THEORY OF HEAT

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PREFACE.

THE following Treatise is an attempt to present in a single view, the most important facts at present known on the subject of heat, and to deduce a theory which, if it does not comprehend all the phenomena, is at variance with none. The French writers on the subject have adopted the title "Theory of Heat" to express the mathematical solution of the problems of Radiation and Conduction, but with the exception of M. Ampère's suggestions, no attempt that I am aware of has been made to combine the varied and apparently opposed characters which present themselves to our view in regarding heat as the agent of expansion, whilst it exhibits all the properties of a state of vibration. The hypothesis which makes heat a *substance* is utterly incapable of accounting for phenomena analogous to those of light, at least in a manner at all consistent with the explanation (now generally deemed correct beyond dispute) of the parallel properties, whilst on the other hand the supposition of a vibratory motion appears to be totally inconsistent with the facts of latent heat and expansion. The former being a Statical hypothesis is

inadequate to take cognizance of the effects of position, otherwise than by reference to the direction in which the equilibrium is disturbed; the latter being a Dynamical hypothesis, fails altogether to represent actions, the operation of which, whilst it varies with a change of circumstances, is totally uninfluenced by time.

In addition to the circumstance that no theory of heat has yet been promulgated, sufficiently comprehensive to include the explanation of all the phenomena, even the simple and well known facts relative to pressure and density are felt to be utterly inexplicable on any of the ordinary suppositions, without the aid of a force, the effect of which is insensible at a finite interval. It is true Mr Dalton appears to have recourse to no such hypothesis, but I do not know that he conceives pressure to be due to the actual contact of particles, and without such a supposition I am at a loss to imagine the manner in which he accounts for the phenomena. Nothing however can be more lucid than his ideas of the constitution of bodies; and I beg to acknowledge that I owe the groundwork of my theoretical explanations of this branch of the subject entirely to him.

It may be expected that I should give a reason for omitting the explanations of the phenomena of expansion, &c., given by Laplace in the Mécanique

Céleste. My principal reasons are that the law of force which Laplace assumes, is one which I cannot *conceive*, having nothing analogous by which to represent it to the mind; and that if I were to allow the possibility of such a law of force, I should be disposed to question many parts of the process by which his conclusions are obtained; particularly the investigations in V. 5. pp. 105, 106, 107, &c. In several memoirs in the Transactions of the Cambridge Philosophical Society have appeared my reasons for supposing that molecular forces are forces varying according to the law of the inverse square of the distance, it is not therefore necessary to enter at all into that subject at present. The hypothesis which I have adopted is that of Dalton, viz. that particles of matter are surrounded by particles of caloric; the magnitude of the latter being very much smaller than that of the former. To this supposition I have attached that of a mutual repulsion between particles of the same medium, and a mutual attraction between those of an opposite one, which furnishes me with a very simple explanation of the facts relative to pressure and expansion, by rendering it necessary that the particles of caloric should be considered as actually in contact with, and pressing against those of matter. By this means the law of the inverse square of the distance is as probable as any other, whilst Dynamical considerations, at the same time that they lead

obviously to the law, afford a complete solution of all the difficulties attending on the phenomena of radiation and transmission.

It has been my endeavour, if possible, to introduce no *new* supposition, but rather to shew that those which I do not find ready to my hands may be arrived at as necessary consequences of the general law: by this means the whole is rendered the result of one grand principle; and if it should be found that some points of the theory are rather indefinite and capable of almost any interpretation which may be required, my defence is that we still labour under a degree of uncertainty as to the experimental facts, and consequently cannot limit our theory to a narrower bound than such as will include all the probable results.

To give an instance. We have no very definite idea of the meaning to be attached to the word temperature: that it is a measure of the state of heat within a body there can be no question, but in what way increments of temperature correspond to increments of heat we know almost nothing at all Whether as Dalton supposes, all bodies expand in geometrical progression, as the quantity of heat they contain is increased in arithmetical progression, or whether bodies of a consistence least likely to be affected with deviations due to changes in the distance of their particles, (such as the gases) expand equally

by equal increments of temperature, is a subject on which we can at present form no correct conclusions. The former hypothesis is an extremely probable one, and it consequently behoves us to leave sufficient latitude in our assumptions by which the latter is explained, to admit of a change to the former, should it be found requisite. But whilst it is necessary not to limit the detail of our hypothesis, it is highly incumbent on us not to admit of assumptions which are in the slightest degree inconsistent with each other. It has consequently been my especial aim to effect unity of hypothesis, preferring a colourable explanation of some difficulties to a disconnected solution of all, and leaving open a considerable number of phenomena, which a very slight addition to the first principles would have reduced to theory, whenever it appeared probable that subsequent consideration would render such addition needless.

It will be seen that I divide the subject into two parts, Statical and Dynamical. The latter is based on calculations suggested by the properties of light, and the two are connected in the following manner.

If heat is to be transmitted from one point to another across any substance, the state of the system is evidently such, that there is a number of particles on the outside of the surface, greater than would be the case were the system in equilibrium; the consequence is, that the repulsion from without communicates a state

of motion to the particles within, analogous to that with which they themselves are affected. But in addition to a motion of translation forwards, the particles may be endued with a transverse vibration, which vibration will neither affect nor be affected by the mean motion of transmission forwards, provided the medium is symmetrical and continuous. Now at the surface this continuity ceases, and also an equilibrium amongst the particles no longer exists; at this point, as it appears from calculation, the particles will receive a new motion of transmission, the magnitude of which varies as the sum of two expressions; the one being the excess of repulsion due to the aggregation of particles; the other, a quantity depending on the *vis viva* of the vibratory motion. The analytical investigation on which this depends is by no means complex, but to those who dislike the trouble of consulting it, I explain it in the following manner. Suppose the particles arranged in a square order, that is, that eight particles of the medium are in the corners of a parallelopiped, then if a set of them, all lying in one plane be simultaneously moved in that plane, the obvious result is, that the repulsions which they experience will be altogether changed; if the medium be symmetrical on both sides of this plane, this change of force will produce no effect, the additional repulsions being equally balanced; if this be not the case, it is obvious that the pressure