IX

Physics and Furniture

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THE proper effect on everyday beliefs of accepting physical theory is as hotly debated as ever, with the most extreme views the most fashionable. On the one hand, there is the Oxford view of Ryle [22] and Strawson [27] that the proper effect is zero; on the other, the American "super-realist" view of Feyerabend (e.g., [4] and [5]), Sellars [24], and Maxwell [11] that the proper effect is one of total replacement.[1] In this paper I relate and reject both views and advocate a modest realist view of physical theory. The problem is not only interesting in itself, but also bears closely on such topics as the relation between philosophical and scientific reconstructions of languages, natural and artificial languages, and the autonomy of analytic philosophy (see e.g., Strawson [27], and Carnap [2]). A satisfactory solution seems to me essential even to formulate these further central and under-analyzed problems correctly.

The problem may usefully be approached via an analysis of Eddington's notorious concept of two worlds, the familiar and the scientific:

> There are duplicates of every object about me—two tables, two chairs, two pens. . . . One of them has been familiar to me from earliest years. It is a commonplace object of that environment which I call the world. How shall I describe it? It has extension; it is comparatively permanent; it is coloured; above all it is *substantial*. . . . It is a *thing*. . . . Table No. 2 is my scientific table. It is a more recent acquaintance and I do not feel so familiar with it. It does not belong to the world previously mentioned. . . . There is nothing *substantial* about my second table. It is nearly all empty space . . . even in the minute part which is not empty we must not transfer the old notion of substance. . . . I need not tell you that modern physics has by delicate test and remorseless logic assured me that my second scientific table is the only one which is really there.[2]

The scientific world has changed in many details since 1929, but its relation to the familiar world remains for many people the same both

[1] Numbers in brackets indicate entries in the list of References at the end of the article.

[2] Eddington [3], pp. xi–xiv.

in interest and unclarity. Much of the unclarity has been dispelled by Stebbing [25], but her comments on Eddington's problem are rather too therapeutic. She is quite right to observe that there are not "duplicates of every object" in the familiar world:

> Eddington firmly excludes *colour* from the scientific world, and rightly so. But the *rose* is coloured, the *table* is coloured, the *curtains* are coloured. How, then, can that which is not coloured duplicate the rose, the curtains, the table? . . . a coloured object could be *duplicated* only by something with regard to which it would not be meaningless to say that it was coloured.[3] . . . It is as absurd to say that there is a scientific table as to say that there is a familiar electron or a familiar quantum.[4]

Thus Stebbing resolves Eddington's dilemma by separating the scientific world from the familiar world. An entity in the one may have a "counterpart"[5] in the other, as an electromagnetic wavelength is the counterpart of a colour,[6] but not a duplicate.

But even when Eddington's confusions have been dealt with, his problem remains. The relation "counterpart of" is not much clearer than the relation "duplicate of." We still need to know how

> the symbolic construction at which physics aims is related to the familiar world. There would seem to be three alternatives: (1) the construction is an imitation of the world; (2) the construction is more real than (or truer than?) the familiar world; (3) the construction is for the sake of correlating certain selected elements in the familiar world, in order that the range of our experience may be extended and what is sensibly experienced may be ordered.[7]

Stebbing's description of the alternatives shows clearly enough that she is going to adopt the third, instrumentalist, view of physical theory. Considered merely as instruments for predicting and ordering experience, perhaps theories raise no problems of truth (since they make no statements of fact), and hence no problems of ontology (since they postulate and describe no entities). But this solution is not available to one who would take even the most modest realist view of theories, for whom there still seem to be two worlds. Even if such a realist agrees that Stebbing has shown these worlds to have no entities in common, that hardly makes the problem of accounting for their relation less urgent.

[3] Stebbing [25], p. 60.

[4] *Ibid.*, p. 58.

[5] *Ibid.*, p. 60.

