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Empirical Realism and Classical Semantics: A Parting of the Ways

Abstract

Part I[1] is concerned with the tenet of modern Empirical Realism that while the theoretical concepts employed in science obtain their *meanings* entirely from the connections their usage establishes with the data language, the *referents* of such terms may be "unobservables," that is, entities which cannot be discussed within the data language alone. Such a view avoids both the restrictive excesses of logical positivism and the epistemic laxity of transcendentalism; however, it also necessitates a break with classical semantics, for it follows from the empirical realistic position that a theoretical term must in principle be capable of simultaneously designating not just one entity, but indefinitely many.

Suppose that in my vocabulary I have a stock of "observation" terms which designate entities (i.e., objects, attributes, relations, etc.) that I have personally observed—entities, that is, with which I have been "directly acquainted," or have "immediately experienced." (For present purposes it does not matter whether the "observed" is interpreted to be sense data, the commonsense world of things, or something else, nor whether one has doubts about the possibility of drawing a sharp line between the observed and the unobserved, so long as it be agreed that the intuitive notions of "observe," "experience," etc., their vagueness and disputed loci in philosophical space notwithstanding, somehow reflect a distinction of sufficient epistemological importance to warrant a more-or-less idealized reconstruction.) Observation terms, logical terms, and the syntactically proper expressions I can construct from them then constitute my "observation language," the (well-formed) sentences of which are cognitively meaningful because their constituent terms are cognitively meaningful. Now: Is it possible for a sentence which is *not* in my observation language to be cognitively meaningful to me? Or, somewhat less linguistically, is it possible for me to have knowledge or beliefs about entities which I have never observed?

It is frequently believed that an empiricist epistemology necessitates a negative answer to the question just posed. Such a conviction, for example, would seem to have inspired Russell's famous analysis of definite descriptions, according to which statements incorporating definite descriptions which *seem* to extend the range of reference beyond the realm of direct acquaintence (e.g., "the far side

[1] (Ed.) Part II, *On the Equivalence of Scientific Theories*, is not included here.

of the moon," "the center of the sun") are *really* elliptical for statements whose descriptive terms refer only to elements of immediate experience. Similarly, Stace has recently defined "the general principle of empiricism" to be:

> "All verbal expressions, whether they consist in single terms or in complete sentences, must if they are to possess cognitive meaning or significance, either refer to some specific but unanalysable experiential datum, or be amenable to a process of analysis the end-terms of which will be such experiential data" (Stace, 1958).

But such an interpretation of empiricism is entirely too severe. One can maintain that all knowledge *derives from* or is *acquired through* experience without thereby being committed to the much stronger view that all knowledge is *about* experience. It is not at all inconceivable that I might be able to cognize about certain unobserved entities by means of concepts whose ability to refer to such entities has been acquired solely through their suitable deployment among expressions in my observation language. In fact, this is the epistemology now held by most logical empiricists. According to this view, known as Empirical Realism and perhaps expressed most clearly in the writings of Herbert Feigl (e.g., 1950, p. 16 ff), many terms which see important application in science and everyday life are not explicitly definable in the observation language, but refer, if all has gone well, to entities which lie outside our rather narrow scope of past and present experience;[2] yet the *meanings* of such terms are held to accrue entirely from the accepted statements (usually lawlike) which connect them with concepts in the observation language.

A particularly explicit formulation of the tenets of Empirical Realism is afforded by the methodological analysis of scientific theories. Let a scientific term which does not belong to the observation, or "data" language of science be called a "theoretical" term, and let a scientific "theory" be the conjunction of all accepted postulates containing one or more theoretical terms. Then a scientific theory may be written as a (presumably complex) sentence, '$T(\tau_i, \ldots, \tau_n)$' in which the '$\tau_i$' are the theoretical terms and the sentential matrix '$T(\ , \ldots, \ )$' contains only terms in the observation language. Accordingly, an accepted theory may be regarded as an ascription of the observational predicate '$T(\phi_1, \ldots, \phi_n)$' to a set of otherwise unidentified entities designated by the theoretical terms 'τ_1', ...,'τ_n'. The empirical realist (in contrast to the logical positivist) then wishes to maintain that even though the 'τ_i' cannot be defined explicitly in observational terms, 'τ_i' are (in general) cognitively meaningful and may, in fact, refer to extra-observational entities, but that they have whatever meaning they do have *because* they instantiate the observational predicate '$T(\phi_1, \ldots, \phi_n)$' to form an accepted theory.

