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How to Compare Theories: Reference and Change

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I. INTRODUCTION

Work at a decent level of generality is at an impasse today in the philosophy of science as well as in the history of science. The impasse separates the old philosophy of science (the positivists, and their sisters and their cousins and their aunts) over the issue of the comparability of scientific traditions from the new philosophy of science (Feyerabend, Hanson, Kuhn, Toulmin, *et al.*). I want here to display the source of the impasse in the theory of meaning, more especially in the theory of reference. I shall do this by examining a way with reference that looks like a promising way to attack the impasse. It turns out that although this way suggests a powerful critique of the new philosophy of science, important insights of the new philosophy of science show as well the error of this way. I shall draw the obvious conclusion about reference from this situation and, finally, I shall try to set this conclusion in the context of a program for how to compare theories.¹

II. THE IMPASSE

Let me begin by emphasizing that the framework for this discussion is realist in the following sense. I understand both parties to hold that terms in the extralogical vocabulary of a scientific theory are *prima facie* referring expressions. That is, that theoretical as well as observational terms of a theory (to use *oldspeak*) are understood as referring to entities, properties, relations, processes, etc., right here in the world.² The semantics for scientific theories, then, is to be specified in the

way familiar for extensional languages, so that a sentence is true just in case the entities referred to stand in the referred-to relations.

The impasse arises over the problem of how to compare competing or successive theories. Very roughly, the old philosophy of science suggests that we do so by making logical or evidential comparisons in a vocabulary shared by both theories. The new philosophy of science contends that although there may be terms shared by different theories, the concepts marked by these terms (in important and typical cases) will have changed so radically in the move from one theory to another as to preclude the use of ordinary logical or evidential tools.

Thus, controversy goes on as to whether ‘mass’ changes meaning in the transition from Newton to Einstein, as to whether “Mass is constant” in Newtonian mechanics contradicts “Mass is not constant” in special relativity. Kuhn ([7]: 101-02), for example, points out that although laws that look like the Newtonian ones can be derived in special relativity under appropriate assumptions, the significance of these laws is vastly different in the two theories and hence that Newtonian mechanics cannot be a limiting case, even, of special relativity. Hanson ([5], Chap. VI) has argued for the same conclusion with respect to the relationship between Newtonian mechanics and quantum mechanics, employing the nice Wittgensteinian metaphor that the logical gears of the two theories do not mesh.

The old philosophy of science really has not known what to do about these arguments, for it seems difficult to deny that the transition from Newton to Einstein, say, is marked by very significant conceptual innovation and change. But if we grant the philosopher-historians their point about radical conceptual change, then the process of scientific development seems to degenerate into a series of puns, into arguments all committing fallacies of ambiguity and into choices none of which are rationally founded.³

III. THE HARVARD GROUP *vs.* KUHN

One suggestion for sorting out things here has been to separate off and to focus on a referential component of meaning. A nice way of putting this idea is to construe the meaning of a word in a rather Fregean way as a pair consisting of the *concept* marked by the word and the *reference* of the word, that is, what the

word is true of. (I use *concept* here indistinguishably from 'sense' or 'intension' and *reference* indistinguishably from 'denotation', 'extension', or 'referent'. In general, I shall be careless about the niceties of philosophical semantics, in the belief that nothing important hangs on neatness.) Thus, we get the Fregean formula:

meaning = ⟨concept, reference⟩.

In this way, one distinguishes the concept of an acid marked in one theory, say, as a substance containing hydrogen which may be replaced by a metal to form a salt, from the concept marked in another theory as a substance having the ability to free hydrogen ions in solution. Surely these concepts are different—as Frege might say, 'acid' is presented here in different modes—but it does not follow that the reference of 'acid' is different. For the Fregean idea is that the concept determines the reference in such a way that words whose references differ mark different concepts, *but not conversely*.

This way of sorting, then, would allow conceptual change to be accompanied by referential (or, if you like, ontological) stability.

