# *The Ontological Status of Theoretical Entities*

That anyone today should seriously contend that the entities referred to by scientific theories are only convenient fictions, or that talk about such entities is translatable without remainder into talk about sense contents or everyday physical objects, or that such talk should be regarded as belonging to a mere calculating device and, thus, without cognitive content—such contentions strike me as so incongruous with the scientific and rational attitude and practice that I feel this paper *should* turn out to be a demolition of straw men. But the instrumentalist views of outstanding physicists such as Bohr and Heisenberg are too well known to be cited, and in a recent book of great competence, Professor Ernest Nagel concludes that "the opposition between [the realist and the instrumentalist] views [of theories] is a conflict over preferred modes of speech" and "the question as to which of them is the 'correct position' has only terminological interest."[1] The phoenix, it seems, will not be laid to rest.

The literature on the subject is, of course, voluminous, and a comprehensive treatment of the problem is far beyond the scope of one essay. I shall limit myself to a small number of constructive arguments (for a radically realistic interpretation of theories) and to a critical examination of some of the more crucial assumptions (sometimes tacit, sometimes explicit) that seem to have generated most of the problems in this area.[2]

[1] E. Nagel, *The Structure of Science* (New York: Harcourt, Brace, and World, 1961), Ch. 6.

[2] For the genesis and part of the content of some of the ideas expressed herein, I am indebted to a number of sources; some of the more influential are H. Feigl, "Existential Hypotheses," *Philosophy of Science*, 17:35–62 (1950); P. K. Feyerabend, "An Attempt at a Realistic Interpretation of Experience," *Proceedings of the Aristotelian Society*, 58:144–170 (1958); N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958); E. Nagel, *loc. cit.*; Karl Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959); M. Scriven, "Definitions, Explanations, and Theories," in *Minnesota Studies in the Philosophy of Science*,

## The Problem

Although this essay is not comprehensive, it aspires to be fairly self-contained. Let me, therefore, give a pseudohistorical introduction to the problem with a piece of science fiction (or fictional science).

In the days before the advent of microscopes, there lived a Pasteur-like scientist whom, following the usual custom, I shall call Jones. Reflecting on the fact that certain diseases seemed to be transmitted from one person to another by means of bodily contact or by contact with articles handled previously by an afflicted person, Jones began to speculate about the mechanism of the transmission. As a "heuristic crutch," he recalled that there is an obvious *observable* mechanism for transmission of certain afflictions (such as body lice), and he postulated that all, or most, infectious diseases were spread in a similar manner but that in most cases the corresponding "bugs" were too small to be seen and, possibly, that some of them lived inside the bodies of their hosts. Jones proceeded to develop his theory and to examine its testable consequences. Some of these seemed to be of great importance for preventing the spread of disease.

After years of struggle with incredulous recalcitrance, Jones managed to get some of his preventative measures adopted. Contact with or proximity to diseased persons was avoided when possible, and articles which they handled were "disinfected" (a word coined by Jones) either by means of high temperatures or by treating them with certain toxic preparations which Jones termed "disinfectants." The results were spectacular: within ten years the death rate had declined 40 per cent. Jones and his theory received their well-deserved recognition.

However, the "crobes" (the theoretical term coined by Jones to refer to the disease-producing organisms) aroused considerable anxiety among many of the philosophers and philosophically inclined scientists of the day. The expression of this anxiety usually began something like this: "In order to account for the facts, Jones must assume that his crobes are too small to be seen. Thus the very postulates of his theory preclude

Vol. II, H. Feigl, M. Scriven, and G. Maxwell, eds. (Minneapolis: University of Minnesota Press, 1958); Wilfrid Sellars, "Empiricism and the Philosophy of Mind," in *Minnesota Studies in the Philosophy of Science*, Vol. I, H. Feigl and M. Scriven, eds. (Minneapolis: University of Minnesota Press, 1956), and "The Language of Theories," in *Current Issues in the Philosophy of Science*, H. Feigl and G. Maxwell, eds. (New York: Holt, Rinehart, and Winston, 1961).





their being observed; they are *unobservable in principle*." (Recall that no one had envisaged such a thing as a microscope.) This common prefatory remark was then followed by a number of different "analyses" and "interpretations" of Jones' theory. According to one of these, the tiny organisms were merely convenient fictions—*façons de parler*—extremely useful as heuristic devices for facilitating (in the "context of discovery") the thinking of scientists but not to be taken seriously in the sphere of cognitive knowledge (in the "context of justification"). A closely related view was that Jones' theory was merely an instrument, useful for organizing observation statements and (thus) for producing desired results, and that, therefore, it made no more sense to ask what was the nature of the entities to which it referred than it did to ask what was the nature of the entities to which a hammer or any other tool referred.[3] "Yes," a philosopher might have said, "Jones' theoretical expressions are just meaningless sounds or marks on paper which, when correlated with observation sentences by appropriate syntactical rules, enable us to predict successfully and otherwise organize data in a convenient fashion." These philosophers called themselves "instrumentalists."

According to another view (which, however, soon became unfashionable), although expressions containing Jones' theoretical terms were genuine sentences, they were translatable without remainder into a set (perhaps infinite) of observation sentences. For example, 'There are crobes of disease X on this article' was said to translate into something like this: 'If a person handles this article without taking certain precautions, he will (probably) contract disease X; and if this article is first raised to a high temperature, then if a person handles it at any time afterward, before it comes into contact with another person with disease X, he will (probably) not contract disease X; and . . .'

Now virtually all who held any of the views so far noted granted, even insisted, that theories played a useful and legitimate role in the scientific enterprise. Their concern was the elimination of "pseudo problems" which might arise, say, when one began wondering about the "reality of supraempirical entities," etc. However, there was also a school of thought, founded by a psychologist named Pelter, which differed in an

[3] I have borrowed the hammer analogy from E. Nagel, "Science and [Feigl's] Semantic Realism," *Philosophy of Science*, 17:174–181 (1950), but it should be pointed out that Professor Nagel makes it clear that he does not necessarily subscribe to the view which he is explaining.

interesting manner from such positions as these. Its members held that while Jones' crobes might very well exist and enjoy "full-blown reality," they should not be the concern of medical research at all. They insisted that if Jones had employed the correct methodology, he would have discovered, even sooner and with much less effort, all of the observation laws relating to disease contraction, transmission, etc. without introducing superfluous links (the crobes) into the causal chain.