[6] The example is Stebbing's, *ibid.* Although a common example, it is a bad one, since only for monochromatic light does wavelength correlate with color, and very little light is monochromatic. Other light of the same color may contain *no* light of the corresponding monochromatic wavelength.

[7] *Ibid.*, p. 66.

The first step towards a realist solution is to clarify the sense of the term "world" as it is used in stating the problem. There is the sense in which the world of industry is a different world from the academic world, or an isolated geographical region with exotic forms of life is a different world.[8] The sense here is that some kinds of activity typical of industry do not take place in universities (and conversely), and some kinds of animals, say, in Australia are not found elsewhere. Hence terms are needed, to name and describe activities and animals of such kinds that are not needed elsewhere. Ryle gives a similar example:

> We know that a lot of people are interested in poultry and would not be surprised to find in existence a periodical called "The Poultry World." Here the word "world" is not used as theologians use it. It is a collective noun used to label together all matters pertaining to poultry-keeping. It could be paraphrased by "field" or "sphere of influence" or "province."[9]

In all these cases, a new language or extension of a language is needed to describe special classes of phenomena. The worlds then comprise these special classes or the special concepts invoked in their description.

This sense of "two worlds" is *prima facie* distinct from that in which alternative sets of concepts may be invoked to describe the same phenomena, as in field and particle formulations of a physical theory, functional and causal descriptions of organic processes, or individualist and class descriptions of economic behavior. Here, in contrast to the first cases, the different descriptions may conflict, since they are taken to be of the same class of phenomena. The conflict may itself be described in terms of varying apparent strength, ranging from comparisons of simplicity to clashes of ontology. Thus, to take one of the cited examples, one may argue whether it is simpler to speak of, or whether there exist, electric particles exerting forces as opposed to electric fields with singularities, and similarly with the other examples. For the moment, it is convenient to put such conflicts in more neutral terms as being about the "adequacy" of alternative languages to provide descriptions in some empirical domain (i.e., "world" in the first sense), although on the analysis I assume later, this is equivalent to apparently stronger ontological disputes. But however they are put, such conflicts seem to be indepen-

[8] As in the title of Conan Doyle's *The Lost World*, which is a novel partly set in such a region.

[9] Ryle [22], p. 73.

dent of whether either language is also adequate for the description of some other domain.

In fact, the problems raised by these two senses of "two worlds" are not so readily separated, since domains cannot be demarcated independently of languages adequate for describing them. Consider two languages, L_1 apparently adequate to describing the academic structure of Oxbridge, but only of Oxbridge, and L_2, adequate to describing that of all other English universities. To be satisfied that L_2 (which lacks such terms as "college" and "tutor") is also adequate to describing Oxbridge academically is to be satisfied that Oxbridge is not an academic domain, a world, distinct from the rest of the English academic world. And this is to be satisfied that L_1 is inadequate *simpliciter*, since it is admittedly inadequate for other universities, and the adequacy of L_2 shows that there is no basis, independent of L_1, for demarcating an academic domain of Oxbridge, within which alone L_1 could be considered adequate. On the other hand, there is at present no language, L_3, lacking academic concepts, whose adequacy in a domain wider than the academic world would show the latter to be a similarly spurious domain and L_2 to be itself inadequate. It has, of course, been suggested that there is such a language L_3, namely that of industry. For example, Clark Kerr, sometime President of the University of California, makes the suggestion in *The Uses of the University*:

> Basic to this transformation (... now engulfing our [i.e. American] universities ...) is the growth of the "knowledge industry." ... The production, distribution, and consumption of "knowledge" in all its forms is said to account for 29 per cent of gross national product ... and "knowledge production" is growing at about twice the rate of the rest of the economy. What the railroads did for the second half of the last century and the automobile for the first half of this century may be done for the second half of this century by the knowledge industry ... and the university is at the center of the knowledge process.[10]

Much criticism of ex-President Kerr seems to rest on the conviction that this language L_3 is not adequate for the academic world and that turning the University of California into an institution for which L_3 was adequate would render the title "University" inappropriate.