It is beyond question that scientists use terms as if the associated criteria were . . . *approximately* correct characterizations of some world of theory-independent entities, and that they talk as if later theories in a mature science were, in general, *better* descriptions of the *same* entities that earlier theories referred to. In my opinion, the hypothesis that this is *right* is the only hypothesis that can account for the communicability of scientific results. . . . (Putnam [13]: 46-47.)

If truth and reference are connected in the realist way previously indicated, then truth depends only on that component of meaning having to do with reference. Since the various logical and evidential relations can be explicated in terms of truth alone, this way of sorting would seem to allow for significant conceptual shifts and yet would retain the usual tools for the comparison of theories. The Harvard group have tried to take this sort of line in response to the new philosophy of science.⁴

This line depends on maintaining referential stability in the face of widespread conceptual change and, in effect, Kuhn has

taken up the challenge from the Harvard group by claiming that things just do not happen this way in science. Speaking of Dalton's atomic theory,

I point out that it implied a new view of chemical combination with the result that the line separating the referents of the terms 'mixture' and 'compound' shifted; alloys were compounds before Dalton, mixtures afterwards.

He continues in a footnote,

This example makes particularly clear the inadequacy of Scheffler's suggestion that the problems raised by Feyerabend and me vanish if one substitutes sameness-of-reference for sameness-of-meaning. . . . Whatever the reference of 'compound' may be, in this example it changes. (Kuhn [8]: 269.)

Presumably what Kuhn has in mind is something like this. In pre-daltonic chemistry, a non-elementary substance observationally homogeneous through and through would be classified as a compound. In post-daltonic chemistry, a non-elementary substance all of whose "ultimate particles" are alike (the language is Dalton's) would be classified as a compound. Thus, subscribers to pre-daltonic chemistry would count the metal alloys as compounds (and, indeed, in the debate between Berthollet and Proust over Proust's law of constant proportions the alloys were so classified, much to Proust's disadvantage). Whereas subscribers to post-daltonic chemistry, you and I presumably, would not count the alloys as compounds. Kuhn concludes from this change in the classification of alloys attendant on the rise of Dalton's atomic theory that there was a corresponding shift in the actual reference of 'compound'.

But what is it for the reference of 'compound' to shift? It is, for example, for "Alloys are compounds" to be true prior to Dalton and false thereafter. The obvious linguistic parallel is with something like "Nixon is the President of the United States", which was true just prior to August, 1974, and false just after August, 1974. That is, it appears that Kuhn views descriptive terms, like 'compound', so that they function (sometimes, at least) in the manner of descriptions used attributively. Kuhn does not give any account of how descriptive terms function. He does not say whether he thinks of them as disguised descriptions, as cluster terms, or whatever. But he says enough to suggest, to me at least, the following view, which

I shall dub *Kuhn's view*. Our concepts, along with our theories and beliefs, have an evolutionary history. The concept marked by a term like 'compound' is embedded in some relevant cluster of theories that employ the term at a given period of time, together with important background beliefs of practitioners of the theories. The concept picks out the reference in the way that the story of cluster terms would have it. Thus sufficient shift in theory and belief could shift the reference of 'compound' as the concept evolves.

Now I do not want to argue the case of compounds historically, although I think it questionable whether there actually was the conceptual evolution here that Kuhn seems to have in mind, but I do want to look at Kuhn's argument over shift in reference. Kuhn's line of argument seems to be just this. Notice that the average conscientious believer in pre-daltonic chemistry would classify the alloys as compounds, whereas the average conscientious believer in post-daltonic chemistry would not. According to Kuhn, therefore, prior to Dalton the alloys *were* compounds, whereas after Dalton they were *not*. Now, pay no mind to the problems over *average* believers and what they *would* do, still, where does the link between classification behavior and genuine reference come from? It can only come from the view, suggested above, that the concept of a compound is embedded in the chemistry plus background beliefs of the time. The classification behavior, then, becomes the living embodiment of the concept. As it classifies, so does the concept pick out the reference.⁵