Now, lest any reader find himself waxing impatient, let me hasten to emphasize that this crude parody is not intended to convince anyone, or even to cast serious doubt upon sophisticated varieties of any of the reductionistic positions caricatured (some of them not too severely, I would contend) above. I am well aware that there are theoretical entities and theoretical entities, some of whose conceptual and theoretical statuses differ in important respects from Jones' crobes. (I shall discuss some of these later.) Allow me, then, to bring the Jonesean prelude to our examination of observability to a hasty conclusion.

Now Jones had the good fortune to live to see the invention of the compound microscope. His crobes were "observed" in great detail, and it became possible to identify the specific kind of *microbe* (for so they began to be called) which was responsible for each different disease. Some philosophers freely admitted error and were converted to realist positions concerning theories. Others resorted to subjective idealism or to a thoroughgoing phenomenalism, of which there were two principal varieties. According to one, the one "legitimate" observation language had for its descriptive terms only those which referred to sense data. The other maintained the stronger thesis that *all* "factual" statements were *translatable* without remainder into the sense-datum language. In either case, any two non-sense data (e.g., a theoretical entity and what would ordinarily be called an "observable physical object") had virtually the same status. Others contrived means of modifying their views much less drastically. One group maintained that Jones' crobes actually never had been unobservable in principle, for, they said, the theory did not imply the impossibility of finding a means (e.g., the microscope) of observing them. A more radical contention was that the crobes were not observed at all; it was argued that what was seen by means of the microscope was just a shadow or an image rather than a corporeal organism.





## The Observational-Theoretical Dichotomy

Let us turn from these fictional philosophical positions and consider some of the actual ones to which they roughly correspond. Taking the last one first, it is interesting to note the following passage from Bergmann: "But it is only fair to point out that if this . . . methodological and terminological analysis [for the thesis that there are no atoms] . . . is strictly adhered to, even stars and microscopic objects are not physical things in a literal sense, but merely by courtesy of language and pictorial imagination. This might seem awkward. But when I look through a microscope, all I see is a patch of color which creeps through the field like a shadow over a wall. And a shadow, though real, is certainly not a physical thing."[4]

I should like to point out that it is also the case that if this analysis is strictly adhered to, we cannot observe physical things through opera glasses, or even through ordinary spectacles, and one begins to wonder about the status of what we see through an ordinary windowpane. And what about distortions due to temperature gradients—however small and, thus, always present—in the ambient air? It really does "seem awkward" to say that when people who wear glasses describe what they see they are talking about shadows, while those who employ unaided vision talk about physical things—or that when we look through a windowpane, we can only *infer* that it is raining, while if we raise the window, we may "observe directly" that it is. The point I am making is that there is, in principle, a continuous series beginning with looking through a vacuum and containing these as members: looking through a windowpane, looking through glasses, looking through binoculars, looking through a low-power microscope, looking through a high-power microscope, etc., in the order given. The important consequence is that, so far, we are left without criteria which would enable us to draw a nonarbitrary line between "observation" and "theory." Certainly, we will often find it convenient to draw such a to-some-extent-arbitrary line; but its position will vary widely from context to context. (For example, if we are determining the resolving characteristics of a certain microscope, we would certainly draw the line beyond ordinary spectacles, probably

[4] G. Bergmann, "Outline of an Empiricist Philosophy of Physics," *American Journal of Physics*, 11:248–258; 335–342 (1943), reprinted in *Readings in the Philosophy of Science*, H. Feigl and M. Brodbeck, eds. (New York: Appleton-Century-Crofts, 1953), pp. 262–287.

beyond simple magnifying glasses, and possibly beyond another microscope with a lower power of resolution.) But what ontological ice does a mere methodologically convenient observational-theoretical dichotomy cut? Does an entity attain physical thinghood and/or "real existence" in one context only to lose it in another? Or, we may ask, recalling the continuity from observable to unobservable, is what is seen through spectacles a "little bit less real" or does it "exist to a slightly less extent" than what is observed by unaided vision?[5]

However, it might be argued that things seen through spectacles and binoculars look like ordinary physical objects, while those seen through microscopes and telescopes look like shadows and patches of light. I can only reply that this does not seem to me to be the case, particularly when looking at the moon, or even Saturn, through a telescope or when looking at a small, though "directly observable," physical object through a low-power microscope. Thus, again, a continuity appears.

"But," it might be objected, "theory tells us that what we see by means of a microscope is a real image, which is certainly distinct from the object on the stage." Now first of all, it should be remarked that it seems odd that one who is espousing an austere empiricism which requires a sharp observational-language/theoretical-language distinction (and one in which the former language has a privileged status) should need a theory in order to tell him what is observable. But, letting this pass, what is to prevent us from saying that we still observe the object on the stage, even though a "real image" may be involved? Otherwise, we shall be strongly tempted by phenomenalistic demons, and at this point we are considering a physical-object observation language rather than a sense-datum one. (Compare the traditional puzzles: Do I see one physical object or two when I punch my eyeball? Does one object split into two? Or do I see one object and one image? Etc.)

Another argument for the continuous transition from the observable to the unobservable (theoretical) may be adduced from theoretical considerations themselves. For example, contemporary valency theory tells us that there is a virtually continuous transition from very small molecules (such as those of hydrogen) through "medium-sized" ones (such as those of the fatty acids, polypeptides, proteins, and viruses) to extremely large ones (such as crystals of the salts, diamonds, and lumps of polymeric plastic). The molecules in the last-mentioned group are macro, "directly observable" physical objects but are, nevertheless, genuine, single molecules; on the other hand, those in the first mentioned group have the same perplexing properties as subatomic particles (de Broglie waves, Heisenberg indeterminacy, etc.). Are we to say that a large protein molecule (e.g., a virus) which can be "seen" only with an electron microscope is a little less real or exists to somewhat less an extent than does a molecule of a polymer which can be seen with an optical microscope? And does a hydrogen molecule partake of only an infinitesimal portion of existence or reality? Although there certainly *is* a continuous transition from observability to unobservability, any talk of such a continuity from full-blown existence to nonexistence is, clearly, nonsense.