If we turn from universities to the universe the problem becomes at once more interesting and more pressing. There is a view on which, given that the universe comprises one fundamental domain in which all other domains are included, these other domains are spurious, and languages adequate for only some of them are not really adequate

[10] Kerr [8], pp. 87–88.

at all. The only adequate language is one adequate for the whole universe, and the only language with such pretensions is that of theoretical physics, L^*. Ryle expresses this view, which he rejects, thus:

> There is nothing that any natural scientist studies of which the truths of physics are not true; and from this it is tempting to infer that the physicist is therefore talking about the cosmos. So, after all, the cosmos must be described only in his terms. . . .[11]

In ontological terms, the claim is that the only things that exist are those over which the individual variables of L^* range. Thus Eddington says: "Modern physics has . . . assured me that . . . my second scientific table is the only one which is really there." This is the version of Eddington's view explicitly upheld by such contemporary philosophers as Feyerabend, Sellars, and Maxwell. Feyerabend puts the matter in terms of "concepts" rather than "entities," but the gist is the same:

> . . . the "uninstructed layman" does not think of molecules when speaking about the temperature of his milk. . . . However . . . a person who has already accepted *and understood* the theory of the molecular constitution of gases, liquids and solids cannot at the same time demand that the premolecular concept of temperature be retained. It is not at all denied by our argument that the "uninstructed layman" may possess a concept of temperature that is very different from the one connected with the molecular theory (after all, some "uninstructed laymen" intelligent clergymen included, still believe in ghosts and in the devil). What is denied is that anybody can consistently continue using this more primitive concept and at the same time believe in the molecular theory.[12]

Sellars puts the point directly in ontological terms:

> According to the view I am proposing, correspondence rules would appear in the material mode as statements to the effect that the objects of the observational framework *do not really exist—there really are no such things.*[13]

Maxwell, who argues in [10] for a modest realist view of theories, has subsequently adopted the "super-realist" view of Feyerabend and Sellars, namely that the entities of physical theory do not merely exist alongside (or inside . . .) those of everyday speech but displace them; that physical theory shows that the latter do not exist (e.g., [11]).

In contrast to these views we may set those of Ryle [22]. Being

[11] Ryle [22], p. 74.
[12] Feyerabend [4], p. 83.
[13] Sellars [24], p. 76.

concerned to preserve the adequacy of everyday language domain, Ryle takes the curious view that scientific languages afford no descriptions at all, and in particular that *L**, which alone claims the universe as its domain, does not do so.

> . . . physical theory, while it covers the things that the more special sciences explore and the ordinary observer describes, still does not put up a rival description of them. . . . It need not be a matter of rival worlds of which one has to be a bubble-world, nor yet a matter of different sectors or provinces of one world, such that what is true of one sector is false of the other. . . . In the way in which the joiner tells us what a piece of furniture is like and gets his description right or wrong (no matter whether he is talking about its colour, the wood it is made of, its style, carpentry or period), the nuclear physicist does not proffer a competing description, right or wrong, though what he tells us the nuclear physics of covers what the joiner describes.[14]

This could be construed as an extreme instrumentalist view, being applied to *all* general scientific statements, laws as well as theories, and Ryle indeed expresses such a view in *The Concept of Mind*.[15] But if the business of the scientist is as radically different from that of the "ordinary observer" as Ryle suggests, the instruments of science can hardly link everyday observation statements, although they may link scientific observation statements. Now if Ryle's view is correct, all problems of the relation between everyday and scientific descriptions of a domain disappear. This view must therefore be dealt with first, before proposed solutions to these problems can profitably be considered.

