On the "Meaning" of Scientific Terms

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Or, more generally, for any positive constants j and k,

$$[(1/(n+k)] \quad [nF^n(\alpha,\beta) + kJ]$$

where J is 1 if $j \leq n$ and the jth α is β; otherwise J is 0. Such estimators are additive, convergent, consistent, and linguistically invariant. Yet, given any data stating a sequence of α's in which both β and $\bar{\beta}$ occur, and given any number N between 0 and 1, at least one of those estimators will estimate the frequency of β among α as N.

Hence Salmon requires yet another premise either to the effect that a contraction is as good as the detailed data or that sensible estimators will not be in any way influenced by the arrangement of β among α. Salmon has given no hints on this point. There is one celebrated theory on passage from detailed data to contractions of it: Fisher's theory of sufficient statistics. This theory offers to explain why, in special cases, contractions are as good as the full detailed information. The idea is central not only to those who follow Fisher's objective conception of statistical inference, but also to entirely subjective theories. But since the current theory of sufficiency is tied, in applications, to parametric methods in statistics, it is unclear how Salmon could employ it. Perhaps in a decade or so, when nonparametric theory is better understood, there may appear some way of avoiding the parametric assumptions needed for using sufficiency theory. Only then might Salmon be able to summon up a justifiable premise.

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COMMENTS AND CRITICISM

ON THE "MEANING" OF SCIENTIFIC TERMS

I

IN his criticism of Ryle, Hanson, and me, Peter Achinstein * notices "considerable oversimplification[s]" and discusses "paradoxical consequences" arising therefrom (498). He points out that meanings do not always change with the theory to which they belong and suggests the existence of "various kinds and degrees of dependence as well as independence" between terms and theories (509). He believes that awareness of these different

* "On the Meaning of Scientific Terms," this JOURNAL, 61, 17 (Sept. 17, 1964): 497–509.

kinds and degrees will eliminate the paradoxes and support two assumptions he finds plausible, viz. "A. . . . it is possible to understand at least some terms employed in a . . . theory before (and hence without) learning the principles of that theory"; and "B. It must be possible for two theories employing many of the same terms to be incompatible . . . And this presupposes that at least some of the common terms have the same meaning in both theories" (499).

This belief of Achinstein's seems to be refuted by the existence of pairs of theories that may be regarded as competitors and yet do not share any element of meaning. Attention to "various kinds and degrees of dependence" clearly cannot eliminate such cases—it will rather bring them to the fore. Two examples exhibiting the property just described will be discussed in the next two sections. It will then be argued that, from the point of view of scientific progress, the examples are to be welcomed (IV). It will also be shown that B, despite its prima facie plausibility, is of a very dubious nature (V). Finally, we shall arrive at the result that, in the decision between competing theories, "meanings" play a negligible part and that attention to the "variety of kinds and degrees of dependence," while certainly populating the semantical zoo, does not solve a single philosophical problem.

II

The first example is the pair $[T,T']$, with $T =$ classical celestial mechanics and $T' =$ general theory of relativity. For the purpose of comparison I shall also consider $\overline{T} = T$ except for a slight change in the strength of the gravitation potential.

Now T and $\overline{T}$ are certainly different theories—in our universe, where no region is free from gravitational influence, no two predictions of T and $\overline{T}$ will coincide. Yet it would be rash to say that the transition $T \rightarrow \overline{T}$ involves a change of meaning. For though the *quantitative values* of the forces differ almost everywhere, there is no reason to assert that this is due to the action of different *kinds of entities*. After all, the existence of rubber bands of different strength does not indicate that there are various concepts of "rubber band." Nor does the existence of such a variety indicate that the notion of the surrounding space-time continuum is not well defined. There is nothing mysterious about such stability. The concept "rubber band"—or the more abstract concept "force"—*has been designed* to cover a great variety of entities, among them also entities of different strength. Hence, they are not affected by any transition leading from one element of their extension to another.

This example shows that a diagnosis of *stability of meaning* involves two elements. First, reference is made to rules according to which objects or events are collected into classes. We may say that such rules determine concepts or kinds of objects. Secondly, it is found that the changes brought about by a new point of view occur *within* the extension of these classes and, therefore, leave the concepts unchanged. Conversely, we shall diagnose a *change of meaning* either if a new theory entails that all concepts of the preceding theory have extension zero or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which change the system of classes itself.

