

on such simple grounds of semantics. By implication, then, Carnap's principle of tolerance has been extended from the realm of syntax to the wider field of syntactico-semantics.

In his concluding remarks Dr. Strauss expressed the opinion that the task of the logic of science lies in the future rather than in the past, and that the theory of theory construction, in its relation to theoretical physics, will develop in a way similar to the way in which theoretical physics itself has developed in relation to experimental physics:—from a tool of analysis and synthesis *post factum* through a period of close mutual collaboration to an operative rôle in the progress of science.

Operational Analysis and the Nature of Some Physical Concepts

The following Report by Dr. G. J. Whitrow of the Meeting held on May 5, 1950, appeared in *Nature* of July 15, 1950. It is reproduced here by kind permission of the Editors of *Nature*.

The Philosophy of Science Group of the British Society for the History of Science, under the chairmanship of Prof. J. H. Woodger, held a meeting on 5th May at University College, London. A general discussion took place on questions arising out of Prof. P. W. Bridgman's three lectures on "The Nature of Some of our Physical Concepts" delivered at University College during April 24–28. (These lectures are to be published in the *British Journal for the Philosophy of Science*.) The meeting was attended by about forty members of the Group and visitors.

Prof. Bridgman opened the discussion with a brief summary of his lectures. He thought that their most important feature was the detailed analysis of how the operational technique can be applied to specific physical concepts and problems. In the first lecture, he had discussed general concepts such as field, action at a distance, and empty space; in the second, thermodynamical phenomena, in particular the laws relating to energy and entropy; and in the third, some situations presented by thermo-electric phenomena. Prof. Bridgman believed that the particular novelty of his analysis in these lectures was the self-conscious separation of physical operations into those of an instrumental character and those of a "paper-and-pencil"—that is, mental—character. He had been aware of this distinction for a long time, but only recently had he seen how it applied to what physicists actually do. With regard to this, there are two points of view, as follows.

Instrumental operations correspond most nearly to what used to be called "physical reality". This aspect of physics can be illustrated by an analysis of the field concept. Instrumentally, the existence of a field at a certain place means that, if you go there with the appropriate type of instrument, it will record a certain "reading". Similarly, in applying the first law of thermodynamics, one must specify the elements of space, and in particular their boundaries, on which one operates, so that the fluxes over these boundaries correspond to the actual instrumental readings.

The other aspect relates to the way in which our verbal demands interlock with our instrumental operations. For example, we accept the typical conservation laws of physics as verbal guides with rational implications, but we also find that there exist instrumental operations corresponding to our verbal demands.

It is Prof. Bridgman's belief that, in general, no sharp dividing line can be drawn in physics between the purely instrumental and the purely mental. He regards the whole operational approach as part of a larger programme, according to which we regard the world about us in terms of "activity" rather

than picturing it statically. From this point of view, it is preferable to describe our theories in terms of what we "do" rather than in terms of "things".

Prof. H. Dingle said that, although he is in general agreement with the operational point of view, he attaches less importance than Prof. Bridgman to the distinction between instrumental and paper-and-pencil operations. He prefers to emphasize instead the distinction between those concepts which are associated with measurements, usually denoted by symbols in our equations, and those, such as action at a distance, empty space, etc., which we employ "pictorially". He thought that this differentiation is important because we should confine our operational analysis to concepts of the former type, as he has already explained in some detail in his presidential address to the Group in March 1949 (*Brit. J. Phil. Sci.*, 1, 5; 1950). Prof. Dingle considered that one of the most important applications of the operational principle concerns the separation of the actual experimental results from the theoretical concepts used in visualizing them. As an example, he directed attention to a well-known method of determining the wave-length of monochromatic light by means of a diffraction grating. The fundamental equation can be written as $d \sin\theta = m\lambda$, as explained in standard text-books. Operationally, the numbers d , θ and m (spectrum number) can be directly determined; but the fourth number λ , the wave-length, is never measured directly, that is, from crest to crest. Hence, the operational content of this equation is confined to the invariance of the quantity $d \sin\theta/m$ for different values of d , θ and m . If we wish, we can denote this invariant by the symbol λ . So far, no picture of this invariance need be formed. If, however, we appeal now to theory and regard light as consisting of trains of waves, we can give a theoretical interpretation to λ as a length; whereas, if we regard light as consisting of corpuscles, we can interpret λ as a constant of the dimensions of action divided by momentum. We are at liberty to use any picture we find satisfactory, provided that λ has the correct physical dimensions. The function of such pictures is to stimulate and guide research, to facilitate the mental grasp of the situation, and ultimately to disappear in favour of better ones. These functions are not assisted but rather hindered by the demand that the details of the pictures must satisfy the operational criterion.