Ryle considers the example of an economic theory dealing with buying and selling by individuals.[16] It contains such terms as "the consumer," "the tenant," and "the investor." My brother, a named individual, may of course on occasion be acting as consumer, tenant, or investor, and the theory's predictions about how he will act on such occasions may be true or false. Yet, for Ryle, "in one way the Economist is not talking about my brother," since the theory does not refer to him, he is not named in it; it does not depend on his existence, or "What kind of a man he is. Nothing that the economist says would require to be changed if my brother's character or mode of life changed." This is the important sense in which, Ryle says, the theory does not describe my brother either truly or falsely: "We no longer suppose that the economist is offering a characterization or

[14] Ryle [22], p. 80.

[15] Ryle here characterizes law-statements as inference-tickets which "do not state truths or falsehoods of the same type as those asserted by the statements of fact to which they apply." [21], p. 121.

[16] Ryle [22], p. 70.

even a mischaracterization of my brother or of anyone else's brother." Thus this economic pseudo-description is not a rival to the real everyday description of my brother.

The sense in which economic theory is "about" my brother and does "describe" him is, for Ryle, the trivial (or at least irrelevant) sense in which it is about, or describes, anyone who is a consumer. "In another way the economist certainly is talking about my brother, since he is talking about anyone, whoever he may be and whatever he may be like, who makes purchases, invests his savings, or earns a wage or salary." The economist may say: "The consumer is . . . ," or "the consumer does . . . ," which is, of course, just to say: "All consumers are . . . ," or "All consumers do . . . ." So we may say that these statements of economic theory are true or false of my brother *in his rôle of consumer*; they describe him *as a consumer*, but they do not describe *him*.

This distinction, between the real everyday description of my brother and the pseudo-description of him by economic theory merely in his rôle of consumer, is both central to Ryle's thesis and entirely spurious. Ryle gives no clear examples of everyday description in the sense which economic theory is unfitted to provide. He merely observes that the economist does not need to know "that I have a brother, or what kind of a man he is." The first part of this, the bare existential assertion, is not a promising candidate for a distinctively everyday description. Someone giving an everyday characterization of brothers in general does not need to know that I have one. Everyday descriptions of brothers doubtless presuppose that there are brothers, but then so do economic descriptions of consumers presuppose that there are consumers—otherwise the theory would be of a mythology, not a science. And those who accept and those who reject Russellian descriptions agree that an economist and a plain man, setting out specifically to describe *my* brother, will equally fail to make true statements if I have no brother.[17]

The spuriousness is perhaps less obvious of a distinction between everyday "real," and economic pseudo-, descriptions that is based on Ryle's concept of "kinds" of men. The idea here is that the everyday description shows what "kind" of a man my brother is, as the economic description does not. Now the dispositional qualities that mark a "kind" of man should presumably be fairly permanent. But the most obvious examples are certainly not immutable: a *tolerant* man may become dogmatic, a *thin* man may grow fat. This sort of change must evidently be distinguished from the sort of change that

[17] E.g., Quine [18] and [19], and Strawson [26].

constitutes starting to display a disposition. A consumer need not consume all the time, but the disposition he has to consume in a certain way is present whether he is actively consuming or not. My brother does not become a certain kind of consumer simply by starting to consume, and cease to be of such a kind when he stops consuming. Changes in his economic *activities* are clearly distinct from changes (if any) in his economic *dispositions*—and it is the latter that economic theory describes.

Perhaps an economic disposition is not permanent enough for Ryle to characterize a "kind" of man, or perhaps it is not important enough. But the same may be as true of an everyday description and, conversely, economic character may be stable and important even in a philosopher's brother. A financially *prudent* (or careful, or mean) brother surely instances a kind of man as much as a *brave* brother. A prudential disposition may not show itself except in economic situations that may be rare (though perhaps not rarer than those which call for displays of bravery), but that observation is not to the point. Popular superstition and the paradoxes of material implication notwithstanding, a man does not influence his weight by declining to weigh himself (except perhaps causally).

