ATOMS

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PREFACE

Two kinds of intellectual activity, both equally instinctive, have played a prominent part in the progress of physical science.

One is already developed in a child that, while holding an object, knows what will happen if he relinquishes his grasp. He may possibly never have had hold of the particular object before, but he nevertheless recognises something in common between the muscular sensations it calls forth and those which he has already experienced when grasping other objects that fell to the ground when his grasp was relaxed. Men like Galileo and Carnot, who possessed this *power of perceiving analogies* to an extraordinary degree, have by an analogous process built up the doctrine of energy by successive generalisations, cautious as well as bold, from experimental relationships and objective realities.

In the first place they observed, or it would perhaps be better to say that everyone has observed, that not only does an object fall if it be dropped, but that once it has reached the ground it will not rise *of itself.* We have *to pay* before a lift can be made to ascend, and the more dearly the swifter and higher it rises. Of course, the real price is not a sum of money, but the external compensation given for the work done by the lift (the fall of a mass of water, the combustion of coal, chemical change in a battery). The money is only the symbol of this compensation.

This once recognised, our attention naturally turns to the question of how small the payment can be. We know that by means of a wheel and axle we can raise 1,000 kilogrammes through 1 metre by allowing 100 kilogrammes to fall 10 metres ; is it possible to devise a more economical mechanism that will allow 1,000 kilogrammes to be raised

1 metre for the same price (100 kilogrammes falling through 10 metres)?

Galileo held that it is possible to affirm that, under certain conditions, 200 kilogrammes could be raised 1 metre without external compensation, "for nothing." Seeing that we no longer believe that this is possible, we have to recognise *equivalence between mechanisms* that bring about the elevation of one weight by the lowering of another.

In the same way, if we cool mercury from 100° C. to 0° C. by melting ice, we always find (and the general expression of this fact is the basis of the whole of calorimetry) that 42 grammes of ice are melted for every kilogramme of mercury cooled, whether we work by direct contact, radiation, or any other method (provided always that we end with melted ice and mercury cooled from 100° C. to 0° C.). Even more interesting are those experiments in which, through the intermediary of friction, a heating effect is produced by the falling of weights (Joule). However widely we vary the mechanism through which we connect the two phenomena, we always find one great calory of heat produced for the fall of 428 kilogrammes through 1 metre.

Step by step, in this way the First Principle of Thermodynamics has been established. It may, in my opinion, be enunciated as follows:

If by means of a certain mechanism we are able to connect two phenomena in such a way that each may accurately compensate the other, then it can never happen, however the mechanism employed is varied, that we could obtain, as the external effect of one of the phenomena, first the other and then another phenomenon in addition, which would represent a gain.[1]

Without going so fully into detail, we may notice another similar result, established by Sadi Carnot, who, grasping the essential characteristic common to all heat engines, showed that the production of work is always accompanied "by the passage of caloric from a body at a higher tem-

[1] At least, the other phenomenon could only be one of those which we know can occur without external compensation (such as isothermal change of volume of a gaseous mass, according to a law discovered by Joule). In that case the gain may still be looked upon as non-existent.

perature to another at a lower temperature." As we know, proper analysis of this statement leads to the Second Law of Thermodynamics.

Each of these principles has been reached by noting analogies and generalising the results of experience, and our lines of reasoning and statements of results have related only to objects that can be observed and to experiments that can be performed. Ostwald could therefore justly say that in the doctrine of energy there are no *hypotheses*. Certainly when a new machine is invented we at once assert that it cannot create work; but we can at once verify our statement, and we cannot call an assertion a hypothesis if, as soon as it is made, it can be checked by experiment.

Now, there are cases where hypothesis is, on the contrary, both necessary and fruitful. In studying a machine, we do not confine ourselves only to the consideration of its visible parts, which have objective reality for us only as far as we can dismount the machine. We certainly observe these visible pieces as closely as we can, but at the same time we seek to divine the *hidden* gears and parts that explain its apparent motions.

