The Definition of Physical Quantities.

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IN the July issue of *Current Science* appears a review of a recent text-book of mine. On the whole it is a careful and generous review, and I have no wish to controvert it in detail, although with regard to some of the criticisms there may well be a legitimate difference of opinion. The reviewer, however, raises points of a fundamental character, intrinsically more important than the merits or demerits of my book, deserving a full and critical discussion. These refer to the fundamental physical quantities, and the method by which they are to be approached and defined. The modern point of view on this question differs radically from that of the older generation of physicists, which was common, let us say, half a century ago.

On the older view, we approach the fundamental quantities from the standpoint of our intuitive knowledge. We have certain conceptions which belong to the familiar world of everyday experience. How these conceptions are acquired is a matter worthy of careful examination, but this point we will for the moment avoid, merely noting that they are there. Thus we recognize a certain 'massiveness' or 'substantiality' as a characteristic of matter, which is the lay equivalent of the scientific concept of mass. Again, the familiar notions of hotness and coldness correspond to the scientific concept of temperature. So too with length and time, which are recognised and known intuitively, quite apart from scientific measurement.

On this view, the process of scientific definition is simply one of the progressive refinement of conceptions. We envisage the quantities as already existing and already known, though perhaps in a rough inexact way. This increasing refinement may be illustrated by the progress of thermometry. From general ideas of hotness and coldness we proceed to Newton's scale of 'degrees of heat' (*e.g.*, "12 degrees: about the heat of a bird hatching its eggs....192 degrees: the heat of coals in a little kitchen fire...."[1] The next step is to prescribe the construction of a mercury thermometer, which enables us to 'measure' the temperature of bodies. Afterwards we have the gas thermometer, then the perfect gas scale, and finally the thermodynamic scale of temperature. It is maintained that we have here methods of measuring temperature which give us, with increasing precision, the value of the 'true temperature' of a body. The 'true temperature' is conceived as a property of the body, having a perfectly definite value, which exists quite apart from our attempts to measure it.

Length, mass and time are conceived in much the same way as intrinsic entities, to which the process of measurement is applied, so to speak, from outside. It is admitted that the exact content of these conceptions cannot be stated fully in words, and the student is therefore referred to his own experience. It is assumed that everyone knows what length is; all the physicist has to do is to discover some accurate method of measuring it.

This view of physical quantities was almost universal until about half a century ago. In such an excellent work as Ganot's *Physics*, for example, (then widely used), this view is implicit in the whole treatment. At the present day, it is still the common method of approach in elementary works, and the quantities are introduced to the student by some general discussion of their everyday counterparts. To take an illustration almost at random, the following way of introducing the idea of temperature is adopted in Kilby's *Introduction to College Physics*: "Ice is said to be cold and steam is said to be hot, but cold and hot are only relative terms. One body is relatively hotter than another when heat will flow from it to the other body, provided they are in thermal communications. However, the quantity of heat that flows, or that is in a body, does not determine its *temperature*, which is defined as a body's thermal condition—its hotness or coldness. But in order to specify the temperature of a body some standard thermal condition, such as that of melting ice, must be used for comparison." It is surely obvious that this definition begs the whole question. Most authors are rather uncomfortable about these introductory remarks, and hurry past them as rapidly as possible.

Even half a century ago quantities (such as *action* and *entropy*) had appeared in physics, to which it was difficult to assign any familiar counterpart. The exact status of these quantities was the subject of much discussion. It was generally held, I think, that entropy was a useful and very

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[1] *Phil. Trans.*, Abridged, **2**, 1701.

convenient integral occurring in thermodynamical theory, but not a physical quantity having the same sort of objective reality as temperature or heat.

The fallacies underlying this intuitive approach to physics were apparent even before the days of relativity to such acute thinkers as Poincaré (*Science and Hypothesis* see especially Chap. VI). The new views became more widely known when the theory of relativity had made it clear beyond all doubt that the only quantities which can be built into a consistent physical scheme are the quantities which result from actual measurement; not the quantities of which we think we have intuitive knowledge. The Newtonian scheme of physics is based on our intuitive conceptions of space and time. In this scheme there is a unique time-relation between any given pair of events, corresponding to our conception of a definite interval elapsing between their moments of occurrence. This interval is conceived to exist quite independently of our observing it, and it is presumed (since physical time is taken to be only a refined version of intuitive time) that measured time intervals will exhibit this same character of uniqueness. We find in practice, however, that the measured time-relation between two events does not actually have this character. If therefore we insist on ascribing uniqueness to it we are led to a theory which is contradicted by the observations.

In modern physical theory length does not appear as an inherent gulf fixed in Nature between two points. It appears as the result of a set of operations with metre rods, performed in a prescribed way. It is precisely this numerical result which is carried into the theory under the name 'length'. It must be emphasized that the result of the prescribed operations *is* the physical quantity 'length', and similar remarks apply to all our other physical quantities. Physical quantities are thus in a sense manufactured articles. As Eddington remarks: "We shall not define a physical quantity as though it were a feature in the world-picture which had to be sought out. *A physical quantity is defined by the series of operations and calculations of which it is the result.*" (*Mathematical Theory of Relativity*; Introduction.)