[5] I am not attributing to Professor Bergmann the absurd views suggested by these questions. He seems to take a sense-datum language as his observation language (the base of what he called "the empirical hierarchy"), and, in some ways, such a position is more difficult to refute than one which purports to take an "observable-physical-object" view. However, I believe that demolishing the straw men with which I am now dealing amounts to desirable preliminary "therapy." Some nonrealist interpretations of theories which embody the presupposition that the observable-theoretical distinction is sharp and ontologically crucial seem to me to entail positions which correspond to such straw men rather closely.





Let us now consider the next to last modified position which was adopted by our fictional philosophers. According to them, it is only those entities which are *in principle* impossible to observe that present special problems. What kind of impossibility is meant here? Without going into a detailed discussion of the various types of impossibility, about which there is abundant literature with which the reader is no doubt familiar, I shall assume what usually seems to be granted by most philosophers who talk of entities which are unobservable in principle—i.e., that the theory(s) itself (coupled with a physiological theory of perception, I would add) entails that such entities are unobservable.

We should immediately note that if this analysis of the notion of unobservability (and, hence, of observability) is accepted, then its use as a means of delimiting the observation language seems to be precluded for those philosophers who regard theoretical expressions as elements of a calculating device—as meaningless strings of symbols. For suppose they wished to determine whether or not 'electron' was a theoretical term. First, they must see whether the theory entails the sentence 'Electrons are unobservable.' So far, so good, for their calculating devices are said to be able to select genuine sentences, provided they contain no theoretical terms. But what about the selected "sentence" itself? Suppose

that 'electron' is an observation term. It follows that the expression is a genuine sentence and asserts that electrons are unobservable. But this entails that 'electron' is *not* an observation term. Thus if 'electron' is an observation term, then it is *not* an observation term. Therefore it is not an observation term. But then it follows that 'Electrons are unobservable' is not a genuine sentence and does not assert that electrons are unobservable, since it is a meaningless string of marks and does not assert anything whatever. Of course, it could be stipulated that when a theory "selects" a meaningless expression of the form 'Xs are unobservable,' then 'X' is to be taken as a theoretical term. But this seems rather arbitrary.

But, assuming that well-formed theoretical expressions are genuine sentences, what shall we say about unobservability in principle? I shall begin by putting my head on the block and argue that the present-day status of, say, electrons is in many ways similar to that of Jones' crobes before microscopes were invented. I am well aware of the numerous theoretical arguments for the impossibility of observing electrons. But suppose new entities are discovered which interact with electrons in such a mild manner that if an electron is, say, in an eigenstate of position, then, in certain circumstances, the interaction does not disturb it. Suppose also that a drug is discovered which vastly alters the human perceptual apparatus—perhaps even activates latent capacities so that a new sense modality emerges. Finally, suppose that in our altered state we are able to perceive (not necessarily visually) by means of these new entities in a manner roughly analogous to that by which we now see by means of photons. To make this a little more plausible, suppose that the energy eigenstates of the electrons in some of the compounds present in the relevant perceptual organ are such that even the weak interaction with the new entities alters them and also that the cross sections, relative to the new entities, of the electrons and other particles of the gases of the air are so small that the chance of any interaction here is negligible. Then we might be able to "observe directly" the position and possibly the approximate diameter and other properties of some electrons. It would follow, of course, that quantum theory would have to be altered in some respects, since the new entities do not conform to all its principles. But however improbable this may be, it does not, I maintain, involve any logical or conceptual absurdity. Furthermore, the





modification necessary for the inclusion of the new entities would not necessarily change the meaning of the term 'electron.'[6]

Consider a somewhat less fantastic example, and one which does not involve any change in physical theory. Suppose a human mutant is born who is able to "observe" ultraviolet radiation, or even X rays, in the same way we "observe" visible light.

Now I think that it is extremely improbable that we will ever observe electrons directly (i.e., that it will ever be reasonable to assert that we have so observed them). But this is neither here nor there; it is not the purpose of this essay to predict the future development of scientific theories, and, hence, it is not its business to decide what actually is observable or what will become observable (in the more or less intuitive sense of 'observable' with which we are now working). After all, we are operating, here, under the assumption that it is theory, and thus science itself, which tells us what is or is not, in this sense, observable (the 'in principle' seems to have become superfluous). And this is the heart of the matter; for it follows that, at least for this sense of 'observable,' there are no a priori or philosophical criteria for separating the observable from the unobservable. By trying to show that we can talk about the possi*bility* of observing electrons without committing logical or conceptual blunders, I have been trying to support the thesis that any (nonlogical) term is a *possible* candidate for an observation term.

There is another line which may be taken in regard to delimitation of the observation language. According to it, the proper term with which to work is not 'observ*able*' but, rather 'observ*ed*.' There immediately comes to mind the tradition beginning with Locke and Hume (No idea without a preceding impression!), running through Logical Atomism and the Principle of Acquaintance, and ending (perhaps) in contemporary positivism. Since the numerous facets of this tradition have been extensively examined and criticized in the literature, I shall limit myself here to a few summary remarks.

Again, let us consider at this point only observation languages which contain ordinary physical-object terms (along with observation predicates, etc., of course). Now, according to this view, all descriptive terms of the observation language must refer to that which has been observed.

[6] For arguments that it is possible to alter a theory without altering the meanings of its terms, see my "Meaning Postulates in Scientific Theories," in *Current Issues in the Philosophy of Science*, Feigl and Maxwell, eds.

How is this to be interpreted? Not too narrowly, presumably, otherwise each language user would have a different observation language. The name of my Aunt Mamie, of California, whom I have never seen, would not be in my observation language, nor would 'snow' be an observation term for many Floridians. One could, of course, set off the observation language by means of this awkward restriction, but then, obviously, not being the referent of an observation term would have no bearing on the ontological status of Aunt Mamie or that of snow.

Perhaps it is intended that the referents of observation terms must be members of a *kind* some of whose members have been observed or instances of a property some of whose instances have been observed. But there are familiar difficulties here. For example, given any entity, we can always find a kind whose only member is the entity in question; and surely expressions such as 'men over 14 feet tall' should be counted as observational even though no instances of the "property" of being a man over 14 feet tall have been observed. It would seem that this approach must soon fall back upon some notion of simples or determinables vs. determinates. But is it thereby saved? If it is held that only those terms which refer to observed simples or observed determinates are observation terms, we need only remind ourselves of such instances as Hume's notorious missing shade of blue. And if it is contended that in order to be an observation term an expression must at least refer to an observed determinable, then we can always find such a determinable which is broad enough in scope to embrace any entity whatever. But even if these difficulties can be circumvented, we see (as we knew all along) that this approach leads inevitably into phenomenalism, which is a view with which we have not been concerning ourselves.