Ryle's other examples, of an accountant's view of a College or library,[18] equally fail to show that such technical statements do not provide descriptions. The more interesting claim that he also makes in these cases is that such statements do not constitute *complete* descriptions of these institutions. Ryle makes this claim too glibly; the matter is more complex than he suggests. Certainly, from the fact (if it were so) that a college or library account said something about every college activity or every library book, it would not follow that it said everything about any (let alone every) activity or book. If we call such accounts "*comprehensive*" in the sense that they cover all items of which an accountant's description can be given, we may say that a comprehensive economic description of a domain need not contain a *complete* description of any item in that domain. Similarly, a comprehensive chemical description of a set of things will not contain a complete description of any member of the set, since every member will also have non-chemical (e.g., physical) properties. But we would think it arrogant of a physicist (whose everyday language is that of physics) to insist that therefore the chemical description of such a thing is not a real (i.e., physical) description at all. A description does not have to be complete to be a description.

However, Ryle's accountant could make a more interesting claim

[18] Ryle [22], pp. 75–78.

for completeness than the one Ryle lets him make. He could claim that the various other descriptions of college activity or library books could be reduced to (or replaced by, or translated into) financial descriptions. For example, he could claim that the value a student placed on an activity or book could in principle be described completely in financial terms. This is the claim analogous to that of the theoretical physicist: not the claim that descriptions in languages other than L^* are not given (which is clearly and trivially false), but the claim that all such descriptions could in principle be translated into descriptions in terms of L^*. Such a claim may be as implausible as the corresponding accountant's claim, but neither is refuted by pointing to the obvious and admitted fact that descriptions, within their various domains, are given in terms of other languages. Yet Ryle links his denial, of the library accountant's supposed claim, to the facts of usage thus: "The student's information about the books is greatly unlike the accountant's, and neither is it deducible from the accountant's information, nor *vice versa*. . . ."[19] This admittedly does not have the explicit form of an inference, but since Ryle gives no other grounds for denying deducibility, I think it must be read as such.

But, if completeness is taken as I suggest, on what grounds are claims about the unique adequacy of L^* to be assessed? What is being claimed for L^* if not, what is clearly false, that the language is used for every domain of things? Presumably that in principle L^* *could* be so used, supplemented with explicit definitions of complex terms to replace such everyday equivalents as "table" and "man." But it is far from clear what principle it is that properly assures us of this remote possibility, and what the grounds are for accepting it. The principle seems to be an ontological one, depending on a realist view of the theories expressed in L^*. And before this can be further discussed, some clarification of the notion of an "ontological principle," of what a realist view of theories commits one to, is needed.

For present purposes, I follow Quine in his analysis of existence. For a *named* individual, class, or attribute to exist is to be *the* value of a variable;[20] for an individual, class, or attribute of some *kind* to exist is to be *a* value of a variable.[21] But whether or not this particular analysis is accepted, existence, like truth, seems too fundamental a concept for the term "existence" and its synonyms to admit of different senses, such that an item could both exist in one sense and

[19] *Ibid.*, p. 78.
[20] Quine [17], p. 50.
[21] Quine [19], p. 224.

not exist in another. We may admit different *kinds* of existents, or entities, where items are classified by the methods used to establish their existence. For example, I reserve the term "thing" for items capable of spatio-temporal location, which are the concern of science, as opposed to such items as numbers. Similarly, we may admit different *kinds* of truths, where true statements are classified by the methods used to establish their truth. Thus we may distinguish mathematical from physical entities just as we may distinguish mathematical from scientific truths. But no more than the latter distinction requires distinct mathematical and empirical senses of "true" does the former require distinct mathematical and empirical senses of "exist."

