variable forms" is, under the circumstances and from the meaning of the terms employed, to take extraordinary liberties with language. Physically regarded, energy is, to strip the term of all technicalities, matter in motion. Then Professor Dolbear's statement becomes matter, is a form of matter in motion, which is hardly intelligible. Again we are told "The energy of a mass of matter varies as the square of the velocities, but the *properties* of the mass vary with the form of the energy, that is to say the physical properties of a heated body are not identical with those of the same body when it is cool, but possesses the same amount of energy in free path motion." Exactly what this sentence means is, I must confess, beyond my comprehension. One thing, however, seems certain, that it expresses an idea directly opposed to the "Mechanical Theory of Heat" and the "Kinetic Theory of Gases" in the statement that a cool body "possesses the same amount of energy in free path motion" as the same body when heated. If this be so, what becomes of $\frac{v p}{\tau} = \frac{v' p'}{\tau'}$ for gases, and what of the "Thermo-dynamic Scale of Temperature."

In regard to the assumption $\frac{mv^2}{2}$ = atomic weight and the calculations based thereon, I will merely remark that if the groups having the same m or those having the same v showed any family likeness or any gradual variation of properties as do Mendelejeff's periods and groups, then they would be worthy of consideration. As it is, however, they seem mere jugglery with figures. That the atoms of the elements have a "common form differing arithmetically from each other in size and velocity" is utterly inconsistent with the well-known facts and phenomena of quantivalence or valency of atoms. There would have to be two forms at least one for artiad, and one for perissad atoms. I think for the present, at least, we must reject this idea of simplicity and still follow Sir William Thomson.

In the third paper we read, "There is now sufficient evidence for the belief that the Kinetic energy of atoms and molecules consists of two parts, one of which is the energy of translation or free path, the other of a change of form due to vibrations of the parts of the atom or molecule toward or away from its centre of mass. The pressure of a gas is immediately due to the former while the temperature depends solely upon the latter." To the first sentence of this quotation I object, because atoms and molecules are treated as if similar, for which assumption we have no evidence. The second sentence contains the very strange idea that the temperature of a gas is due only to the internal energy of the molecule. Maxwell in his "Theory of Heat" Chap. XXII, under "Specific Heat at Constant Volume" says: "Since the product pv is proportional to the absolute temperature, the energy is proportional to the temperature." By energy Maxwell here means, as appears from the context, what Prof. Dolbear would call total energy. From this it appears that Prof. Dolbear's statement can hardly be correct. If we remember that Maxwell speaks of molecules and Prof. Dolbear of atoms the latter's statement becomes still more doubtful. The assumption that "these two forms of energy must indeed be equal to each other in a gas under uniform conditions," upon which all the Professor's calculations in his third paper are based, can easily be disproved. The Kinetic energy of agitation of a molecule is $\frac{1}{2} mv^2$ and the (total) energy is "$\frac{1}{2} \beta mv^2$ where β is a factor always greater than unity and probably equal to 1.634 for air and several of the more perfect gases." Hence the internal energy is $\frac{1}{2}$ (.634 mv^2.) This, of course, invalidates all the Professor's calculations.

Having extended my remarks far beyond what I originally intended, I shall touch upon only one more point, though I find various other difficulties in the Professor's speculations. The last paragraph of the third paper begins: "As at absolute zero each atom is quite independent of every other atom, that is, matter has not a molecular structure, etc." Now, I would like to ask the Professor how he knows this. Such a state of affairs would indeed make the absolute zero a more than singular point in the curve of the properties of matter.

BUFFALO, N. Y., *April* 20, 1881. WM. H. DOPP.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

INTRA-MERCURIAL PLANETS.

To the Editor of "SCIENCE:"

I wish to say that in the sketch given to "SCIENCE," No. 35, p. 95, the position of Professor Swift's Vulcans is very nearly as they were put down by Professor Swift himself on a map that now hangs in my room at the Naval Observatory.

As to negative evidence there is something to be said on both sides of the question. When extraordinary discoveries are reported they are to be severely examined and carefully criticised. If the observations on which