This line of argument is coherent but not at all satisfactory. For it rules out even the possibility that in classifying the alloys as compounds the pre-daltonic chemists were mistaken. That is, Kuhn's view as just set out leaves no possibility that prior to Dalton the alloys were not compounds. Kuhn's view makes it necessarily true that prior to Dalton the alloys were compounds. Of course, to establish this we have to look at the living embodiment of the concept, the classification behavior, so the necessity here is *a posteriori*. But it is nevertheless necessary. The only possibility for alloys not to have been compounds prior to Dalton is, one might say, an epistemic one. There might be some systematic error in the calculations of average practitioners, or one might have introduced the terminology of compounds in a different way, for example, by interchanging 'compound' with 'mixture'. But,

apart from simple errors of calculation, the only possibility that Kuhn's view allows for alloys not to have been compounds is the possibility that our use of language might have been different from what it was.

Of course if we follow this view, then we shall find that although scientists under different paradigms may seem to communicate, necessarily there are always subtle ambiguities of reference present. We shall be forced to count paradigm changes not merely as changes of world view but actually as changes of worlds. We shall then see incommensurability where others have seen progress. And so when we try to map out the evolution of a scientific discipline, we shall find that it consists entirely of separate highways with no connecting roads. Thus, Kuhn's view leaves scant room for stability of reference and, in so doing, leads to a map of science that I think Kuhn would be the first to reject.

Conventional historians judge it an error on Berthollet's part to have counted the alloys as compounds. Perhaps the historians are wrong in this judgment. But surely, they might be correct. Berthollet might well have been wrong, and not just in the tenuous epistemic sense allowed by Kuhn's view. We have no guarantee that in conscientiously applying a theory, we will get matters right, even matters of classification. For not only are we fallible as practitioners, but our theories and background beliefs are fallible as well. If the interposition of evolving concepts links conscientious judgments of classification to genuine reference as a matter of necessity, then so much the worse for the semantical view that posits such rigid connectors. The idea of an empirical science precludes any necessary connections between the stories told by theories and what is actually true, and hence Kuhn's view runs counter to the idea of an empirical science.

IV. TROUBLE AT HARVARD

It appears, then, that Kuhn's response to the Harvard group is fundamentally inadequate, and perhaps their search for referential stability in the face of conceptual change is well founded. Unfortunately, I think there is a difficulty with the view of the Harvard group which is as fundamental as the difficulty with Kuhn's view. The trouble with Harvard, stated generally and briefly, is this. Reference is fixed by what is true. What is true,

as the counter to Kuhn's view emphasizes, does not depend on what any particular scientific theory says. But then, this theory-independence of truth must lead to a theory-independence of reference. Hence, that reference does not change when a theory changes would seem to follow merely from the referential machinery itself when deployed in the context of an extratheoretical concept of truth. But this seems too strong. For just as Kuhn's criterion leaves no room for sameness of reference, the Harvard way leaves no room for difference of reference. In both cases, embedded semantic assumptions seem to dictate conclusions in a totalitarian way, a way that allows no freedom for the facts of reference to speak for themselves.

I should like to elaborate this argument by showing in a little detail what the trouble is with Putnam's theory of reference. Putnam's theory of reference is the KKK theory of proper names suitably embellished to apply to general terms.⁶ It goes like this. In the beginning of the use of a term (like 'water', 'compound', 'electron', etc.), the term is attached by an act of introduction (conventional definition, ostension, or whatever) to some existent. Thereafter, the term refers to that existent to which it was originally attached. It does so without the mediation of necessary and sufficient conditions and without the mediation of concepts (as beliefs in the head or in the theory) that determine the reference. Thus, to answer the question, "What does 'compound' refer to?", trace back the chain of uses of 'compound' to the introductory act, and then 'compound' refers to whatever was picked out on that occasion.