Now it is not the purpose of this essay to give a detailed critique of phenomenalism. For the most part, I simply assume that it is untenable, at least in any of its translatability varieties.[7] However, if there are any unreconstructed phenomenalists among the readers, my purpose, insofar as they are concerned, will have been largely achieved if they will grant what I suppose most of them would stoutly maintain anyway, i.e., that theoretical entities are no worse off than so-called observable physical objects.

[7] The reader is no doubt familiar with the abundant literature concerned with this issue. See, for example, Sellars' "Empiricism and the Philosophy of Mind," which also contains references to other pertinent works.





Nevertheless, a few considerations concerning phenomenalism and related matters may cast some light upon the observational-theoretical dichotomy and, perhaps, upon the nature of the "observation language." As a preface, allow me some overdue remarks on the latter. Although I have contended that the line between the observable and the unobservable is diffuse, that it shifts from one scientific problem to another, and that it is constantly being pushed toward the "unobservable" end of the spectrum as we develop better means of observation—better instruments—it would, nevertheless, be fatuous to minimize the importance of the observation base, for it is absolutely necessary as a confirmation base for statements which do refer to entities which are unobservable at a given time. But we should take as its basis and its unit not the "observational term" but, rather, the quickly decidable sentence. (I am indebted to Feyerabend, *loc. cit.*, for this terminology.) A quickly decidable sentence (in the technical sense employed here) may be defined as a singular, nonanalytic sentence such that a reliable, reasonably sophisticated language user can very quickly decide[8] whether to assert it or deny it when he is reporting on an occurrent situation. 'Observation term' may now be defined as a 'descriptive (nonlogical) term which may occur in a quickly decidable sentence,' and 'observation sentence' as a 'sentence whose only descriptive terms are observation terms.'

Returning to phenomenalism, let me emphasize that I am not among those philosophers who hold that there are no such things as sense contents (even sense data), nor do I believe that they play no important role in our perception of "reality." But the fact remains that the referents of most (not all) of the statements of the linguistic framework used in everyday life and in science are *not* sense contents but, rather, physical objects and other publicly observable entities. Except for pains, odors, "inner states," etc., we *do not usually observe sense contents*; and although there is good reason to believe that they play an indispensable role in observation, we are usually not aware of them when we visually or tactilely) *observe physical objects*. For example, when I observe a distorted, obliquely reflected image in a mirror, I may seem to be seeing a baby elephant standing on its head; later I discover it is an image of Uncle Charles taking a nap with his mouth open and his hand in a peculiar position. Or, passing my neighbor's home at a high rate of

[8] We may say "noninferentially" decide, provided this is interpreted liberally enough to avoid starting the entire controversy about observability all over again.

speed, I observe that he is washing a car. If asked to report these observations I could quickly and easily report a baby elephant and a washing of a car; I probably would not, without subsequent observations, be able to report what colors, shapes, etc. (i.e., what sense data) were involved.

Two questions naturally arise at this point. How is it that we can (sometimes) quickly decide the truth or falsity of a pertinent observation sentence? and, What role do sense contents play in the appropriate tokening of such sentences? The heart of the matter is that these are primarily scientific-theoretical questions rather than "purely logical," "purely conceptual," or "purely epistemological." If theoretical physics, psychology, neurophysiology, etc., were sufficiently advanced, we could give satisfactory answers to these questions, using, in all likelihood, the physical-thing language as our observation language and *treating sensations, sense contents, sense data, and "inner states" as theoretical* (yes, theoretical!) *entities.*[9]

It is interesting and important to note that, even before we give completely satisfactory answers to the two questions considered above, we can, with due effort and reflection, train ourselves to "observe directly" what were once theoretical entities—the sense contents (color sensations, etc.)—involved in our perception of physical things. As has been pointed out before, we can also come to observe other kinds of entities which were once theoretical. Those which most readily come to mind involve the use of instruments as aids to observation. Indeed, using our painfully acquired theoretical knowledge of the world, we come to see that we "directly observe" many kinds of so-called theoretical things. After listening to a dull speech while sitting on a hard bench, we begin to become poignantly aware of the presence of a considerably strong gravitational field, and as Professor Feyerabend is fond of pointing out, if we were carrying a heavy suitcase in a changing gravitational field, we could observe the changes of the $G_{\mu\nu}$ of the metric tensor.

I conclude that our drawing of the observational-theoretical line at any given point is an accident and a function of our physiological make-

[9] Cf. Sellars, "Empiricism and the Philosophy of Mind." As Professor Sellars points out, this is the crux of the "other-minds" problem. Sensations and inner states (relative to an intersubjective observation language, I would add) are theoretical entities (and they "really exist") and *not* merely actual and/or possible behavior. Surely it is the unwillingness to countenance theoretical entities—the hope that every sentence is translatable not only into some observation language but into the physical-thing language—which is responsible for the "logical behaviorism" of the neo-Wittgensteinians.





up, our current state of knowledge, and the instruments we happen to have available and, therefore, that it has no ontological significance whatever.

## What If We COULD Eliminate Theoretical Terms?

Among the candidates for methods of eliminating theoretical terms, three have received the lion's share of current attention: explicit definability, the Ramsey sentence,[10] and implications of Craig's theorem.[11] Today there is almost (not quite) universal agreement that not all theoretical terms can be eliminated by explicitly defining them in terms of observation terms. It seems to have been overlooked that even if this could be accomplished it would not necessarily avoid reference to unobservable (theoretical) entities. One example should make this evident. Within the elementary kinetic theory of gases we could define 'molecules' as 'particles of matter (or stuff), not large enough to be seen even with a microscope, which are in rapid motion, frequently colliding with each other, and are the constituents of all gases.' All the (nonlogical) terms in the definiens are observation terms, and still the definition itself, as well as kinetic theory (and other theoretical considerations), implies that molecules of gases are unobservable (at least for the present).