This point needs to be made because it is tempting to try and evade the problem by saying that everyday and theoretical entities, like scientific and mathematical entities, can both exist, but in different "ways" or senses; just as God is sometimes said to exist in a different way from the rest of us. One is reminded of Mill's famous outburst against the convenient multiplication of senses in theology:

> Language has no meaning for the words Just, Merciful, Benevolent, save that in which we predicate them of our fellow-creatures; and unless that is what we intend to express by them, we have no business to employ the words. If in affirming them of God we do not mean to affirm these very qualities . . . we are neither philosophically nor morally entitled to affirm them at all. . . . To say that God's goodness may be different in kind from man's goodness, what is it but saying, with a slight change of phraseology, that God may possibly not be good?[22]

And in this respect at least the same is true of the existence of theoretical entities. What Russell calls "a robust sense of reality" is as necessary in philosophy of science as in logic, and he who pretends that electrons, say, have "another kind of reality" does as much "disservice to thought" as he who pretends that "Hamlet has another kind of reality"[23]—more indeed, since physical theory, unlike *Hamlet*, is not advanced as a piece of fiction.

In particular, it is regrettable that Nagel[24] should have taken his elaborate discussion of the different criteria for what he calls "physical reality" or "physical existence"[25] as establishing different "senses of 'real' or 'exist' that can be distinguished in discussions about the reality of scientific objects."[26] Of course, it establishes nothing of the

[22] Mill [13], pp. 122–123.
[23] Russell [20], p. 170.
[24] Nagel [14], pp. 146–151.
[25] *Ibid.*, p. 146.
[26] *Ibid.*, p. 151.

sort, as Maxwell rightly observes;[27] what it *does* establish is (a) that the existence of different "scientific objects" is established in different ways, and (b) that there are disputes about what the ontological commitments of particular theories are. These latter disputes, which are the subject of this paper, would not of course be resolved merely by accepting such an analysis of the *one* sense of "existence" as that of Quine's. An instrumentalist who will not admit the existence of electrons would then not admit a formalization of physical theory in which a variable ranges over electrons—except as a convenient "eliminable shorthand." But then, as Quine observes in a different context, the onus is on the instrumentalist to "devise contextual definitions explaining quantification with respect to [these] alleged entities."[28] Otherwise, "he will perhaps still plead that his apparent . . . entities are merely convenient fictions; but this plea is no more than an incantation, a crossing of the fingers, so long as the required contextual definitions are not forthcoming."

The point of invoking the Quinean analysis is that it shows in what terms ontological disputes may be resolved. It is characteristic of a theory not merely to make new statements but also to provide "a new language or extension of a language" in which to make them. Such is the language L^* in which physical theory T is formulated. If alternative formulations, with variables ranging over different things, are accepted as equivalent, i.e., as of the same theory, T, then an instrumentalist view is being taken at least of those things not common to both formulations. Conversely, a realist view must distinguish these formulations as distinct theories, since they carry distinct ontological commitments. To take a recent example, Hoyle's objection to a field formulation of relativity, because it permits a solution in a one-body universe, must be taken by a Quinean to reflect a realist view of the gravitational field.

Thus the ontological commitment of those advocating a theory is shown in the restrictions they impose on languages in which it can be stated. Fortunately, the detailed analysis of such commitment is beyond the scope of this paper, since it is common ground that, however weak the restrictions are, everyday language cannot satisfy them, i.e., that everyday language is inadequate for the domain of fundamental physics. There is therefore no doubt, on this analysis, that physical theory T assumes the existence of *some* things other than those assumed in everyday language. Disputes about just *what*

[27] Maxwell [10], pp. 20–21.
[28] Quine [17], p. 51.

things are assumed—fields or particles, for example—are not to the present purpose.

