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# ON REFERENCE AS A COMPONENT OF MEANING IN SCIENCE[1]

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In this paper I attempt a somewhat unorthodox foray into the realm of semantics. My intention is to give a critical examination of the concept of reference and its role as a component of the meaning of scientific constructs. This I try to accomplish not by remaining within the confines of a particular theory of meaning, but by evaluating the subject in a wider philosophical perspective which clarifies its relevance to such matters as the instrumentalism/realism dispute, meaning variance and theory change, indeterminacy of reference and the ramifications of a fallibilistic epistemology.

## *A Rationale for Referential Semantics: the Rejection of Instrumentalism*

First of all I must illustrate why the concept of reference should be considered to be constituitive of the meaning of scientific constructs at all, and this can best be done by exposing the inadequacies of the most widely held interpretation of the meaning of scientific terms, the partial interpretation thesis, together with the instrumentalism to which it gives succour.

According to this view[2] the non-logical terms in which a theory is expressed, standing by themselves, are meaningless. They, together with the logical connectives, constitute a formal language for which "there is no independent interpretation", the theory being a system of uninterpreted theoretical postulates composed in terms of them. Both theory and terms are then given "an indirect and incomplete interpretation" by means of 'correspondence rules', sentences connecting some of the terms of the theory with observation terms — the latter being those terms which refer to things, events, properties or relations that are directly observable and whose meaning is

therefore considered self-evident.

It is important to realise that this account is essentially reconstructionary[3]. Theoretical scientific terms, though "generally highly suggestive of physical significance"[4], cannot be taken at face value : whatever their apparent meaning, their true meaning can only be ascertained by establishing interpretations in terms of the observational basis of the theory by means of 'rules' that are uncovered through analysis.

Consequently, it cannot be asserted that a term such as 'electron' refers to an entity existing in space-time with its various peculiar properties simply on the strength of its appearing to do so. Or rather, in claiming that such a term does refer to a real object, "no matter what connotations (one) may connect with this assertion", one is really to be understood as endorsing the theory in which the term occurs as "an economical systematization... of a large class of particular facts and empirical generalisations... which is heuristically fertile in suggesting further questions and new hypotheses."[5]

On this interpretation of science the idea of reference as a component of meaning is plainly ruled out. For although *some* terms (the observational ones) do refer to real objects as they appear to[6], others (the theoretical terms) appear to, but when their meaning is analysed properly, do not. The situation is made the more hopeless when it is realised that there is no precise criterion by which theoretical terms can be demarcated from observational ones.[7]

This hopelessness, however, can be reserved entirely for the partial interpretation thesis itself, whose contrived and artificial character is revealed immediately one begins to consider the *function* of theories and the *purpose* of postulating new kinds of entity.

If the function of a theory is to *explain* some set of phenomena, then it must provide a conceptual scheme which is an *improvement* on that in terms of which the phenomena were previously described — for if the observational language supplied a satisfactory description there would be nothing to explain. Yet according to the standard account these new concepts cannot be understood except in terms of the original observational terms. This being so, it is impossible to see how these concepts could constitute a new framework for describing the phenomena, or how the theory could give the new insight that is its very function to provide.[8]

An unrepentant empiricist might reply to this that there is no contradiction here. Although a new concept must be interpretable in terms of concepts already understood, and must ultimately derive its meaning from its connection with the observational language, it is nevertheless richer in meaning in the sense that it *correlates* the

observational terms in a certain way,[9] unifying the description of previously diverse phenomena.

Now this defence could be accepted if it were possible on the standard account to understand *how* a theoretical concept correlates terms and helps to effect a unified explanation. But the 'how' is simply not forthcoming so long as the 'correlation' is provided by a facile juxtaposition of terms in mixed sentences, so long as the scope of the new terms is restricted so as not to exceed that of the old, so long as the terms supposedly explaining the phenomena *can refer ultimately only to those phenomena themselves.*[10]

This point can be illustrated by the example of kinetic theory. Here a very wide selection of phenomena, ranging from heat conduction and evaporation to observed values of specific heats, diffusion and Brownian motion, are all given explanations in terms of the vibration, translation and rotation of minute particles possessing certain slightly idealised properties. Now none of these explanations will work unless these particles are actually postulated as physically existing agents[11] with the properties ascribed to them. It is only on this assumption that anything new is learnt about the nature of heat and its propagation, evaporation, changes of state and so forth. If there exists no agency operating as a common material cause to these phenomena then there may perhaps be some computational convenience in 'connecting' them through theoretical concepts — but there is certainly no *explanation* of why or how they are connected the way they are or why we should expect the new concepts to provide predictive success. Nor, it should be pointed out, can there be given any reason for the "heuristic fertility" of theories. For if one postulates physically existing agents to explain something, it is likely that their existence and activity will have consequences elsewhere, whereas if the concepts are mere fictions this will not be expected.