It seems to me that a large number—certainly not all, however; for example, 'photon,' 'electromagnetic field,' '$\psi$-function'—of theoretical terms could be explicitly defined wholly in terms of observation terms, but this would in no way avoid a reference to unobservable entities. This important fact seems to have been quite generally overlooked. It is an important oversight because philosophers today are devoting so much attention to the meaning of theoretical terms (a crucially important problem, to be sure), while the ontological stomach-aches (ultimately unjustifiable, of course) concerning theories seem to have arisen from the fact that the *entities* rather than the *terms* were nonobservational. Implicit, of course, is the mistaken assumption that terms referring to unobservable entities cannot be among those which occur in the observation language (and also, perhaps, the assumption that the referent of a defined term always consists of a mere "bundle" of the entities which are referents of the terms of the definiens).

[10] Frank P. Ramsey, *The Foundations of Mathematics* (New York: Humanities, 1931).

[11] William Craig, "Replacement of Auxiliary Expressions," *Philosophical Review*, 65:38–55 (1956).

Surprisingly enough, both the Ramsey sentence and Craig's theorem provide us with genuine (in principle) methods for eliminating theoretical terms provided we are interested only in the deductive "observational" consequences of an axiomatized theory. That neither can provide a viable method for avoiding reference to theoretical entities has been pointed out clearly by both Hempel and Nagel.[12] I shall discuss these two devices only briefly.[13]

The first step in forming the Ramsey sentence of a theory is to take the conjunction of the axioms of the theory and conjoin it with the so-called correspondence rules (sentences containing both theoretical and observational terms—the "links" between the "purely theoretical" and the observational). This conjunction can be represented as follows:

$$---P---Q---\ldots$$

where the dashes represent the sentential matrixes (the axioms and C-rules) containing the theoretical terms (which are, of course, almost always predicates or class terms) 'P,' 'Q,' '. . .'; the theoretical terms are then "eliminated" by replacing them with existentially quantified variables. The resulting "Ramsey sentence" is represented, then, by

$$(\exists f)(\exists g)\ \ldots\ (---f---g---\ldots).$$

Or, consider an informal illustration. Let us represent schematically an oversimplified axiomatization of kinetic theory by

> All gases are composed entirely of molecules. The molecules are in rapid motion and are in frequent collision, etc., etc.

And for simplicity's sake, suppose that 'molecules' is the only theoretical term. The Ramsey sentence would be something like the following:

> There is a kind of entity such that all gases are composed entirely of these entities. They are in rapid motion and are in frequent collision, etc., etc.

Now it is a simple matter to demonstrate that any sentence containing only observation (and logical) terms which is a deductive consequence of the original theory is also a deductive consequence of its Ramsey sentence (see, for example, Rozeboom's article in this volume); thus, as far as any deductive systemization is concerned, any theory may be

[12] Carl G. Hempel, "The Theoretician's Dilemma," in *Minnesota Studies in Philosophy of Science*, Vol. II, Feigl, Scriven, and Maxwell, eds. Nagel, *loc. cit.*

[13] For an extended consideration of the Ramsey sentence see Professor William Rozeboom's essay in this volume.





eliminated and its Ramsey sentence used instead. However, it is also easy to prove (if indeed it is not obvious) that if a given theory (or a theory together with other considerations, theoretical or observational) entails that there exist certain kinds of unobservable entities, then the appropriate Ramsey sentence will also entail that there exist the same number of kinds of *unobservable* entities.[14] Although, insofar as deductive systemization is concerned, the Ramsey sentence can avoid the use of theoretical terms; it cannot, even in letter, much less in spirit (Hempel, *loc. cit.*, was too charitable), eliminate reference to unobservable (theoretical) entities.

The Craig result, like the Ramsey sentence, provides a "method" of reaxiomatizing a postulate set so that any arbitrarily selected class of terms may be eliminated, provided one is interested only in those theorems which contain none of these terms. Its "advantages" over the Ramsey sentence are that it does not quantify over predicates and class terms and that its final reaxiomatization eliminates reference both in spirit and in letter to unobservable entities. However, its shortcomings (for the purposes at hand) render it useless as an instrument of actual scientific practice and also preclude its having, even in principle, any implications for ontology. The resulting number of axioms will, in general, and particularly in the case of the empirical sciences, be infinite in number and practicably unmanageable.

But if the practical objections to the use of Craig's method as a means for elimination of theoretical terms are all but insurmountable, there are objections of principle which are even more formidable. Both Craig's method and the Ramsey device must operate upon theories (containing, of course, theoretical terms) which are "already there." They eliminate theoretical terms only after these terms have already been used in inter-

[14] The proof may be sketched as follows: Let 'T' designate the theory (conjoined, if necessary, with other statements in the accepted body of knowledge) which entails that the kinds of entities C, D, . . . are not observable, i.e., T entails that

$$(\exists x)(\exists y)\ \ldots\ (Cx \cdot Dy \ldots x \text{ is not observable} \cdot y \text{ is not observable} \ldots)$$

which in turn entails

$$(\exists f)(\exists g)\ \ldots\ (\exists x)(\exists y)\ \ldots\ (fx \cdot gy \ldots x \text{ is not observable} \cdot y \text{ is not observable} \ldots).$$

Now the Ramsey result holds for any arbitrary division of nonlogical terms into two classes, so we may put 'observable' into the class with the observation terms, so that the latter formalized statement may be treated as an "observational" consequence of T (transitivity of entailment). But then it is also a consequence of the Ramsey sentence of T. Q.E.D.

mediary steps. Neither provides a method for axiomatization *ab initio* or a recipe or guide for invention of new theories. Consequently neither provides a method for the elimination of theoretical terms in the all-important "context of discovery."[15] It might be argued that this objection is not so telling, after all, for we also lack any recipe for the invention of theories themselves, and it is logically possible that we should discover, without the use of theories as intermediaries, Ramsey sentences or Craig end products which are just as useful for explaining and predicting observations as the theories which we happen to have (accidently) adduced. It might be added that it is also logically possible that we should discover just those observation statements (including predictions, etc.) which happen to be true without the use of any instrumental intermediaries.