There is a great deal to be said by way of filling in this sketch, sorting out some of the ambiguities, hedging it against obvious objections, bracketing off some problems of detail, and so on.⁷ But the trouble, as I see it, attaches directly to the root idea itself. For on its occasion of introduction what was picked out by the use of 'compound'? Was it a kind of substance which includes the alloys or not? Putnam no doubt is sufficiently progressive in his thinking to hold that post-daltonic chemistry is more nearly correct about these matters than is pre-daltonic chemistry. Hence, Putnam, no doubt, holds that alloys are not compounds and never were. A more reactionary thinker, however (or someone who knows something about chemistry that we do not yet know—i.e., a truly advanced thinker), might on Putnam's theory hold to the contrary that alloys are

compounds and always were. Neither alternative here yields the change of reference that Kuhn suggests. Is there any way on Putnam's account for such a change to have occurred?

One might suggest this. Suppose there are two words spelled and pronounced as 'compound' is, one word introduced in ancient times to refer to non-elementary substances homogeneous through and through and another introduced at the time of Dalton to refer to non-elementary substances all of whose ultimate particles are alike. We might even suppose that the use of the first word became obsolete after Dalton. All of this is possible on Putnam's account of reference, but it does not yield the possibility Kuhn has in mind. For Kuhn wants it true that alloys were compounds before Dalton, and he wants it false that alloys were compounds after Dalton. But neither word above satisfies both of these wants. In the sense of the obsolete word, it is still true today that alloys are compounds. Whereas in the sense of the newer word, it has always been false that alloys are compounds.

The source of the difficulty is clear and quite general. Because 'compound' attaches directly to its referent, there is no possibility that the referent could change its station *as referent* over time. The very root idea of Putnam's account of the descriptive terms of science is that they never function in the manner of definite descriptions used attributively. To the contrary, they are used purely referentially. In Kripke's parlance, 'compound' is a "rigid designator". The immediate consequence is that general statements, such as "Alloys are compounds", are if true at all then necessarily true (otherwise, necessarily false). But this connection between a term and its reference is as otiose in its rigidity as is the connection, according to Kuhn's view, between reference and the embodiment of a theory-dependent concept in classification behavior. Just as the commonplace notion that science is empirical serves to undermine Kuhn's view, so the commonplace idea that the subject of scientific endeavor varies from time to time likewise undermines Putnam's account of reference. For not only can one be mistaken and later rectify the mistake, but one can also choose a subject of study and later change one's mind. The story of the electron illustrates this.

The name 'electron' was introduced by G. Johnstong Stoney (1826-1911), in 1891, not of course as the name of the particles, but

as the name of the fundamental unit of electricity, namely, the electric charge on a hydrogen ion in electrolysis . . . as a natural unit. . . . (Cajori [1]: 359.)

The very *meaning* of the word [electron] has changed radically from the one it had when Millikan and others first used it. In the earlier sense the word did not refer, as it does today, to a particle or entity of some kind. . . . (DuMond [2]: XV.)

These citations indicate that the history of the term 'electron' as used in modern electrical theories fits precisely the suggestion that Kuhn makes about 'compound'. From the time of its introduction by Stoney in 1891 until just after the charge-to-mass experiments of J. J. Thompson in 1897, 'electron' referred to the unit quantity of electrical charge, whether positive or negative. Thereafter, with the increasing acceptance of the particulate nature of electricity, the term was naturally assimilated to the "corpuscles" of Thompson and was used to refer to the "particles" that carry the unit charge of negative electricity (these last quotes are scare quotes for the benefit of those scared at the thought that electrons are *bona fide* particles). Stoney was interested in building a theory of fundamental physical constants and systems of measurement. He (and Millikan after him) made no mistake of classification in using 'electron' to refer to a unit quantity of charge. No subsequent development in physics has shown him wrong in this classification. He was not acting on theories and beliefs that we now find fault with, nor was he ignorant of later developments which, had he known about them, would have changed his use of the term. For example, Stoney was well aware of particulate theories of electricity and sympathetic to them. He simply did not want to use 'electron' to commit him on that score. Thus, Stoney is not like Berthollet whose belief in combination by indefinite proportions and whose ignorance of later developments in chemistry lay behind his classification of the alloys as compounds. No; if Stoney made an error, it was simply an error of choice. Stoney might have chosen to introduce 'electron' to refer to the particle carrying the unit quantity. He chose otherwise. Subsequently this choice has been altered.