Turning to a point of detail, Prof. Dingle said that he had not been convinced by Prof. Bridgman's argument that energy can be located, as it appeared to be at variance with Prof. Bridgman's own demonstration that light cannot be located in transit, but only at points of emission and absorption.

Prof. Bridgman said that he regarded λ as having a larger instrumental component than Prof. Dingle had indicated. He thought that Prof. Dingle had considered this particular question from too narrow a point of view and had omitted the fact that a definite λ is to be correlated with a definite source. The same source can be used for many other experiments, for example, diffraction, Newton's rings, etc., yielding the same λ . This invariant would then be regarded as a fundamental characteristic of the source having not only paper-and-pencil, but also instrumental, significance. We can both usefully calculate with λ as a constant in theoretical work and also connect λ with the instrumental specification of the source. Consequently, he thought that in a wider setting Prof. Dingle's sharp distinction between the instrumental and the mental components would become blurred.

With regard to the specific question about energy and light, Prof. Bridgman remarked that he was glad to have the opportunity of disowning his former view, expressed more than twenty years ago in his book "The Logic of Modern Physics", that energy is not localized. He thought that the localization of energy in a beam of light becomes possible when we can

determine the microscopic E - and H -vectors in the beam as they vary during a period, and can thus determine the fine-scale Poynting vector.

The question came from Mr. C. C. L. Gregory, who inquired about the application of operational analysis to the interior of stars. In view of the practical difficulty of determining temperatures, etc., in such regions, he asked if we ought to regard the subject of stellar structure, at least in so far as it relates to internal constitution, as wholly of a paper-and-pencil character. More generally, he wondered whether physicists do not often assume implicitly that operations can be done instrumentally which, in fact, cannot be so done. He also wondered whether the whole concept of change does not entail difficulties as regards the application of our paper-and-pencil concepts. Certainly, we seem to base physical theories as far as possible on conservation laws which correspond to essentially mental demands.

Dr. G. J. Whitrow agreed that we often tend to regard certain impracticable instrumental processes as possible "in principle" when, strictly speaking, they are not even that. In Prof. Bridgman's discussion concerning our inability to distinguish instrumentally between the concepts of field and action at a distance (with the conclusions of which he fully agreed), he thought that just such an example had been introduced. It has been suggested that gravitational force, whether due to a field or to action at a distance, can in principle be detected instrumentally in a region midway between the earth and the sun. He thought that there is a difficulty here, over and above that arising from the inaccessibility of such a region. In time such a region may cease to be inaccessible, but, unless special precautions can be taken, the instrument concerned will be influenced in the same way as the actual test-mass to be introduced in this region and so will give no "reading". The necessary precautions for counteracting the gravitational effect on the instrument can, of course, be taken on the (almost) rigid surface of the earth, but he thought that in inter-planetary space the situation is very different. In general, he considered that the farther one goes from terrestrial laboratories, etc., the more predominant becomes the paper-and-pencil technique in physics, while the instrumental element tends to be restricted to kinematical rather than dynamical phenomena. In reply, Prof. Bridgman said that he had, of course, intended that the instrument used should be "anchored" to the sun, and he thought that no difficulty arose in principle. Dr. Whitrow considered that this was so impracticable that he found it difficult to imagine that any instrument could be effectively shielded against the gravitational influence of the sun. In subsequent discussion, Prof. K. R. Popper suggested that a proper platform for the gravitation-indicating instrument could in principle be provided by a rocket so controlled as to be moving with uniform velocity relative to the sun.