Objections to such realism typically centre on the fact that some postulated entities, like, for instance, phlogiston or caloric fluid, have turned out to be non-existent. If such entities can in the light of further knowledge be shown to be fictions, what evidence do we have that any substance we care to postulate will not be found to be unreal in the same way ?

The answer to this is that the evidence will vary depending on the particular entity in question, and on what arguments can be mustered for or against its existence given the consequences that its activity would entail. But there can be no question of a *guarantee* of the existence of any entity exactly as it is described. Given the history of science it would be presumptuous indeed to expect that

any theory could provide a perfect description of the activity of some corner of the world.

Yet this state of affairs can by no means be construed as supporting the case of instrumentalism (or even the agnosticism that is more often what is explicitly avowed on this issue of whether theoretical terms should be interpreted as referring to real objects). For wherever in the history of science the postulation of the existence of some kind of entity has been successful in explaining some otherwise diverse physical effects but has subsequently been found to be inadmissable on other counts, it has always been superseded by an explanation in terms of another but different kind of physical entity—*not* by a merely logical construction. Indeed, it could not be otherwise, since it would be impossible to explain a connection among *physical* effects in terms of a *logical* entity : *physical effects must be caused by the activity of physical objects.*

Thus the failure of a *particular* postulated entity to fit the bill has no bearing on the question of whether in general an explanation of the connection and interrelations among diverse effects can be established without assuming a physically existing agency as their cause : and all the evidence indicates that it cannot.

In other words, we must take the meaning of scientific terms *literally*. Reconstructionary accounts of meaning and their accompanying instrumentalism are inadequate to cope with a working science, founded as they are only on the weight of authority and a tacit espousal of an incoherent ontology. And, as this discussion has showh, a realistic interpretation of science is not possible save on the assumption that all factual theories and their terms refer to kinds of entity whose existence is taken to be real.[12]

It is this fact that reference to real objects must be presumed in order to make sense of a theory that constitutes the rationale for treating reference as an essential component of meaning.[13]

*A Clarification : the Distinction between Reference and Extension*

Before proceeding further there is a matter concerning reference that demands our consideration. We have seen that reference to objects usually involves a presumption of their existence : but need things be actually existent in order to be referred to ?

These two concepts, existence and reference, are normally thought of as inextricably bound up : the set of referents of any construct[14] are those existing things which it applies to. Differently put, the *extension* of a construct is the set of things it is true of. However, as Bunge[15], and before him Geach and Buridan[16], have pointed out,

these definitions can be accepted as defining extension, but *not* reference. The reference relation is quite independent of questions of truth and existence : one may well refer to entities and not know whether one's description is *true* of anything in fact, whether the entities one has described do *exist*.

Although in the formal sciences this distinction might seldom be needed, the distinction between the class of referents and the extension of those constructs which concern factual matters is of crucial importance. Theoretical science would be impossible — or experimental science pointless — if reference could not be made to things whose existence was not already established — extinct species, new elementary particles, quasars, chromosomes, Stone Age societies and so on.

The alternative is to deny the distinction and instead of reference to talk of 'mistaken reference' in cases like that of phlogiston. But this position can quickly become uncomfortable. For it can be argued that if one makes legitimate reference to a thing dependent on the thing's existence then, with our knowledge of what exists always open to modification, strictly speaking no scientific construct has definite referents.[17] Maintaining the distinction and divesting the notion of reference of any dependence on actual existence or truth is surely preferable to this.

In any case, we are trying to capture as nearly as possible the concept of reference as it is used, not to reconstruct it so that most uses of it are illegitimate. And as a component in the interpretation of a working science it cannot depend upon evaluations which can only be established in hindsight. What a construct refers to must be known *before* it can be established whether the construct actually applies to anything. Reference to non-existent objects is common in the sciences and is quite as properly to be called reference as when the objects referred to are found to exist. There is not a different relation involved, for example, in references to continental land-bridges as opposed to drifting continental shelves.