So here in the case of 'electron', we have a concept evolving, *à la* Kuhn, so as to bring with its evolution a shift of reference. The term introduced by Stoney does not refer to the particles we call 'electrons'; Stoney and Millikan made no mistake about the reference. The term we use these days does

not refer to the (unsigned) unit quantity of charge; we are not mistaken about the reference of 'electron' either. Nor are there two words, one introduced to refer rigidly in the Stoney way and the other introduced to refer rigidly in our way. For neither of these words fits the true formula, "Once 'electron' referred to a quantity of charge, but now 'electron' refers to a particle." Contrary to the idea at the root of Putnam's account, the referent of 'electron' as originally introduced is no longer the referent of 'electron' (just as Nixon is no longer the President). Certain particles are now in the reference of 'electron' which were not previously in the reference. These particles are now electrons truly; truly, they were not electrons in the year 1891. Of course, this change marks no change in the particles or in our beliefs about their nature. It marks a change in the reference of 'electron', and it shows the natural evolution of the subject matter of the science of electricity. By attaching the referent rigidly to the word, however, Putnam's account forecloses this very possibility.

We find in Putnam's account of reference the totalitarian character endemic to the Harvard group. They see stability of reference everywhere, just as Kuhn sees change. Kuhn relies entirely on a complex of theories plus background beliefs in order to determine reference. The Harvard group by-passes theories and beliefs entirely and attaches reference directly to the words. Both ways with reference are inflexible. Surely there must be a more moderate proposal. I shall make one.

V. A MODEST PROPOSAL

The proposal I have in mind is to let the facts of reference speak for themselves in the cases of interest. These cases are the ones that relate to the comparability problem. They can be characterized intuitively as those situations in the transition between theories (or, more broadly, between scientific traditions) where we are inclined to say that significant conceptual change has occurred. In just such situations, the question seems to arise as to whether the reference of certain terms is altered in the transition. The answer posed here is that we look at these situations and see.

One might suppose that this proposal is to let the facts in each case *decide* the question of sameness or difference of reference. But I should like to convince you that if we really

allow the facts to speak for themselves freely, they will not decide the question of co-reference in *any* of the cases of interest. That is, I should like to convince you of the following claim: *in the situations of interest, whenever a case can be made for sameness of reference, an equally good case can be made for difference of reference, and conversely.* The argument in support of this claim is straightforward, and we have already rehearsed it in the preceding sections.

The only evidence one can muster in support of a case for sameness or difference of reference is evidence as to classification output—what was (is) actually said to be what—and evidence as to the patterns of argument, reasoning, or whatever that motivates the output.⁸ (Notice the parallel here to the ways of supporting a moral thesis, say, an account of justice, namely, in terms of descriptive adequacy and adequacy to patterns of reasoning.) But in the cases of interest, the Kuhnian hypothesis, that the reference is determined by the conscientious classification behavior of average believers of the relevant theories, will fit the evidence almost perfectly well. For the Kuhnian hypothesis is cooked up precisely to mirror actual behavior patterns. Thus, the facts in these cases will always support a case for difference of reference. But, conversely, a good case for sameness of reference is likewise always available. For we can always mimic Harvard and select whichever theory in the transition seems the best precursor to the scientific beliefs we now hold true. (If none seems best, just pick any theory.) From this theoretical perspective, we can argue away discrepancies between our way with reference and actual classification behavior as plausible mistakes which holders of a flawed theory are bound to make. And in this way the facts can be mustered to support a good case for sameness of reference.⁹