It is important to realize that these two criteria lead to unambiguous results only if some further decisions are first made. Theories can be subjected to a variety of interpretations, and the relation of concepts to practice can also be seen in many different ways. Not every interpretation leaves its mark on current procedures. Interesting ideas may therefore be invisible to those who are concerned with the relation between existing formalisms and "experience" only.[1] It follows that we must (a) adopt a certain notion of "interpretation"; and (b) choose from among the various kinds of interpretations consistent with this notion the particular one we prefer. Questions concerning constancy or change of meaning have an unambiguous answer only *after* the decisions just described have been made. Otherwise we are dealing with pseudoproblems which, of course, we can "solve" or "refute" in any manner we please.

In what follows I shall decide (a) and (b) without giving detailed reasons for my decision. No such discussion is needed in the present brief note. All I intend to show now is that a position I hold can be presented coherently and that the alleged paradoxes it creates are harmless. I shall decide (a) by rejecting Platonism. This makes human practice the guide for conceptual considerations and the object of suggestions of conceptual reform. I shall decide (b) by adopting an epistemological realism. This means regarding theoretical principles as fundamental and giving secondary place to the "local grammar," that is, to those peculiarities of the usage of our terms which come forth in their ap-

[1] Berkeley's notion of space as explained in *De Motu* was different from Newton's—but this difference appeared neither in experiment nor in the mathematical formalism accepted by either man. It consisted in an attitude influencing the *future development* of the theory of gravitation.

plication in concrete and, possibly, observable situations. It is intended to subject the local grammar to the theories we possess rather than to interpret the theories in the light of the knowledge —or alleged knowledge—that is expressed in our everyday actions. Or, to put it differently, we want to analyze, to explain, to justify, and perhaps occasionally to correct the "common knowledge" (which may also be the scientific knowledge of the preceding generation) by relating it to new theoretical ideas rather than to interpret such ideas as new ways of talking about what is already well known. This is the way in which fundamental revolutions have taken place in the seventeenth century, and again in the twentieth century; and this is also the way that a reasonable theory of knowledge invites us to take.[2]

Let us now apply these decisions to the case at hand. Will our diagnosis be one of change or of stability? And if it is a diagnosis of change, then what kind of change can we expect? We see at a glance that the spatiotemporal frameworks of T and T' certainly have little in common (three-dimensional Euclidian continuum with absolute time constituting a four-dimensional continuum not permitting any nonsingular metric in the case of T; four-dimensional Riemannian continuum with nonsingular metric in the case of T': not even the over-all topology of the two spaces is the same). Stability of meaning of the spatiotemporal terms can be diagnosed only if we can show that the transition $T \rightarrow T'$ occurs within the extension of a more general idea of space S *that was established already before the advent of* T' [projecting more recent notions back into the past would mean revoking our decision on (a)]. Now I think it will be agreed by everyone that an idea such as S is supposed to be cannot have been part of *common sense* (it would be necessary to assume that common sense is or was able to distinguish between topological, affine, and metrical properties of space and that it is not committed to an unambiguous distinction between spatial and temporal properties). Nor is it possible to locate a suitable S in the *empirical sciences*. Riemann still retained an over-all Euclidian topology. And the contribution of time to the metric did not occur before Einstein. It is of course quite conceivable that S may have been part of some *metaphysical system,* and I for one am prepared to accept a stability of meaning derived from such a background. But, first of all, reference to metaphysics is relevant only if the particular ideas needed are shared by the defenders of T and T'. This is not likely to be the

[2] See my "Problems of Empiricism," in *Beyond the Edge of Certainty.* Robert G. Colodny, ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1965).

case (absolutism on the part of Newton; relational theories on the part of the forerunners of relativity). Secondly, we may safely assume that metaphysical S's will be rejected by empiricists—and this includes my opponent. Result: the transition $T \rightarrow T'$ involves a change in the meaning of spatiotemporal notions.