Thus reference to something is not to be taken as a guarantee of its existence on the one hand : but it is nevertheless to be taken quite literally on the other. A continental land-bridge is taken to be something quite as solid and substantial as a continental shelf. Constructs may refer to either, but only those referring to the latter have extension.

*Reference and Theory Change*

However, the case for this construal of reference is not always as

clear-cut as this. Consider, for instance, a case where a highly-confirmed theory describing some sort of object in the physical world is superseded by another which is acknowledged to be superior in important respects. Now the property attributions in the second theory will in general be subtly different from those in the first. But does this not imply, if a construct's reference to a thing doesn't involve its being true of it, that the theories refer to different objects ? If it does, then surely this undermines the motivation for constructing a realist semantics. Should we not rather say in such cases that both theories *refer* to the same real object, only the second gives a superior *description* ?

This question is perhaps best answered with reference to a particular instance of such a theory change. To this end I shall use the rather hackneyed example of the supersedence of relativistic mechanics over the classical Newtonian theory. One of the property attributions that is subtly altered in the transition from one theory to the other is that of *mass*. Newtonian mechanics concerns bodies each of which is taken to possess a mass of unique value. This property is considered to be a constant with respect to the motion of the body. It is characterised most importantly by its role in the relation between the total force F applied to the body and the rate of change of the momentum p of the body (where p is defined by the relation $p=mv$) : $F=dp/dt$.

According to the Special Theory of Relativity, however, these two characteristics of the mass property are incompatible. To be more precise, if 'mass' is characterised by the relations $F=dp/dt$, $p=mv$ then it is not a constant, for its value depends on the velocity of the body relative to the inertial frame from which F and p are being considered (i.e. v). This property may be called the relativistic mass, and it is related to the mass of the body in its own inertial frame, the proper mass, $M_O$, which *does* have a unique, constant value for that body, by the formula $M_{AB} = Mo(1-(v/c)^2)^{-1/2}$ (c= velocity of light *in vacuo*, A is the inertial frame of the body, B the inertial frame from which it is being regarded.) Confirmation that the relativistic theory gives the correct account of the behaviour of bodies in all areas where its differences with Newtonian Mechanics would be apparent completely decides the issue in its favour.

Now, do we conclude from this situation that Newton's theory is wrong because the objects it refers to — bodies with the attribute of Newtonian mass, amongst others — do not in fact exist ? Or do we conclude that it refers to bodies (as they are, independently of theory) only the description it gives, including the attribution of Newtonian mass, is wrong ?

The rationale for the first answer is that since the meaning of 'mass' for a Newtonian is clearly different from that of either relativistic or proper mass — as indeed are the meanings of 'momentum', 'force', 'time' and the relations among them different from those of the terms with the same name in Relativity theory — then it is clear that a Newtonian's conception of a body is quite different from that of an Einsteinian. The term 'body' means different things to each of them, so that it is obvious that the referents ('bodies') are different in each case. The conceptualisation of the world is different in each theory, the 'ontology' is completely changed. Such a thesis has been argued with some energy by both Feyerabend and Kuhn,[18] who have also drawn the conclusion that since the theories do not concern the same things, the terms in each are not strictly comparable, or as they phrase it, the theories are incommensurable.

It is the threat of idealism implicit in this position more than any other consideration that lends weight to the other answer given to this problem. If indeed it were true that the theories were about entirely different objects then it is difficult to see how there could be any disagreement between them, or how there could be any empirical or intertheoretical evidence which would count both for Einstein's theory and against Newton's, as seems to be the case. Since there can be no such crucial ambiguity of reference in such statements as do count for one theory and against the other, the statements must refer to the same things, even if the meaning of the terms is not the same in both cases. Thus reference is seen as the stable element in theory change which ensures that theories *are* comparable, even though the meaning of the terms is changed by the different senses bestowed on them in the two theories.