What is to the purpose is the "super-realist" claim that the ontology of whatever L^* is most adequate to physical theory T must be *complete* since L^* alone has the universe as its domain and T is the best supported theory of the universe. Let us, for convenience, call the things ranged over by variables in T, whatever they are, "basic things." Then other things, such as tables, that seem to exist independently in the physical world do not really do so; there are simply in their place (so to speak) suitably arranged collections of basic things. This is the super-realists' ontological principle. But let us press the question: what are, or could be, the grounds for thinking that this ontological principle is true? It seems only to entail that all statements ostensibly about tables, etc., can be translated into statements about basic things, but not conversely; and this claim begs the very question at issue, since it is admitted that such translations are not made, and as a matter of fact cannot be made with our present mathematical expertize. If it is insisted that such translation could be carried out in principle, we observe that the possibility of translation *is* the principle that is supposed to establish the unique adequacy of L^*. If this possibility is itself only established by assuming the unique adequacy of L^*, the argument is viciously circular.

In fact, I shall argue that the translatability claim is false and that, when it is analyzed a little more closely, common knowledge shows it to be false. The point of the claim seems to be that a statement about everyday things is either equivalent to some theorem of T or is false. No doubt, if current theoretical physics is true, this is true. But this is just what we don't know in any sense strong enough to support the conclusion. The translatability claim is, after all, not made for the "ultimate" physical theory (if any) of which we know nothing—except that it is true. The claim is made for the *current* theory T, of which all we know, at best, is that it is well supported by empirical evidence.

The translatability claim must therefore be that *T is better supported than any of the apparently independent statements that are to be translated into L^**. Only if this is so can we reasonably claim to know that any such statement must either be equivalent to a theorem of T or be false. Now if this were so, then suppose that from T, by dint of Eddington's "remorseless logic," we can extract as a theorem a statement S equivalent to "mahogany is blue." Until derivations of this complexity (in terms of L^*) have been carried out, which they haven't, we don't know that S is *not* a theorem. And if S *is* a theorem of T, the translatability claim entails that our pre-theoretical preference for $\sim S$ must be discarded. It entails that *any future conflict between a theorem of T and an accepted everyday statement must be resolved by discarding the everyday statement as false.* But a "remorseless logic" that leads to this conclusion would do better to show remorse. Experiments and observations peculiar to T, i.e., such that their results are describable only in L^*, are very few and doubtful compared with those that also lend support to statements in less comprehensive languages (such as, for example, that of atomic physics). The vast bulk of the evidence for T is also, and more directly, evidence for statements in other languages than L^*. Hence the constraint is on T, that *its* statements about collections of fundamental particles be either equivalent to accepted statements about the color of mahogany or be rejected as false, not *vice versa.*

There are, of course, borderline cases where the evidence for T and statements in other languages is on a par, and T reconstructs previously accepted statements. But rather too much has been made of these cases. Thus Popper:

> It is well known that Newton's dynamics achieved a unification of Galileo's terrestial and Kepler's celestial physics. It is often said that Newton's dynamics can be induced from Galileo's and Kepler's laws, and it has even been asserted that it can be strictly deduced from them. But this is not so: from a logical point of view, Newton's theory, strictly speaking, contradicts both Galileo's and Kepler's (although these latter theories can of course be obtained as approximations, once we have Newton's theory to work with).[29]

As a point against the necessity of an inductive *method* in science, this is well taken: that Newton could not have argued inductively in this case certainly shows that scientists do not always do so. It may be that they never do so, as Popper asserts (e.g., in [16]), or only do so in periods of "normal" science in between paradigm changes.[30] But whatever the fact is (see, e.g., Achinstein [1]) it is merely psychological, about how scientists actually make progress; *pace* Feyerabend,[31] it is not a *logical* fact, that "the contents of a whole theory (and thereby again the meaning of the descriptive terms which it contains) depends on . . . the set of all the alternatives which are being discussed at a given time."

The admitted existence of theoretical controversy and the modification by theories of laws which they have successfully explained have

[29] Popper [15], pp. 29–30.
[30] See Kuhn [9].
[31] Feyerabend [4], p. 68.

been much exaggerated in importance.[32] That "one and the same set of observational data is compatible with very different and mutually inconsistent theories"[33] is not a startling new discovery; it is a well-known elementary logical fact. Neither it nor the other facts cited go any way to support the super-realists' ontological principle. If to be is to be a value of a variable, then what we can assume exists is what is quantified over in well supported law-like statements, and T is *not* better supported than more mundane laws. Ontology is dependent upon epistemology, since what we can know to exist is merely a part of what we can know.