[2]The designata of such terms are usually described as "unobserv*ables*," which is a blunder. There are quite enough problems in the epistemology of the unobserved without confounding them with questions about the possible limits of future experience.

As an epistemological thesis, Empirical Realism has much to recommend it, both by way of common sense and by its effortless avoidance of numerous difficulties which beset alternative epistemologies. Nonetheless, it does have one disturbing semantical consequence which so far seems to have escaped general recognition; namely, that the empirical realist is committed to abandon the classical semantic principle that within a fixed context of usage, *a descriptive term can have at most one referent*. I have explored this situation at some length in a recent monograph (Rozeboom, 1962), but the essence of the difficulty can be summarized quite simply: To say that the theoretical terms in a theory '$T(\tau_i, \ldots, \tau_n)$' are *given* their meanings by the connections so established between the 'τ_i' and expressions in the observation language is essentially to say that the referents, if any, of the 'τ_i' are determined wholly by the predicate '$T(\phi_1, \ldots, \phi_n)$'—that is, that this predicate constitutes the sole *criterion* for the designata of the 'τ_i'. But this criterion is (in general) unable to single out a *unique* set of entities designated by the 'τ_i', and hence if a theory imparts referents to its theoretical terms at all, it must in general tolerate a multiplicity of such referents. This conclusion is not an easy one to accept—there are competent philosophers to whom a theory of multiple reference seems too ludicrous to warrant serious consideration—and my object in Part I of this paper is to present some elementary examples of garden variety theories in order to make as intuitively clear as possible that *if* one is to give a realistic interpretation to these theories, *then* no alternative remains but to grant multiple designata to the theoretical terms involved.

1

Suppose that 'Johm' is a theoretical term introduced by the theory,

> 'Johm ransacked the Smiths' residence while
> the Smiths were on vacation.'

(While not extensive, this theory has pragmatic value—it explains, for example, why the Smiths found their possessions in such disarray when they returned, and why certain of their valuables were missing.) If this theory is correct, Johm should be prosecuted for a criminal offense, the implementation of which, however, necessitates discovery of Johm's identity. That is, if this theory is taken seriously, we want to know *who* Johm is. Now, unless we have some transcendental access to the referents of theoretical terms, the identities of theoretical entities must be determined wholly by the observational properties ascribed to them by the theory. In this instance, the sole criterion for application of the name 'Johm' to an individual i is whether or not i ransacked the Smiths' residence while the Smiths were

on vacation. Suppose that as a matter of fact, the Smiths' residence was ransacked twice while they were away, once by Pat and once by Mike. Then there is no way for 'Johm' to designate Pat to the exclusion of Mike, or conversely, and hence if 'Johm' refers to anybody at all, it must refer *both* to Pat and to Mike. (There are no reasonable grounds at all on which to argue that 'Johm' designates someone who did *not* ransack the Smiths' residence.) It could, of course, be contended that it is intrinsic in the use of a name, theoretical or otherwise, that it designate at most one entity. But then, since the present theory provides no way to single out a unique referent for 'Johm', it would follow that 'Johm', so introduced, does *not* designate anyone. And what *is* the cognitive status of this theory, then—true, false, meaningless calculation device, or what?

One superficially plausible attack on this problem is to contend that to adopt the theory, 'Johm ransacked the Smiths' residence [etc.]', rather than, e.g., the alternative theory, 'Johm and perhaps other persons ransacked the Smiths' residence [etc.]', is to assume that there was only one burglar involved, and that since there were actually two, the theory is in fact false. This argument will not stand much inspection—for example, why should the sentence derived from 'x ransacked the Smiths' residence [etc.]' by instantiation with a theoretical term presuppose uniqueness when no such commitment is made when the predicate is ascribed to a person whose identity is already known? But even if some such approach could be carried through in the present instance, it would still not resolve the clash between empirical realism and classical semantics, for even if a theory makes commitment to the exact number of entities satisfying the observational predicate which characterizes the theory, and this is, in fact, the correct number, it will still in general be necessary to grant multiple designata to the theoretical terms if they are to be construed as designating at all. Suppose, for example, that after sifting the evidence, the police suspect the existence of a second intruder, but no more, and introduce another theoretical name, 'Jin', in addition to 'Johm', by the theory

> 'The Smiths' residence was ransacked by Johm, and also by another person, Jin, but by nobody else.'