Thus we see that in the cases of interest there are no reasons pushing co-reference one way or the other. When we allow the facts to speak freely, they do not turn out substantial enough to decide the question of sameness or difference of reference. My proposal is that we accept this as the way things are. Let us acknowledge that in a transition marked by significant conceptual change, there are no facts about sameness or difference of reference except for these: the reference of terms does not remain the same, nor does it change. I propose, then, that we accept the cases that Kuhn and Feyerabend have called cases of “incommensurability” as genuine cases of

indeterminacy with regard to sameness or difference of reference. In these cases, then, questions of co-reference are idle, for there is no fabric of truth out of which one could fashion an answer. The impasse over comparability arises precisely because out of just this fabric both Harvard and Princeton have been designing the emperor's clothes, each side pleading that its design shows her off at her best.

VI. HOW TO COMPARE THEORIES

The proposal is that radical conceptual change is marked by indeterminacy of co-reference. Two obstacles seem to stand in the way of its acceptance. One is posed by the belief, apparently shared by Harvard and the old philosophy of science, that the communicability, intelligibility, continuity, or whatever of science requires a common domain of reference. The other obstacle is that the proposal seems to leave comparability at a genuine impasse. But if I tell you how, accepting the proposal, to go about comparing theories, I think I shall have got round both obstacles. Let me try.

The idea of an empirical science encompasses the idea that we can never know whether our theories are true, and it accepts the evidence that none of them ever is. The best we can hope for, from a realist point of view, is that our theories are approximately true. We can hope, that is, that the terms of a theory pick out a portion of the world that satisfies the core theoretical principles at least approximately. Indeed, for a realist and an empiricist, a good way to think of a theory is as a *family* of models—construed as real-world structures—that satisfy a core of theoretical principles approximately. I shall refer to these models as the *worlds* of the theory. To compare theories, let us suppose that the worlds of the theories overlap in such a way that there is a correlation between the terms of the theories that makes correlated terms co-referential in each world of the region of overlap. Under this supposition, we can compare the theories by working out the usual logical and evidential comparisons in the overlapping worlds. The conclusions we arrive at will be of the form: if the world were such and so, *this* is how the theories would compare. If we pretend that one of the theories is really true, we can detach the conclusion, *this* is how the theories compare. Pretending to

truth thus yields the conventional realist (but non-empiricist) wisdom on the comparison of theories.

I should like to elucidate this prescription for how to compare theories by sketching out a case, namely, the case of Newtonian mechanics versus quantum mechanics. The Newtonian worlds are the structures of physical objects (particles and systems) with quantitative properties (mass and force will do) moving and interacting in space and time (or in space/time) approximately as Newton's laws would have it. The quantum worlds are the structures of physical objects (particles and systems) with states and quantitative properties (the "observables" of the theory) whose states evolve approximately according to Schrödinger's equation. Consider the Newtonian worlds with particles whose mass is, say, at least one gram. Corresponding to each such Newtonian world, there is a quantum world consisting of the same particles with the same mass numbers, each particle having an associated wave function which in position representation can be localized, say, to within one-tenth of a centimeter. The wave function for the entire quantum world can be considered a product of the individual wave functions at some initial moment. Its subsequent evolution is determined by the field of force derived via the correspondence principle from the classical force field of the correlated Newtonian world. It is a result due to Ehrenfest that the evolution of the quantum world will approximately match the evolution of the Newtonian world, provided we identify the various Newtonian dynamical quantities with the average values of the quantum-mechanical quantities bearing the same name. This approximation will hold to within two-tenths of a centimeter, in linear dimensions, for times on the order of 10^{18} years. (For comparison, the solar system is estimated to be only 4.5×10^9 years old.) One could put this somewhat loosely as follows: quantum mechanics enables one to derive that the macroscopic world is approximately Newtonian. Or again, that the approximate validity of Newtonian mechanics is derivable from quantum mechanics.