So a reasonable realist view of theories takes them on their first acceptance as *adding* to the stock of known things. As their continued success leads scientists in fact to treat the statements they explain as replaceable by those of the theory, so and no further may the pre-theoretical things be taken to be replaced by those of the theory. But success in explaining what is not yet replaced is a precondition of such ontological replacement; it does not comprise it. Nagel is right in insisting on an *initial* meaning invariance of accepted laws and theories under further theoretical explanation:

> Despite what appears to be the complete absorption of an experimental law into a given theory, so that the special technical language of the theory may even be employed in stating the law, the law must be intelligible (and must be capable of being established) without reference to the meanings associated with it because of its being explained by that theory. Indeed, were this not the case for the laws which a given theory purportedly explains, there would be nothing for the theory to explain.[34]

And similarly for the terms in a theory whose success is to be explained by its reduction to a theory of another science:

> . . . expressions distinctive of a given science (such as the word "temperature" as employed in the science of heat) are intelligible in terms of the rules or habits or usage in that branch of study, they must be understood in the senses associated with them in that branch *whether or not the science has been reduced to some other discipline.*[35]

Nagel is wrong only in implying here that successful explanation or reduction may not be *followed* by a shift in meanings. The point is that once a theory T_1 is successfully reduced to another, T_2, the very success of T_2 in its wider domain may lead us to abandon the narrower domain (of terms, and hence of things) within which T_1 was

[32] See Mellor [12], and on "meaning-variance" between terms employed in conflicting theories, see Fine [6] and Hesse [7].

[33] Feyerabend [4], p. 48.

[34] Nagel [14], p. 87.

[35] *Ibid.*, p. 352. My italics.

admittedly adequate. We may still, of course, retain the terms of T_1 for their convenience within its domain, without ontological commitment. The things they purport to refer to *are* then regarded as "convenient fictions." But until such a replacement, subsequent to successful reduction, of T_1 has in fact taken place, no ontological savings have been made; and until then no principle can guarantee that such savings will be made. To assert otherwise is to issue a blank check on the future success of current theory. Such checks having always bounced in the past, there is no good inductive reason to accept one now.

In fact, the ontological replacement even attempted by current physical theory is very modest, extending no further than things of molecular dimensions. The domain of L^* is not in fact universal: it is simply the domain of the very small. No doubt the very small can be found everywhere, but so can the very large. If it is true that inspecting common objects more closely reveals that their parts are atoms, it is equally true that inspecting them more distantly reveals that they in turn are parts of galaxies. The preference for explanation in terms of the very small is, as Schlesinger has observed,[36] a logically unwarranted prejudice (for those who share it, a regulative principle . . .), namely that of "micro-reduction." A similar principle of "macro-reduction"[37] might equally be held, as a determination to explain everything functionally, in terms of larger wholes of which it is a part. Indeed, Mach's Principle, that the inertial properties of a thing should be taken to be a function of the positions of all other things, is an illustration of just such an attitude. Either principle may be justified pragmatically by the scientific discoveries to which it gives rise. Neither is justified ontologically until either all talk of electrons can be not merely reduced to, but replaced by, talk of galaxies, or conversely. Until then, the universe, so far as we have reason to believe, contains as independent entities both galaxies and electrons—*and* tables and men. And it is more likely to continue to be reasonable to believe in the independent existence of tables and men than in the existence of electrons and galaxies, well supported as theories referring to the latter are. No conceivable theories either of the very large or very small are likely to carry their replacement of current things as far as tables and men; and any that do will certainly have replaced electrons or galaxies first.

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[36] Schlesinger [23], p. 46.
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