Such a thesis has been championed by Bunge in his (1968)[19], and with altered terminology and in greater detail in his (1974)[20]. In the latter work the (referential) commensurability of competing theories is guaranteed by its definition as the possession of referents in common.[21]

Similarly, Putnam has shown an inclination for this Fregean construal of meaning, and again the motivation is transparent. He states :

"It is beyond question that scientists use terms as if the associated criteria were... *approximately* correct characterisations 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..."[22]

Now there appears to be much truth in this second thesis. It is clear that the two theories do concern to some extent the same features of the world, even though they describe them differently : each concerns at least the behaviour of moving objects, even if the objects are conceived of differently, and even if this difference in conception results in predictions of drastically different behaviour under certain conditions, and thus a different knowledge of what exists (or different 'ontology'). The question is whether or not "differently conceived objects" count as different referents or the same ones.

To say that they are the same referents implies that what any theory actually refers to is *not* determined by the meaning of the concepts of that theory. Both Bunge and Putnam admit that the concepts of 'mass', 'length' etc. are different in classical and relativistic contexts : it is what they are attributed of that is claimed to be the same, the object or referent, so the latter cannot depend on the former.

At first this seems fine. Of course the object doesn't depend on our concepts of its properties — indeed, this is the very pillar of realist epistemology ! However we must not be misled by the confusing Fregean terminology. Theories cannot refer to the object as it exists independently of our conception of it : we can at most refer to things as we conceive them to exist. To deny this is to commit oneself to a view of reference as a transcendental relation between our thoughts or constructs and the thing itself, a sort of latter-day divine intuition.

Now bodies as conceived in the classical framework are not the same as bodies as they are conveived within the relativistic framework; this has been granted. To say then that the theories nevertheless refer to the same things must amount to saying that although Newtonian theory was assumed to refer to bodies as classically conceived, it in fact referred to bodies as we now know them to be with the help of relativistic mechanics, and described them wrongly.

However, the knowledge we have of bodies through relativistic mechanics is itself contingent. If we make it a policy to decide the referents of theories with the aid of contemporary knowledge then, as we saw earlier, the referents of any theory will always be susceptible to revision in the light of later knowledge.

Field[23] has emphatically argued a very similar point in the course of an article attacking referential semantics. He claims that since the term 'mass' in Newtonian mechanics cannot be said to denote either

relativistic or proper mass, "the word 'mass' as used before relativity theory was discovered had no determinate denotation".[24] He then suggests the possibility that the referents of many current scientific theories may be refined, and "hence that many of our current scientific terms are referentially indeterminate".[25] These conclusions are somewhat ironical : constancy of reference despite change of conception can only be bought at the expense of a fluidity of reference from the historical standpoint.

But it is salutary at this point to remark that Field's whole thesis is roundly braced upon the reference-extension conflation which we discussed and rejected earlier. It is from the fact that "Einstein showed that there *is* no such quantity as "Newtonian mass" " that Field concludes that the quantity is referentially indeterminate, as he openly avows :

"I'm denying that there is or ever was such a quantity as "Newtonian mass", and hence I'm denying that Newton could have referred to "Newtonian mass" when he used the word 'mass'."[26]

In the light of the distinction between reference and extension introduced earlier, Field's electric claims are soon defused. That bodies with "Newtonian mass" have been discovered not to exist is no reason to deny that Newtonian mechanics *refers* to them : it only means that the theory has no *extension*, there are no bodies it is true of.[27] This perspective on reference is thus committed to the claim that the referents of the two theories *are* different, in agreement with Feyerabend and Kuhn. Newtonian mechanics refers to entities that do not exist — missiles, billiard balls, particles etc, which do not get heavier as they accelerate. Relativistic mechanics refers, as far as we know, to ones that *do*.

The intimation here is that the thesis of referents remaining constant through such a theory change either overtly depends upon, or else covertly involves, the idea that the referents of a theory must be identified with some set of objects that are known to exist.[28] This can be seen in the phrasing that was used to make sense of that thesis : "although Newtonian theory was *assumed to refer* to bodies as classically conceived, it *in fact referred to* bodies as we now know them to be with the help of Relativistic Mechanics". What a construct 'in fact refers to' is what we have here called its extension : what it is 'assumed to refer to' ('assumed' in a non-psychological sense) *is* what it refers to.

On this view it is impossible for a theory not to refer so long as it describes something — although in science, in cases where the description is self-inconsistent the referents may[29] be without interest once the discrepancy is discovered, as they may[30] be also if the description is inconsistent with other current scientific

knowledge. Accordingly, a theory refers to such objects as have the properties and behaviour it describes.