To define a physical quantity, then, we must prescribe a set of operations. For example, to define length it is first agreed that a particular piece of metal in Paris shall be called a standard metre, and we manufacture copies of it as exactly as possible. We then state a procedure for determining a length AB, namely that standard metres are to be laid down in a straight line between A and B, and the number which can be thus placed is to be counted. This number *is* the length AB. (Fractional values are taken into account by subdividing the unit into standard centimetres, etc.) Experiment shows that the length so determined does not have a unique value, but depends on the motion of the observer. Classical physics endeavours to save the situation by means of an *ad hoc* hypothesis (the Fitzgerald contraction); modern physics simply accepts the facts and recognizes that the quantity defined as length is not an absolute relation between A and B.

In practice more roundabout methods are used for determining length, but they are only valid to the extent to which they are equivalent to the above procedure. A surveyor works from a base line laid out according to the definition, his subsequent measurements are made with a theodolite, utilizing the properties of rays of light. If it turned out that lengths measured in this way disagreed with direct metre-rod determinations it would be necessary to decide which method was legitimate, *i.e.*, to re-define length. In the measurement of mass it is generally recognized that there actually are two alternative procedures, and we distinguish accordingly between gravitational and inertial mass, even though experiment shows that the two are numerically identical. As another illustration may be mentioned the discrepancy in the determinations of e/m for electrons, corresponding to the two types of measurement, crystal diffraction and direct deflection. If this discrepancy proves to be genuine we shall have to distinguish two e/m's, corresponding to the two methods of determination.

Returning to temperature, we now see that it can *only* be defined as the reading on a standard thermometer, *i.e.*, by referring it to the system of operations by which it is obtained. Writers on heat admit this implicitly, since, after the introductory chat about hotness, they devote a chapter to the proper method of constructing a thermometer. This chapter forms the real definition, and we hear no more about hotness. (This concerns, of course, one's first approach to the concept of temperature. At a later stage in the theory of heat, temperature is related to length, mass, and time, and may then be re-defined by reference to these. But in the earlier stages it is on the same footing as the three fundamental quantities, and must be defined accordingly.) To show that temperature has no meaning apart from a thermometer, we have only to ask the question "What is the temperature of interstellar space?" The question as it stands is meaningless, and only acquires a meaning when we have prescribed a procedure by which we imagine the temperature to be determined. Judged by the energy of the residual molecules this temperature may be very high: judged by the intensity of radiation it is certainly very low. In this connection the reviewer wonders why I do not "define force as the indication of a standard dynamometer". This is precisely how force *is* defined, according to Newton's second law. The ideal dynamometer is the free massive particle and the 'indication' is its measured acceleration. In practice, we may use other types of dynamometer but this is merely a matter of practical convenience.

In the quantum theory it is made even clearer that physical quantities are the results of certain operations of measurement and calculation. Unobservables are excluded from the scheme. We have an intuitive conception of a particle in definite place having a definite velocity but we cannot locate electrons by extending this conception to the atom. If we insist on treating electrons along the lines of our intuitive conceptions as miniature billiard balls we are led to a scheme (the earlier Bohr theory) which is contradicted by the more refined observations.

Now it seems to me that in the teaching of physics our definitions, indeed our whole approach to the subject, must conform to what we now know of the nature of physical quantities. Such a definition as "mass is the quantity of matter in a body" is objectionable on two grounds: (*a*) It is manifestly wrong, and has been known to be wrong since the mass of radiation was first recognized. The only possible way of attaching a meaning to the expression "quantity of matter" is to refer it to the number of protons and electrons in a body, and this number is certainly not equivalent to mass in any sense. (*b*) It represents an appeal to intuitive conceptions, which, as indicated above, is a false appeal. I define mass unambiguously from the practical standpoint as the result of certain operations involving a balance and a standard kilogram. Poincaré was even more radical; after a brilliant analysis of the various ideas involved he concluded that the only possible definition was "Masses are coefficients which it is found convenient to introduce into calculations".

Let me not be misunderstood. I am not advocating the introduction of advanced physics into elementary teaching. I am in agreement with Planck on this point, who remarks, "I should consider it extremely dangerous if the intermediate schools were to deal with the theory of relativity or the quantum theory....and I would definitely condemn any attempt to take such a question as that of the universal validity of the principle of conservation of energy—which, of course, to-day is seriously regarded as an open one in nuclear physics—and treat it as debatable before pupils who cannot have properly grasped the meaning of the principle involved, much less its potential scope."* No, the solution of our difficulties does not lie in anticipating advanced work in the elementary stages, but surely the wider view gained in these higher flights may help to determine the direction in which we shall walk on the more pedestrian levels.

The biologists tell us that an organism, in its embryonic stages, recapitulates the past history of its race. I cannot see why this should also be regarded as necessary in educational method. Why should a student be required to recapitulate the mistakes of his predecessors? Why should he not start at once on the right lines? The modren ideas are not intrinsically more difficult than any others, but only less familiar. I find in practice that students grasp these modern definitions quite readily. This is to be expected, since what is clear and explicit is more acceptable to the mind than what is confused and hazy. For beginners the presentation must naturally be made very simple (as in every other kind of teaching). This simplification, I find, offers no difficulty, and even the youngest students can grasp the main essentials.

"What is learnt", says Planck, "is not as important as how it is learnt". But in the teaching of physics there is, most unfortunately, a tremendous weight of the most deadly formality and conservatism, which makes any departure from tradition a most difficult task. The nature of physical quantities is surely a fundamental question, and my remarks will have fulfilled a useful purpose if they serve to stimulate a more general interest in such questions.

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* Planck, *The Philosophy of Physics* (Geo. Allen & Unwin, 1936).