Dr. W. Ehrenberg asked whether the view that energy is localized would imply that the field concept is right and action at a distance wrong. Prof. Bridgman thought that in any given case a problem such as this can only be answered by carrying out an analysis which will show in detail what the localization is. With regard to gravitation, he thought that the energy can consistently be located in the massive bodies, just as an alternative mathematical method of localizing electrostatic energy is in the charges. If there were not a magnetic field surrounding an electrostatic charge in motion, there would be no Poynting vector. The magnetic field about a moving charge is so small that it is difficult to detect instrumentally when charges are displaced, and until it was discovered energy had to be localized in the charges. But with the discovery of the mere existence of this very small effect, the picture changes qualitatively, and we now visualize the energy as diffused throughout

space. A perfectly consistent picture of the localization of gravitational energy in the masses can be built up.

In reply to a question by Dr. E. H. Hutten, concerning entropy and irreversible thermodynamic processes, Prof. Bridgman said that bodies exist for which every departure from a given configuration is irreversible. He has posed the problem of inventing a generalized entropy function which would be universally applicable and would continually increase for the universe. Dr. Hutten agreed that in Nature there are no truly reversible processes, and that, strictly speaking, the classical entropy concept applies only to abstract processes.

Concerning the fundamental question of the meaning of operational analysis, Dr. Hutten thought that it is important to distinguish between thought-experiments which are essentially of a paper-and-pencil character and others, for example, those relating to the far side of the moon, which are only so because of our present technical limitations. He suggested that, on the lines of the traditional "synthetic-analytic" division of thought, we should describe the division now contemplated as "semantic-syntactic". Prof. Bridgman, however, did not favour a rigid division, and gave it as his opinion that one must work in a laboratory in order to understand fully what operational analysis is.

It was suggested by Dr. M. D. H. Strauss that there are two main uses of operational analysis, namely, for clarifying a given theory, and also for helping to delimit the field of application of a theory. Often a restricted domain of phenomena can be described theoretically in a number of different ways which are operationally equivalent, but when a wider domain is considered some of the competing theories will tend to be discarded. Maxwell's equations, for example, are not applicable to the elementary particles. Prof. Bridgman said that his own remarks concerning these equations applied only to ponderable matter. He thought that, when attempting to construct general theories in physics, it is a mistake to be dominated by fixed intuitions. In his opinion, Einstein made a mistake of this kind in constructing his generalized theory of gravitation, for his point of view was dictated by his intuition concerning the universal application of the field concept.

Mr. L. L. Whyte thought, however, that one of the main functions of operational analysis is its use in clarifying basic principles in physics with the object of extending their field of application. Einstein and Heisenberg, for example, each made a kind of operational analysis directed to differentiating between the measurable and the inherited mental components in certain branches of physics. Their work was essentially positive and creative, for they discovered elements in our thought which were inessential. Our thought became more powerful when these elements were discarded. On a point of detail, Mr. Whyte said that he believed that a thoroughgoing operational analysis should be made of the concept of time in quantum physics. A direct appeal to time is not usually made in determining, say, the velocity of an electron, although in recent work with electron tubes direct time effects are observed. Prof. Bridgman agreed that this whole question ought to be analysed much more fully than it had been hitherto.

In reply to a further question by Mr. Whyte, Prof. Bridgman said that he had worked on high-pressure physics before he began to study operational analysis. This study arose out of a distinct response to his own personal experience; it grew out of his essay on dimensional analysis. It was greatly stimulated by his being suddenly called upon to give a course of lectures on electrodynamics, and so being confronted with the need to think matters out anew for himself.