But how is this description-dependent construal of reference to be reconciled with a realist theory of science ? Surely whatever the internal force of the arguments for this position they have done nothing to allay the idealism which this position seems to entail : that theories of science are about our conceptions of the world as opposed to real objects whose existence cannot be doubted.

*Concluding Remarks*

In the remainder of this paper I shall try to lay to rest any such suspicions that this construal of reference harbours any support for idealism, and at the same time to throw new light on the matters of reference and realism themselves.

First, this description-dependent account of reference does not imply that referents are merely the model objects of a given theory. (That it should appear to do so is partly a consequence of the universal character of the example of theory change considered — not every theory change drastically alters our conception of the world.) In supposing that it does a seminal point is missed. For every object will possess properties apart from those attributed to it by any one theory, since no theory claims to provide a *complete characterisation* of an object, but only of some of its properties and modes of behaviour. In other words, theories describe objects according to sets of properties that characterise some aspects of the things' behaviour : they describe *natural kinds* of object rather than specific individuals. The individuals that are subsumed under a given kind will all have their own peculiar properties as well as the ones that characterise the kind in question. They will usually also fall under many different kind-classifications, as a stone might be a mineral, a magnet, an object subject to mechanical laws etc. Thus the total conception of any given object is composed of many subsidiary conceptions, each embodying a description of properties, behaviour and so forth under some kind-classification. It is to this object as totally conceived that a theory or other construct refers.[3 1]

The relevance of this observation to epistemology is this. Our knowledge of real objects is not exhausted by the description according to any one theory. Although a theory may form such an important part of our conception of an object that its supersedence by another theory radically alters our conception of what that object is, so that in the new light we are no longer referring to the same object as before — still our conception of the object will have many

elements in common with the old, it will still be the same kind of thing from many other perspectives. This fact, I think, substantially takes the sting out of referential change and the charge that if theories have different referents they cannot be competitors.

Secondly, reference as construed here is a *conceptual classification of objects.* However, the fact that a construct may refer to objects that do not actually exist, or that referents are *conceptions* of what exists rather than actual objects, presents no obstacle to a realistic interpretation of science, as I have already argued. Reference must not be interpreted as an anchor for realism in the sense of linking our constructs with things-in-themselves, or as a sort of realist analogue of ostension.

That is to say, (thirdly), reference must not be confused with the quite separate operation of picking out particular individuals that really exist, even though the same term is sometimes used for it in ordinary parlance — as in 'I was referring to that green object there'. For in distinguishing something only according to what properties it is taken to have, a crucial ambiguity is always left: there may be other things of the same kind. To single out a *particular* individual, on the other hand, an approximate spatio-temporal location needs to be allocated in addition to at least one property-attribution. This latter operation may conveniently be termed *'indication'*.

Now clearly this operation bears a much lighter conceptual load than does reference. One can indicate the objects referred to by competing theories without assuming either of the theories to be true, so it is not necessary for theories to refer to the same objects in order for experiments to be performed to decide between them. Again I think this obviates the need to promote the sharing of referents of consecutive theories as a weapon against incommensurability.

Before concluding, there is one more seemingly artificial feature of this account of reference which should first be cleared up. In denying that reference is a relation mapping onto theory-independent things, and in distinguishing it from extension, we were forced to conclude that the referents of Newtonian Mechanics do not exist. Yet the theory was thought to extend over all material things, and indeed it described their behaviour so well that it was almost universally held to be true for over two hundred years. The conclusion to be drawn from this is that questions of existence of kinds of things, (and thus extensions of theories), and of truth are relative to the body of knowledge available at any given time. We should therefore be careful not to attach too absolute a conception of non-extension to theories which have some truth in them — nor too absolute an

extension or degree of truth to theories that are good to the best of our knowledge.[32]

The implications of this account of reference for realism should now be drawn out. Real objects are not the referents of scientific theories : world must not be equated with world-picture. Instead we must recognise a notion of real object that transcends its particular descriptions : it is the part of the world-process which theories attempt to describe, their partial success giving evidence for its existence. There is something there existing and acting in space-time, which can be indicated as a result of its interaction with other objects. It is only in this sense of object — a roughly sketched thing inferred transcendentally from our improvements in understanding of it — that we can talk about consecutive theories concerning the same object : to claim that such theories have the same object in the Fregean sense of *referent* is incorrect.