We must reply that the accomplished fact that it *is* theories, referring to unobservables, which have been invented for this purpose and that many of them serve it so admirably—this fact, itself, cries out for explanation. To say that theories are *designed* to accomplish this task is no reply unless at least a schema of an instrumentalist recipe for such designing is provided. As far as I know this has not been done. The thesis that theoretical entities are "really" just "bundles" of observable objects or of sense data would, if true, provide an explanation; but it is not taken very seriously by most philosophers today—for the very good reason that it seems to be false. The only reasonable explanation for the success of theories of which I am aware is that well-confirmed theories are conjunctions of well-confirmed, genuine statements and that the entities to which they refer, in all probability, exist. That it is psychologically possible for us to invent such theories is explained by the fact that many of the entities to which they refer resemble in many respects (although

[15] The Ramsey sentence is intuitively tractable enough so that very simple "theories" might be invented as full-blown Ramsey sentences without the use of intermediary terms. However, Craig's theorem provides *no* means of operating *ab initio*. Craig points out (*loc. cit.*) that once the original theory is "there," reference, in letter, to theoretical entities in the application of his method may be avoided by using the names of theoretical terms rather than using the terms themselves (i.e. by mentioning theoretical terms rather than using them). But surely only a diehard instrumentalist can take more than very scant comfort from this. The question would still remain: Where did the theory come from in the first place, and why are the names of these particular terms arranged in this particular manner such admirable "instruments" for explanation and prediction of observations? Whatever ontological implications this modification of the Craig method may have, they seem to be exactly the same as those of instrumentalism proper.





they may differ radically from them in others) the entities which we have already observed.

It should also be remembered, at this point, that theories, even as instruments, are important not only for deductive systemization but also for inductive systemization (see Hempel, *loc. cit.*). We often reason theoretically using induction, and the conclusions may be either observational or theoretical. Thus we might infer from the facts that a certain substance was paramagnetic, that it catalyzed the recombination of free radicals, and that it *probably* contained a "one-electron" bond; and we might go on to infer, again inductively, that it would probably catalyze the conversion of orthohydrogen to parahydrogen. The Craig result applies only to deductive systemization and, thus, cannot, even in its Pickwickian fashion, eliminate theoretical terms where inductive theoretical reasoning is involved. Although Craig's theorem is of great interest for formal logic, we must conclude, to use Craig's (*loc. cit.*) own words, "[as far as] the meaning [and, I would add, the referents] of such expressions [auxiliary expressions (theoretical terms)] . . . [is concerned] the method . . . fails to provide any . . . clarification."

We have seen that the elimination of theoretical *terms*, even by explicit definition, would not necessarily eliminate reference to theoretical (unobservable) *entities*. We have also seen that, even if reference to theoretical entities could be eliminated after the theories themselves have been used in such an elimination (for example, by a device such as Craig's), the reality (existence) of the theoretical entities is not thereby militated against. But the most crucial point follows. Even if we do come up with a gimmick—a prediction machine or "black box"—into which we can feed data and grind out all the completely veridical observational predictions which we may desire, the possibility—I should say the likelihood—of the existence of unobserved causes for the observed events would still remain. For unless an *explanation* of why any prediction machine or "calculating device" in terms of the established rules of explanation, confirmation, etc., were forthcoming, the task of science would still be incomplete.

This brings us to another mistaken assumption that has been responsible for much mischief in considerations concerning the cognitive status of theories—the assumption that science is concerned solely with the "fruitful" organization of observational data or, more specifically, with successful prediction. Surely the main concerns of, say, a theoretical

physicist involve such things as the actual properties and varieties of subatomic particles rather than the mere predictions about where and how intense a certain spectral line will be. The instrumentalist has the picture entirely reversed; as far as pure science is concerned, most observational data—most predictions—are mere instruments and are of value only for their roles in confirming theoretical principles. Even if we obtain the prediction machine, many of the theories extant today are well confirmed enough to argue strongly for the reality of theoretical entities. And they are much more intellectually satisfactory, for they provide an explanation of the occurrence of the observational events which they predict. And—equally important—an explanation for the fact that theories "work" as well as they do is, as already noted, also forthcoming; it is simply that the entities to which they refer exist.

## "Criteria" of Reality and Instrumentalism

It was pointed out in the beginning of this article that Professor Ernest Nagel considers the dispute between realists and instrumentalists to be merely a verbal one.[16] There follows here a brief and what I hope is a not too inaccurate summary of his argument. Various criteria of 'real' or 'exist' (runs the argument) are employed by scientists, philosophers, etc., in their considerations of the "reality problem." (Among these criteria—some of them competing, some compatible with each other—are public perceivability, being mentioned in a generally accepted law, being mentioned in more than one law, being mentioned in a "causal" law, and being invariant "under some stipulated set of transformation, projections, or perspectives."[17]) Since, then (it continues) any two disputants will, in all probability, be using 'real' or 'exist' in two different senses, such disputes are merely verbal. Now someone might anticipate the forthcoming objections to this argument by pointing out that the word 'criteria' is a troublesome one and that perhaps, for Nagel, the connection between criteria and reality or existence is a contingent one rather than one based on meaning. But a moment's reflection makes it obvious that for Nagel's argument to have force, 'criteria' must be taken in the latter sense; and, indeed, Nagel explicitly speaks for the connection between criteria and the "senses [sic!] of 'real' or 'exist.' "[18]

[16] *Op. cit.*, pp. 141–152.
[17] Nagel, *op. cit.*, pp. 145–150.
[18] *Op. cit.*, p. 151.





Before proceeding to a criticism of these arguments, let me point out that Professor Gustav Bergmann, completely independently, treats ontological questions in a similar manner. Rather than criteria, he speaks of "patterns," although he does say that he "could instead have spoken of criteria," and he makes explicit reference to various "uses" of 'exist.'[19]

There are two main points that I wish to make regarding this kind of approach to ontological issues. First, it seems to me that it commits the old mistake of confusing meaning with evidence. To be sure, the fact that a kind of entity is mentioned in well-confirmed laws or that such entities are publicly perceptible, etc.—such facts are evidence (very good evidence!) for the existence or "reality" of the entities in question. But I cannot see how a prima-facie—or any other kind of—case can be made for taking such conditions as *defining* characteristics of existence.

The second point is even more serious. One would hope that (Professor Norman Malcolm notwithstanding) over nine hundred years of debate and analysis have made it clear that existence is *not* a property. Now surely the characteristics of being mentioned in well-confirmed laws, being publicly perceptible, etc., are properties of sorts; and if these comprised part of the meaning of 'exists,' then 'existence' would be a predicate (and existence a property).