Notice that this comparison of the theories is a comparison by way of real-world structures and that it nevertheless begs no questions of co-reference. It supposes, only, that certain Newtonian systems can be treated quantum-mechanically,

under certain restricted conditions, provided we take the reference of certain Newtonian expressions to coincide with the reference of certain quantum-theoretical expressions *for these systems under those conditions*. This proviso for restricted co-reference is part of what every young physicist learns. Hence, whether we assimilate the descriptive terms to descriptions used attributively (*à la* Kuhn) or to descriptions used referentially (*à la* Putnam), we should equally well find the proviso in effect.

My general prescription for how to relate theories contains the supposition of overlapping worlds with a co-referential correlation of terms. If this supposition fails, then we can not compare the theories in the usual way at all. But as the case of classical to quantum mechanics illustrates, the supposition can certainly hold. Indeed, one would expect that since later theories develop out of earlier ones, and since contemporaneous rival theories develop against one another, the supposition of overlap is likely to hold in all the cases of interest.¹⁰ It is then a fact of the historical context in which theories grow that the practitioners of a theory learn restricted reference-preserving correlations as part of learning the theory. Thus, my general prescription contains a factual assumption that directs us towards detailed studies of each case.

There is one other feature of my general prescription worth attending to. It is the emphasis on approximations in the sense of what is approximately true. I have a strong feeling that this loose notion, which straddles the border between semantics and pragmatics, is just the right one (or, at least, one of the right kind) for treating the development of science. For both warring parties in the impasse over comparability have been guilty in their semantics of what Frank Ramsey called *scholasticism*, “the essence of which is treating what is vague as if it were precise and trying to fit it into an exact logical category” ([14]: 269). The concept of approximate truth is, perhaps, just loose enough to do the job of comparison properly.

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NOTES

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² I want to leave open the possibility that some referring expression may fail to refer (e.g., caloric, ether) as well as the possibility that some expressions may not even *prima facie* be referring expressions (e.g., point particle, light ray). With regard to the latter possibility, see Shapere [15].

³ The bewilderment generated by the new philosophy of science can be seen in the hesitant and clearly inadequate responses of Hempel [6] and Nagel [9].

⁴ I mean, of course, Scheffler [16] and Putnam [11]-[13]. Putnam takes himself to be building a theory in support of Dudley Shapere's notion of a "transtheoretical" term, so I suppose we should include Shapere in the group as well. No doubt several other writers on this topic would wish to be associated with Harvard.

⁵ Nosich [10] examines criteria for sameness of meaning and reference that involve belief behavior.

⁶ KKK is for Kripke, Kaplan, and Keith (Donnellan).

⁷ Teller [17] works out many of these details and their consequences. I have profited by discussions with Teller.

⁸ One might suggest another kind of evidence in addition; namely, that we see what sort of view of the development of science our supposition about reference fits in with. But since we introduce our hypothesis precisely in order to deal with the comparability issue, I think we have to take what comes here. We cannot beg the

question by feeding into the decision, from the outset, the plausibility of general views of science. The facts are supposed to speak for themselves freely.

⁹The reader may be puzzled at this point over the electron story. For there I claimed difference of reference, and here I seem to be undermining the claim. Not so. The electron case does not fit the prescription for a transition between theories marking a fundamental conceptual shift. There is no transition of note between Stoney, Millikan, Thompson, *et al.*, with regard to electrical theory. Hence, the “plausible mistake” ploy cannot be invoked to support sameness of reference for ‘electron’.

¹⁰In effect, Field [4] has argued that no such correlation exists between Newtonian mechanics and special relativity. But it appears from a response by Earman [3] that there is in fact a rather simple correlation.