In conclusion I will summarise the chief results of this investigation :

(i) Reference to kinds of object that are taken to be real is an essential component in the interpretation of scientific theories.

(ii) However, the kinds of object referred to do not necessarily exist exactly as described, and may not exist at all.

*Reference to kinds* is thus a *conceptual classification* of objects : objects referred to are objects as conceived, not conception-independent entities.

(iii) The class of referents of a construct should also be distinguished from its *extension.* The latter depends on the validity of application of the construct within the context of contemporary knowledge — and is accordingly fallible.

(iv) Reference is thus more closely related to possibility and imagination, extension to validity and fact. Neither should be confused with the *indication* of objects by spatio-temporal location and the odd property.

(v) A referential semantics which recognises these distinctions is fully in accord with a critical scientific realism.

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NOTES

[1] This paper is based on, and in part reproduces, material from chapters 1 and 3 of my M.A. dissertation, "A Disquisition on Properties", McGill University 1976.

[2]See particularly Carnap (1956), pp. 45-47, from which these quotations were taken.

[3]This reconstructionary character of the standard interpretation is a direct result of its historical development from the earlier positivist attempts to reconstruct science in such a way as to eliminate theoretical elements altogether. For an account of this development and a critical assessment see my (1976).

[4]Jammer (1974), p. 10. (It is a measure of the extraordinary influence of the partial interpretation thesis that the author of a treatise on Quantum Mechanics should devote half a chapter to it even though it nowhere aids him in his exposition.)

[5]Hempel (1958), p. 87 — Differently expressed, one is really assenting that the partially interpreted theory in which the term occurs "is accepted" and can "guide one's expectations" by enabling "predictions about future observable events (to be derived) from observed events", (Carnap (1956), p. 45); or that "available empirical evidence is sufficient to establish the adequacy of the theory as a leading principle for an extensive domain of enquiry." (Nagel (1961) p. 151).

[6]A stricter empiricism, of course, will not even allow this. Cf. that of Danto and Morgenbesser :
"For a sentence to have factual content some criterion must be specified which has *reference to sense experience.*" (My italics) Danto and Morgenbesser (1960), p. 24.

[7]See Hempel (1958), p. 42, and Nagel (1961), p. 83.

[8]There are of course numerous other objections to the standard view : not least that since it is only within a theoretical context that observations can be made and interpreted, the meaning of all scientific terms, and the observational statements, procedures and laws expressed in terms of them, must derive from the theoretical contexts in which they function — not the other way round. See my (1976).

[9]— specified by the logical structure of the theory and correspondence rules.

[10]— and, to boot, only through the medium of the original observational terms which were insufficiently explanatory in the first place.

[11]I use the terms 'agent' and 'agency' to emphasize the fact that the *activity* of objects and their resulting interaction with other objects is

their most fundamental characteristic — and the very characteristic which makes the postulation of their existence any use in an explanation.

[12] This is not to say, of course, that one must *believe* referents to be real : non-existent things are often referred to for the sake of argument, or to help us find out about existing things (as in the case of frictionless wheels, the idealized particles of kinetic theory etc.).

[13] Reference of course, cannot be the *only* component of meaning, since many concepts of quite different meaning might nevertheless refer to the same set of objects. In accounts of meaning in which it plays a part, it is usually paired with *sense* or *intension*. But I shall not discuss such a complement to reference here.

[14] A construct is a conceptual object such as a proposition, concept or theory.
Let me add here an expository note. Mario Bunge (in his (1974)) has shown how discussions of the reference of scientific theories can be shorn of irrelevant complications by construing the notion as a relation between constructs and sets of objects, rather than what particular people have in mind when they refer to things. Such a move does not depend on the assumption of a Popperian 'World 3', since constructs are only treated *as if* they have an independent existence of their own. This sort of treatment is possible because if the context in which a construct appears is specific enough people will have the same thing in mind. This is my justification for appealing in what follows to quasi-psychological notions such as a 'Newtonian's conception of a body' in my attempts to clarify the reference relation.

[15] Bunge (1974), Chs. 2 and 9.

[16] Commenting on Russell's theory, according to which "the reference of "some cat" would have to differ according as the proposition "Some cat is P" were true or false", Geach writes "This result is absurd; for, as Buridan pointed out long since, the reference of an expression can never depend on whether the proposition it occurs in is true or false". (Geach (1962)), pp. 51-52.