Thus, it is seen that the issue between instrumentalism and realism can be made into a merely verbal one *only* by twisting the meanings of 'existence' and 'reality,' not only beyond their "ordinary" meaning but, also, far beyond any reasonable meanings which these terms might be given. In fact, it seems not too much to say that such an interpretation of the "reality problem" commits a fallacy closely akin to that of the Ontological Argument.

What can be said about the meanings of 'real' and 'exists'? I submit that in "ordinary language," the most usual uses of these terms are such that

$$\Phi_s \text{ are real} =_{df} \Phi_s \text{ exist}$$

and that

$$\Phi_s \text{ exist} =_{df} \text{there are } \Phi_s$$

and that the meanings of these definiens are clear enough so that no further explication is seriously needed. (In most "constructed languages," 'There are $\Phi_s$' would, of course, be expressed by '$(\exists x)(\Phi x)$.') Thus, if

[19] "Physics and Ontology," *Philosophy of Science*, 28:1–14 (1961).

we have a well-confirmed set of statements (laws or theories plus initial conditions) which entail the statement 'There are $\Phi_s$' (or '$(\exists x)(\Phi x)$'), then it is well confirmed that $\Phi_s$ are real—full stop!

In summary, let us recall three points concerning instrumentalism. First, as is shown above, it cannot be excused on the grounds that it differs from realism only in terminology. Second, it cannot provide an explanation as to why its "calculating devices" (theories) are so successful. Realism provides the very simple and cogent explanation that the entities referred to by well-confirmed theories exist. Third, it must be acutely embarrassing to instrumentalists when what was once a "purely" theoretical entity becomes, due to better instruments, etc., an observable one.[20]

## The Ontological Status of Entities—Theoretical and Otherwise

As I have stated elsewhere (see the second reference in footnote 22), the key to the solution of all significant problems in ontology can be found in Carnap's classic article, "Empiricism, Semantics, and Ontology."[21] Taking this essay as our point of departure, we may say that in order to speak at all about any kind of entities whatever and thus, a fortiori, to consider their existence or nonexistence, one must first accept the "linguistic framework" which "introduces the entities."[22] This simply means that in order to understand considerations concerning the existence of any kind of entities one must understand the meanings of the linguistic expressions (sentences and terms) referring to them—and that such expressions have no meaning unless they are given a place in a linguistic framework which "talks about the world" and which has at least a minimum of comprehensiveness. (Since I am interested, here, primarily in empirical science, I neglect universes of discourse containing only "purely mathematical" or "purely logical" entities.)

Although wide latitude in choosing and constructing frameworks is permissible, any satisfactory framework will embody, at the very least,

[20] Although I cannot agree with all the conclusions of Professor Feyerabend's essay in this volume, the reader is referred to it for an interesting critique of instrumentalism.

[21] R. Carnap, *Meaning and Necessity*, 2nd ed. (Chicago: University of Chicago Press, 1959).

[22] For a more detailed discussion of linguistic frameworks as well as their relevance for ontological problems, see Carnap, *ibid.*; and G. Maxwell, "Theories, Frameworks, and Ontology," *Philosophy of Science*, vol. 28 (1961). For an elaboration of the linguistic theses presupposed by the latter article and, to some extent, by this essay,





the following features: (1) the usual L(ogical)-formation and L-transformation rules and the corresponding set of L-true sentences which they generate; (2) a set of confirmation rules, whose nature I shall not discuss here but which I shall assume are quite similar to those actually used in the sciences; (3) a set of sentences whose truth value is quickly decidable on other than purely linguistic grounds—these correspond to "singular observation statements," but, of course, as we have seen, it is neither necessary nor desirable that such statements be incorrigible or indubitable or that a sharp distinction between observation and theory be drawn; and (4) a set of lawlike sentences, which, among other things, provide that component of meaning which is nonostensive for every descriptive (nonlogical) term of the framework. (I have argued in the references given in footnote 22 that every descriptive term has a meaning component which is nonostensive.[23] Even a term such as 'red' has part of its meaning provided by, for example, the lawlike sentence 'No surface can be both red and green all over at the same time.' Such a view is sometimes stigmatized by the epithet 'holism.' But if there is any holism involved in the view I am advocating, it is completely conceptual or epistemological and not ontological. Just what relations are present, or absent, between the actual entities of the "real world" is an empirical question and must be decided by considerations *within* a descriptive linguistic framework rather than by consideration *about* such frameworks.)

At this point, two views may be mentioned. I will omit consideration of explicitly defined terms, since they are, in principle, always eliminable. According to one view, it is always a proper subset of the lawlike sentence containing a given term which contributes to the term's meaning. The sentences in this subset are A-true[24] (analytic in a broad sense) and are totally devoid of any factual content—their only function is to provide part of the meaning of the term in question. The situation is immensely complicated by the fact that when actual usage is considered,

see G. Maxwell and H. Feigl, "Why Ordinary Language Needs Reforming," *Journal of Philosophy*, 58:488–498 (1961); G. Maxwell, "Meaning Postulates in Scientific Theories," in *Current Issues in the Philosophy of Science*, Feigl and Maxwell, eds.; and my brief article, "The Necessary and the Contingent," in this volume.

[23] Cf. also the writings of Wilfrid Sellars, for example in "Some Reflections on Language Games," *Philosophy of Science*, 21:204–228 (1954).

[24] See R. Carnap, "Beobachtungsprache und theoretisch Sprache," *Dialectica*, 12:236–248 (1957); as well as the references in fn. 22.

a sentence which is A-true in one context may be contingent in another and that even in a given context it is, more often than not, not clear, unless the context is a rational reformation, whether a given sentence is being used as A-true or as contingent. This confusion can be avoided by engaging in rational reformation, i.e., by stipulating (subject to certain broad and very liberal limitations) which sentences are to be taken as A-true and which as contingent. Needless to say, this is the viewpoint which I prefer.

The complication just mentioned, however, has led many philosophers, including Professor Putnam[25]—to say nothing of W. V. Quine—to the other viewpoint. According to it, no segregation of the relevant lawlike sentences into A-true and contingent should be attempted; each lawlike sentence plays a dual role: (1) it contributes to the meanings of its descriptive terms and (2) it provides empirical information. Fortunately, we do not have to choose between these two viewpoints here, for the thesis of realism which I am advocating is (almost) equally well accommodated by either one.