Suppose, as before, that the dastardly deed was done by exactly Pat and Mike. Then the theory is correct in implying the existence of exactly two intruders, and supplies a name for each one. But does 'Johm' designate Pat and 'Jin' designate Mike, or does 'Johm' designate Mike and 'Jin' designate Pat? Both the ordered pair ⟨Pat, Mike⟩, and the ordered pair ⟨Mike, Pat⟩, satisfy 'The Smiths' residence was ransacked by x, and also by another person y, but by nobody else', yet whether or not this predicate is satisfied by a pair of entities $\langle t_1, t_2 \rangle$ is the sole criterion for whether or not 'Johm' and 'Jin' designate t_1 and t_2, respectively. If we insist on unique designata for 'Johm' and 'Jin', it would seem that these terms cannot

designate when introduced by this theory. Now, can a theory ever be *true* (over and above being an effective calculation device) if it contains name-like terms which have no referents? Yet if 'The Smiths' residence was ransacked by Johm, and also by another person Jin, but by nobody else' cannot be true even though there are exactly two such persons, under what conceivable circumstances *could* it be semantically true, and what, then, has happened to the realistic interpretation of theories?

It may be argued that a *proper* name, by definition, designates exactly one individual, and hence that if 'Johm', introduced as a theoretical term in the above fashion, designates both Pat and Mike, it must necessarily be a *general* term. I see no strong objection to this move so long as it is appreciated that the realistic interpretation of theories would thereby admit general terms of zero type level, and that 'Johm' is *not* the name of a *class*.

2

A generic objection which might be raised (though in my opinion incorrectly) against the preceding example is that theoretical terms are there used in such a way that if they are to designate at all, they must designate *particulars*—the point of the objection being that this is an illegitimate usage. In fact, I suspect that a major reason why the semantical problems of Empirical Realism have not heretofore been recognized is a widespread belief that theoretical terms which arise in actual scientific practice always refer to classes if they refer at all, and that if several classes of entities have the observational properties ascribed by the theory to a theoretical class, then the latter is simply the union of the former. For example, now that modern physics has replaced the older chemical concept of 'Hydrogen' with 'Hydrogen$_1$', 'Deutrium', and 'Tritium', it is easy (and in this instance perhaps correct) to assume that the classes referred to by the latter terms are merely subclasses of the class of atoms designated by 'Hydrogen' in its sense, say, at the turn of the century. More formally, the situation may be illustrated as follows: Suppose '$T(\tau)$' is a theory which introduces a theoretical class-term 'τ' by the observational predicate '$T(\phi)$', and suppose also that further research discovers two distinct classes, c_1 and c_2, both of which satisfy '$T(\phi)$'. Then what is the class, if any, to which 'τ' in '$T(\tau)$' refers? The standard assumption seems to be that τ-kind is simply those entities which belong either to c_1 or to c_2, and hence that $\tau = c_1 \cup c_2$.

But this just won't work. For if 'τ' is introduced by theory '$T(\tau)$', then whatever is designated by 'τ' must satisfy '$T(\phi)$', and it is by no means always the case that whenever classes c_1 and c_2 satisfy '$T(\phi)$', their union also does so. For example, suppose that '$T(\tau)$' entails that 'τ' has exactly n members, and suppose

also that c_1 and c_2 are mutually distinct classes each of which satisfies '$T(\phi)$'. Then the union of c_1 and c_2 has *more* than n members, and it is hence false that $T(c_1 \cup c_2)$.[3]

To see that the situation here envisioned abstractly is, in fact, a genuine possibility for what might arise in scientific practice, consider the following hypothetical but not implausible example. Suppose that a certain physicist studying proton tracks in bubble chamber photographs concludes that one particle occasionally swerves away from another in a manner which cannot be accounted for by repulsion between like charges, collision, or any other classical process by which one particle might deflect another. This new form of repulsion appears to involve only protons, and only a small proportion of protons seem to affect one another in this way. To account for his data, our physicist hypothesizes the existence of a special kind of particle, the τ-proton, which has all the properties of orthodox protons (consistent with the property to follow) except that for some reason, τ–protons cannot come within a distance of r mm. of one another, although no effect is observed at distances greater than r mm., and other particles approach τ-protons in the normal fashion. If this theory is correct, it follows that a given volume of hydrogen should have a compressibility limit computable from the parameter r and the number of τ-protons present in that volume. Sure enough, hydrogen does turn out to be not as compressible as classical gas theory predicts, and our physicist is able to hypothesize, on the basis of his compression data, that in ordinary hydrogen, p% of the nuclei are τ-protons. We thus have a new theoretical particle-kind, τ-protons, introduced by the postulates (together with the rest of particle theory) that no two τ-protons are ever within r mm. of each other and that τ-protons constitute p% of the nuclei of ordinary hydrogen.