This change is drastic enough to exclude the possibility of common elements of meaning between T and T'. To see this, consider the notion of the spatial distance between two simultaneous events, A and B. It may be readily admitted that the transition from T to T' will not lead to new methods for estimating the size of an egg at the grocery store or for measuring the distance between the points of support of a suspension bridge. But in considering (b) we have already decided not to pay attention to any prima facie similarities that might arise at the observational level, but to base our judgment on the principles of the theory only. It may also be admitted that distances that are not too large will still obey the law of Pythagoras. Again we must point out that we are not interested in the empirical regularities we might find in some domain with our imperfect measuring instruments, but in the laws imported into this domain by our theories. Now these laws are very different in T and T'. According to T, the distance (AB) is a *property* of the situation in which A and B occur; it is independent of signal velocities, gravitational fields, and the motion of the observer. An observer can influence (AB) only by actively interfering with either A or B. Any process on the part of the observer that does not reach either A or B leaves the distance unchanged. According to T', (AB) is a projection, into the spacetime frame of the observer, of the four-dimensional *interval* $[AB]$. $(AB)_{T'}$ will change even in those cases where a causal influence upon either A or B is excluded in principle. Now one might still wish to retain the idea of a common core of meaning by interpreting the difference between $(AB)_T$ and $(AB)_{T'}$ as being due to the different *assumptions* about "space and time" contained in T and T', respectively. And the locution 'space and time' would now refer to what can be characterized independently of either T or T', though in a manner that conflicts with neither (the last proviso is necessary in order to prevent a return to common sense). Evidently it would correspond to the S mentioned above. We have already shown that no such idea can be assumed to exist. It follows, then, that the difference between $(AB)_T$ and $(AB)_{T'}$ is wholly due to the meanings of the notions used for explaining their properties. In traditional philosophical terminology: $(AB)_T$ and $(AB)_{T'}$ are *constituted* by the basic principles of T and T', *respectively.* These entities cannot be described, not even in part, by means that are independent of either theory at the time of the advent of T'. In earlier papers I have expressed the fact by saying that "$(AB)_T$" and "$(AB)_{T'}$" are *incommensurable notions.*

III

The very same considerations apply if we consider the transition $T =$ classical mechanics $\rightarrow T'' =$ quantum theory (I am now talking about the elementary quantum theory in the form in which it has been developed by von Neumann, and not about the earlier and more intuitive ideas of Rutherford, Bohr, and Sommerfeld). T'' introduces properties whose universalization [3] is possessed by a physical system only if certain conditions are first satisfied. This is true of all so-called "dynamical" properties (angular momentum, which Achinstein discusses on pp. 499 and 503 is a dynamical property, and so are position, momentum, spin, etc.; electrical charge is not a dynamical property, at least not within the framework of the elementary theory). Now if we adopt an interpretation of the elementary theory that ascribes this feature to microproperties and macroproperties alike [4] and if we also decide to retain a two-valued logic,[5] we discover again that T and T'' are incommensurable. This feature of the pair has been discussed ever since Bohr introduced the principle of correspondence.

Some authors [6] have commented on the difficulties connected with a principle that apparently ties together two theories whose concepts cannot be accommodated in a single point of view. This is not the place to examine the matter in detail.[7] Let me only emphasize that the reappearance of conservation laws in the quantum theory cannot be regarded as an argument in favor of a common core of meaning (for such an argument, cf. Achinstein, p. 507). For it is clear that the "conservation laws" of the quantum theory share only the name with the corresponding laws of classical physics. They are expressed in terms of Hermitian

[3] If P is a property, then $P \vee \sim P$ will be called its "universalization."

[4] This interpretation is suggested by Temple's proof of isomorphism (*Nature,* **135**: 951; cf. also Groenewold, *Physica,* **12**: 405 ff). It agrees with our decision on (b).

[5] For reasons, cf. my paper in vol. I of the *Publications of the Salzburg Institute for the Philosophy of Science,* Salzburg, 1965.

[6] Cf. N. R. Hanson, *Patterns of Discovery* (New York: Cambridge, 1958), as well as my comments in *Philosophical Review,* **69**, 3 (July, 1960): 251.

[7] According to Bohr, the principle of correspondence is a theorem of the quantum theory rather than a "bridge law" connecting quantum mechanics and classical physics.

operators, whereas the classical laws use ordinary functions that always have some value. They allow for "virtual states" which are, strictly speaking, incompatible with conservation. No such states are possible in classical physics. They make use of properties that cannot be universalized simultaneously (position in the potential energy, momentum in the kinetic part) whereas classical properties can always be universalized. Only insufficient analysis could make one believe that the occurrence of so-called conservation laws in the two theories establishes a common core of meaning.

IV

The above considerations, though not sufficient to settle the matter, still provide at least strong prima facie evidence for the existence of "paradoxical" cases in Achinstein's sense. I do not see how the attention to details that Achinstein recommends is going to eliminate these cases. What we need are not further details, but principles such as those involved in our decisions on (a) and (b), which teach us how to evaluate various proposals and which remove the ambiguities characteristic of all semantic information: we can relate the "local grammar" of well-known expressions to different theories in different ways (realism, instrumentalism) and thereby give them different meanings. The adoption of such principles will be guided partly by actual scientific practice, partly by the demands of a reasonable methodology, such as maximum testability.[8] Now, according to both these guides, major revolutions are preferable to small adjustments, since they affect and, thereby, lay open to criticism even the most fundamental assumptions. Achinstein's rule B (which I would formulate as saying that competing theories must have common meanings) puts a restriction on the extent to which we are allowed to revise such assumptions. It does so in the belief that theories can compete only if they are incompatible and that they can be incompatible only if they have common meanings. Is this transcendental defence of an epistemological conservatism effective? In order to decide the question we return again to our first example, the relation between the general theory of relativity and classical physics.