To divine in this way the existence and properties of objects that still lie outside our ken, *to explain the complications of the visible in terms of invisible simplicity*, is the function of the intuitive intelligence which, thanks to men such as Dalton and Boltzmann, has given us the doctrine of Atoms. This book aims at giving an exposition of that doctrine.

The use of the intuitive method has not, of course, been used only in the study of atoms, any more than the inductive method has found its sole application in energetics. A time may perhaps come when atoms, directly perceptible at last, will be as easy to observe as are microbes to-day. The true spirit of the atomists will then be found in those who have inherited the power to divine another universal structure lying hidden behind a vaster experimental reality than ours.

I shall not attempt, as too many have done, to decide between the merits of the two methods of research. Certainly during recent years intuition has gone ahead of induction in rejuvenating the doctrine of energy by the incorporation of statistical results borrowed from the atomists. But its greater fruitfulness may well be transient, and I can see no reason to doubt the possibility of further discovery that will dispense with the necessity of employing any unverifiable hypothesis.

Although perhaps without any logical necessity for so doing, induction and intuition have both up to the present made use of two ideas that were familiar to the Greek philosophers; these are the conceptions of *fullness* (or continuity) and of *emptiness* (or discontinuity).

Even more for the benefit of the reader who has just read this book than for him who is about to do so, I wish to offer a few remarks designed to give objective justification for certain logical exigencies of the mathematicians.

It is well known that before giving accurate definitions we show beginners that they already possess the idea of continuity. We draw a well-defined curve for them and say to them, holding a ruler against the curve, "You see that there is a tangent at every point." Or again, in order to impart the more abstract notion of the true velocity of a moving object at a point in its trajectory, we say, "You see, of course, that the mean velocity between two neighbouring points on this trajectory does not vary appreciably as these points approach infinitely near to each other." And many minds, perceiving that for certain familiar motions this appears true enough, do not see that there are considerable difficulties in this view.

To mathematicians, however, the lack of rigour in these so-called geometrical considerations is quite apparent, and they are well aware of the childishness of trying to show, by drawing curves, for instance, that every continuous function has a derivative. Though derived functions are the simplest and the easiest to deal with, they are nevertheless exceptional; to use geometrical language, curves that have no tangents are the rule, and regular curves, such as the circle, are interesting though quite special cases.

At first sight the consideration of such cases seems merely an intellectual exercise, certainly ingenious but artificial and sterile in application, the desire for absolute accuracy carried to a ridiculous pitch. And often those who hear of curves without tangents, or underived functions, think at first that Nature presents no such complications, nor even offers any suggestion of them.

The contrary, however, is true, and the logic of the mathematicians has kept them nearer to reality than the practical representations employed by physicists. This may be illustrated by considering, in the absence of any preconceived opinion, certain entirely experimental data.

The study of *colloids* provides an abundance of such data. Consider, for instance, one of the white flakes that are obtained by salting a soap solution. At a distance its contour may appear sharply defined, but as soon as we draw nearer its sharpness disappears. The eye no longer succeeds in drawing a tangent at any point on it; a line that at first sight would seem to be satisfactory, appears on closer scrutiny to be perpendicular or oblique to the contour. The use of magnifying glass or microscope leaves us just as uncertain, for every time we increase the magnification we find fresh irregularities appearing, and we never succeed in getting a sharp, smooth impression, such as that given, for example, by a steel ball. So that if we were to take a steel ball as giving a useful illustration of classical continuity, our flake could just as logically be used to suggest the more general notion of a continuous underived function.

We must bear in mind that the uncertainty as to the position of the tangent plane at a point on the contour is by no means of the same order as the uncertainty involved, according to the scale of the map used, in fixing a tangent at a point on the coast line of Brittany. The tangent would be different according to the scale, but a tangent could always be found, for a map is a conventional diagram in which, by construction, every line has a tangent. An essential characteristic of our flake (and, indeed, of the coast