Now when we engage in any considerations about any kinds of entities and, a fortiori, considerations about the existence of theoretical entities, it is to the lawlike sentences mentioning the entities—for theoretical entities, the theoretical postulates and the so-called correspondence rules—to which we turn. These sentences tell us, for example, how theoretical entities of a given kind resemble, on the one hand, and differ from, on the other, the entities with which we happen to be more familiar. And the fact that many theoretical entities, for example those of quantum theory, differ a great deal from our ordinary everyday physical objects is no reason whatever to ascribe a questionable ontological status to them or to contend that they are merely "calculating devices." After all, the very air we breathe as well as such things as shadows and mirror images are entities of quite different kinds from chairs and tables, but this provides no grounds for impugning their ontological status. The fact that molecules, atoms, etc., cannot be said in any non-Pickwickian sense to have a color has given some philosophers ontological qualms. But, of course, the air has no color (unless we invoke the color of the sky); and a transparent object whose refractive index was the same as that of air would be completely invisible, although it would have all

[25] See his essay in this volume.





the other properties of ordinary physical objects. Molecules, for example, are in about the same category; they are physical things which possess some but not all of the properties of everyday physical things.

A: Do molecules exist?

B: Certainly. We have an extremely well-confirmed theory, which when conjoined with other true sentences such as 'There are gases' entails that there are molecules.

A: But are they real?

B: What do you mean?

A: Well, I'm not sure. As a starter: Are they physical objects?

B: Certainly the large ones are. Take, for example, that diamond in your ring. As for those which are submicroscopic but still large enough to have large quantum numbers, it seems that in almost any reasonable reformation they would be classified as physical objects. It would seem unjustifiable to withhold from them this status simply because they cannot be said to have a color in any straightforward fashion. In fact, I would even be inclined to call the smallest, the molecule of hydrogen, a physical object. It has mass, a reasonably determinate diameter, and, usually, something which approximates simple location, etc.

A: How about electrons?

B: The decision here is more difficult. We might find it necessary to try several reformations, taking into account many facets of contemporary physical theory, before we arrived at the most satisfactory one. It would also be helpful to have a more specific problem in view than the one which we are now considering. At any rate, we might begin by pointing out that electrons do have mass, even rest mass. They can be simply located at the expense of refraining from ascribing to them a determinate momentum. They can be said to causally interact with "bona fide" physical objects, even by those who have a billiard-ball notion of causality. The important point is that the question 'Are electrons physical objects?' is a request for a rational reformation of a very thoroughgoing variety. For most purposes, a rational reformation would not need to answer it. For your purposes, why not be content to learn in what ways electrons are similar to, and in what ways they differ from, what you would call "ordinary physical objects"? This will enable you to avoid conceptual blunders.

A: Perhaps you are right. However, I am genuinely puzzled about fields, and even photons.

B: Take the last first. We would probably never call them physical objects. For example, they have no rest mass and it would be a conceptual mistake to ask, except in a Pickwickian sense, What is their color? However, it would be reasonable to say that they are a sort of physical continuant; and they can even interact with electrons in a billiard-ball manner. At any rate, we must agree, speaking loosely, that they are "every bit as real" as electrons. The concepts of field theories are so open textured that it is difficult to decide what kinds of reformations one should adopt here. And it is virtually impossible to find similar kinds of entities with which one is prescientifically familiar. Perhaps these theories will someday be enriched until decisions concerning the most appropriate rational reformations are easier to make—perhaps not. But even here, the meanings of the terms involved are usually sufficiently clear to avoid conceptual blunders and ontological anxieties. You might like to consider the "lines of force," which are often spoken of in connection with fields. These are often used as a paradigm of the "convenient fiction" by those who hold such a view of theories.[26] But though convenient, lines of force are *not* fictions. They "really exist." Let me try to make this a little more plausible. Consider the isobars of meteorology, or the isograms which connect points of equal elevation above sea level. Now at this very moment, the 1017 millibar isobar, i.e., the line along which the barometric pressure is 1017 millibars, exists right here in the United States. Its location can even be determined "operationally." And all of this is true whether anyone ever draws, or ever has drawn, a weather map. Since a well-confirmed theory (plus, perhaps, other well-confirmed sentences) entails that there are lines of force, lines of force exist. To be sure, they are very different from everyday physical objects. But as long as we are clear about this, what metaphysical—what ontological—problems remain?

[26] Cf. B. Mayo, "The Existence of Theoretical Entities," *Science News*, 32:7–18 (1954), and "More about Theoretical Entities," *ibid.*, 39:42–55 (1956). For a critique of these articles and for excellent constructive remarks concerning theoretical entities, see J. J. C. Smart, "The Reality of Theoretical Entities," *Australasian Journal of Philosophy*, 34:1–12 (1956).

In connection with convenient fictions, we might consider such entities as ideal gases and bodies uninfluenced by external forces. These actually are fictions. But no theory (or theory plus true sentences) entails that there are such things. To understand their function, we need only recourse to the notion of a limit, often used in mathematics. Roughly speaking, what we actually do when we use theories involving such "fictions" is to assume, for example, that the influence of external forces on the body in question is very, very small, or that the behavior of the gas with which we are concerned is approximately given by 'PV = nRT,' or, in early kinetic theory, that the diameter of a molecule is very, very small compared to the distance between molecules. Note that had van der Waals taken the calculating-device or convenient-fiction view, he probably would not have developed his equation which embodies a correction for the effect due to the finite (greater than zero) diameter of molecules.





One of the exciting aspects of the development of science has been the emergence of reference to strikingly new kinds of entities. This is particularly true in field theories and quantum theory. The great difference between these and the old, familiar categories seems to have caused many philosophers and philosophically inclined scientists to despair of effecting a satisfactory conceptual analysis of these powerful new conceptual tools. The attitude too often has been, "Let us proceed to use those new devices and, if necessary for heuristic reasons, even to behave *as if* they consisted of genuine statements about real entities. But let us remember that, in the last analysis, they are only meaningless calculating devices, or, at best, they talk only of convenient fictions, etc. The only *real* entities are the good old familiar ones which we sense directly everyday." To turn the purpose of a saying of Bertrand Russell's almost completely about-face: such a view has advantages—they are the same as those of theft over honest toil. The compulsion toward metaphysical asepsis which appears to have been the motivation for the espousal of many of these reductionistic philosophies seems, itself, to have arisen from a preoccupation with metaphysical pseudo problems, e.g., the conviction that there are very few ontologically legitimate kinds of entities, perhaps only one.