When shortly thereafter the meson-microscope is developed, by which atoms can be observed directly, our physicist eagerly searches for direct confirmation of his theory. Nor are his hopes thwarted, for it is soon discovered that while most protons look grey under the meson-microscope, there is a small percentage p% in ordinary hydrogen, of blue ones which never get closer than r mm. to one another. Surely this confirms the theory. But alas, there are also a few red protons, again p% of ordinary hydrogen, which likewise never come closer than r mm. to one another, even though no such exclusion holds between red protons and blue protons, or between red or blue protons and other particles. (Note that there is no incompatibility between this possibility and the result of the compression experiment—both the presence of red protons and the presence of blue protons prevent compression beyond a certain minimal volume, but the effects are strictly

[3]Even if the union of classes satisfying '$T(\phi)$' *does* also satisfy '$T(\phi)$', there are still no valid grounds on which to argue that 'τ', introduced by '$T(\tau)$', refers only to this union. If c_1, c_2, and $c_1 \cup c_2$ all satisfy '$T(\phi)$', the theory does not provide a criterion by which $c_1 \cup c_2$, rather than c_1 or c_2, is selected as a unique referent of 'τ'.

non-cumulative across kinds.) But which protons are the τ-protons? By the only criteria provided by the theory, red protons are τ-protons to precisely the same extent that blue protons are. Yet the class of τ-protons cannot be the class of red-or-blue protons, for the latter violates the postulates by which 'τ-proton' was given meaning in two distinct ways: (a) ordinary hydrogen contains only p% of τ-protons, but $2p$% red-or-blue protons, and (b) no two τ-protons can come within r mm. of each other, whereas this is not true of red-or-blue protons since a red proton can approach a blue one without this restriction. But are we then to conclude that there are no τ-protons? If so, what are we to say about this theory from the realist's standpoint?

Moreover, a case similar to that of Johm and Jin arises here. Suppose that our physicist's earlier data had led him to hypothesize not one but two special kinds of protons, τ_1-protons and τ_2-protons, each comprising p% of ordinary hydrogen and such that protons of each special type exclude other protons of its type, and only these, from within a radius of r mm. Then surely discovery of red and blue protons constitutes a triumphant verification of the theory. But do 'τ_1-proton' and 'τ_2-proton' denote red protons and blue protons, respectively, or is it the reverse? Or is there some other alternative, apart from allowing a term to have more than one referent, which will let the theory provide the sole criterion for the possible referents of its constituent theoretical terms and still grant them a genuine semantical status?

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These examples are sufficiently simple and close to actual theoretical practice that any account of the semantico-epistemological status of theories which is not fully able to answer the questions they raise cannot pretend to adequacy. Do 'Johm' (or 'Johm' and 'Jin') and 'τ-proton' (or 'τ_1-proton' and 'τ_2-proton'), as introduced above, designate anything when the facts are as stipulated? If so, *what* are their designata? Or if they *don't* designate anything, are these theories then false? If so, how could they be modified to make them true, and what conditions, if any, would be necessary for them to be true as they now stand? Or if, under the conditions given, it is neither the case that these theoretical terms have referents nor that the theories which introduce them are false, does it not then follow that a sentence-like expression containing theoretical terms is merely a cognitively meaningless device for deriving sentences in the observation language? I submit that the empirical realist has no alternative (short of relinquishing his epistemology) but to develop a theory of semantics built around the possibility that a descriptive term may have more than one referent.

References

Feigl, H. (1950). Existential hypotheses. *Philosophy of Science*, *17*, 35–62.

Rozeboom, W. W. (1962). The factual content of theoretical concepts. In H. Feigl & G. Maxwell (Eds.), *Minnesota studies in the philosophy of science* (Vol. 3). Minneapolis: University of Minnesota Press.

Stace, W. T. (1958). Some misinterpretations of empiricism. *Mind*, *67*, 465–484.