V

Combine T' with two assumptions which are contrary to fact, viz. (i) the over-all metric of space is almost Minkowskian; and

[8] For details, cf. my "Reply to Critics," which discusses objections by Smart, Putnam, Sellars, forthcoming in vol. II of the *Boston Studies in the Philosophy of Science.*

(ii) the velocity of light is almost infinite. These two assumptions do not change the semantical properties of T'. T and T' are still incommensurable. Yet it is possible, to a high degree of approximation, to establish an isomorphism between certain selected semantical properties of some (not all) descriptive statements of T' and of some (not all) descriptive statements of T (let the corresponding classes be C and C', respectively). The isomorphism will be valid for finite distances (AB), but not for distances approaching infinity. It will be valid for a finite number of parallel displacements of (AB) around a closed curve, but no longer if this number approaches infinity. Considering that meanings are dependent on structure and not on the particular ways in which the structure is realized, we may say that, within the restrictions given, C and C' have a common core of meaning. We may even identify C and C'. (As $C \neq T$ and $C' \neq T'$, this does not affect the relation between T and T'). However, C' is formulated in terms of T' and can, therefore, be examined in the manner preferred by Achinstein (meanings shared between the critic and the point of view criticized) within that theory. The examination will of course lead to the rejection of C' and, via the isomorphism, of C, and, as C is part of T, of T also. We see that the show can be rigged in such a manner that the demands for partial stability of meaning are satisfied. But the very method of rigging indicates that the demand is superfluous: when making a comparative evaluation of classical physics and of general relativity we do not compare meanings; we investigate the conditions under which a structural similarity can be obtained. If these conditions are contrary to fact, then the theory that does not contain them supersedes the theory whose structure can be mimicked only if the conditions hold (it is now quite irrelevant in what theory and, therefore, in what terms the conditions are framed). It may well be that those champions of T or of T' who see light only when they see—or believe they see—meanings "are always at least slightly at cross purposes."[9] The fact that argument proceeded even through the most fundamental upheavals, that it was understood, and that it led to results[10] shows that meanings cannot be that essential. I conclude, then, that principle B is neither necessary nor desirable.

[9] T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: Univ. of Chicago Press, 1962), p. 147.

[10] The debates during the period of the older quantum theory are an excellent example of discussions of this kind.

VI

These results can be immediately applied to such notorious museum pieces as the mind-body problem, the problem of the existence of the external world, the problem of free will, and to many other problems.[11] In all such cases "new" points of view (which are actually as old as the hills) are criticized because they lead to drastic structural changes of our knowledge and are therefore inaccessible to those whose understanding is tied to certain principles. Now this conservation may well have a physiological foundation. Education, as Prof. Z. Young has put it so well,[12] consists in seriously damaging our central nervous system and in eliminating reactions of which it was initially capable. Admitting such damage and the consequent lack of imagination is one thing. However, one should never go so far as to try to inflict it upon others in the guise of a philosophical dogma.[13]

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A NOTE ON SCHEFFLER ON NIDDITCH

IN a 1961 note P. Nidditch[1] attempted to rescue Ayer's revised formulation of the verifiability principle[2] from Church's well-known attack.[3] Tucked away in a footnote of Israel Scheffler's richly rewarding *Anatomy of Inquiry*[4] is an argument purporting to prove that Nidditch's retrenchment has the unwelcome consequence (call it "UC") that "every statement *S* whatever, unless it is simply a logical consequence of an observation-statement, is indirectly verifiable." We show that Scheffler's argument pins somewhat less than UC on Nidditch, but that with a slight modification it delivers UC and much more.

11 For details cf. again my paper referred to in footnote 2.

12 *Hitchcock Lectures*, University of California in Berkeley, 1964.

13 For details, cf. the paper referred to in footnote 8.

1 P. Nidditch, "A Defence of Ayer's Verifiability Principle against Church's Criticism," *Mind*, **70** (1961): 88–89.

2 A. J. Ayer, *Language, Truth and Logic*, 2d ed., (New York: Dover, 1946), p. 13.

3 A. Church, "Review of A. J. Ayer, *Language, Truth and Logic*, Second Edition," *Journal of Symbolic Logic*, **14** (1949): 52–53.

4 Israel Scheffler, *The Anatomy of Inquiry* (New York: Knopf, 1963), p. 154n.

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[Footnotes]

[*] **On the Meaning of Scientific Terms**
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[6] **Patterns of Discovery**
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