[17] Such a claim is briefly considered later in this paper.

[18] See Feyerabend's 'Explanation, Reduction and Empiricism', in (Feigl and Maxwell 1962).
For Kuhn's assert to this thesis, see in particular his 'Reflections on My Critics' in (Lakatos and Musgrave 1965), p. 269.
Whether they would accept my implication that their position entails the non-existence of bodies with Newtonian mass, I do not know —

though Kuhn's terminological confusion on exactly this issue is dramatically illustrated by these two quotes from the same page (p. 102) of his (1962). :

"But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name."

"Just because it did not involve the introduction of additional objects or concepts, the transition from Newtonian to Einsteinian mechanics illustrates ...'

[19] Bunge (1968), espec. pp. 302-304.

[20] (Bunge 1974), vol. 1, ch. 2, espec. section 4.2, pp. 66-68.
'Extension' and 'intension' are the components of meaning in (Bunge 1968), the meaning of a construct being expressed as the ordered pair of its extension E and intension I, ⟨ E, I ⟩. In (Bunge 1974) 'reference' is distinguished from 'extension' and takes its place as the first component of the ordered pair.

[21] *ibid*, Def. 2.31, p. 66.

[22] Putnam (1974), p. 155, quoted from Fine (1975). See also Putnam's (1973a) and (1973b).

[23] (Field 1973).

[24] *ibid.*, p. 462.

[25] *ibid.*, p. 480.

[26] *ibid.*, p. 470.

[27] However, Field could quite justifiably object that he has not been 'defused' at all. We have merely transferred the indeterminacy from reference to extension. This is true, extension is indeterminate in this sense : yet we have nonetheless made a considerable advance from the thesis that we cannot know what some theories are even about.

[28] Putnam does not distinguish reference from extension. It will be evident that the relationship of Bunge's work to the argument given here is more complex. For in his books 'Semantics, I and II' he strongly advocates both the reference/extension distinction (ch. 2, vol. I, ch. 9 vol. II) and the sharing of some referents of rival theories (pp. 66-68 ch. 2, vol. 1).
I contend that these two theses are representative of two conflicting strains in Bunge's work — or rather, that the latter seems to show a non-recognition of the former which is evidenced elsewhere in the work. Compare
"The notion of reference does not presuppose the concept of truth, which the concept of extension does." (Vol. II, ch. 9, p. 134)

with
"The class of objects for which a statement or a theory holds true may be called its *actual reference class*". (Vol. I, ch. 2, p. 37)
This covert reference/extension conflation is further betrayed on the next page (p. 38) :
"Should the hypothesis (in question) turn out to be false, the actual reference class of the theory would shrink to nought, but the assumed reference class would remain non-empty."

[29] I use 'may' and not 'will' here with the case of the inconsistencies of Quantum Electrodynamics in mind...

[30] ...and here with instances of major conceptual changes in science in mind.

[31] However, since reference occurs within a given context, especially in the case of abstract scientific theories, the conception is usually focused on the description according to this context : what kind of object one is referring to is, *most pertinently*, the kind of object the theory describes.

[32] This accords with the 'indeterminacy of extension' we granted Field in footnote 27.

## REFERENCES

ARTHUR (1976) : 'The Logical Empiricist Account of Scientific Knowledge' (Forthcoming).

BUNGE (1968) : 'Problems Concerning Intertheory Relations' in *Induction, Physics and Ethics,* Weingartner and Zecha (Eds.), D. Reidel, Dordrecht, 1968.

BUNGE (1974) : *Treatise of Basic Philosophy* — Vol. 1: Sense and Reference — Vol. 2: Interpretation and Truth, D. Reidel, Dordrecht, 1974.

CARNAP (1956) : 'The Methodological Character of Theoretical Terms', in *Minnesota Studies in Philosophy of Science Vol. I,* ed. Feigl and Scriven, pp. 38-76, Minnesota, 1956.

DANTO and MORGENBESSER (1960) : *Philosophy of Science, Readings,* Meridan, N.Y., 1960.

FEYERABEND (1962) : 'Explanation, Reduction and Empiricism', in *Minnesota Studies Vol. III,* ed. Feigl and Maxwell, Minnesota, 1962.

FIELD (1973) : 'Theory Change and the Indeterminacy of Reference', *J. Phil* LXX, 14, August 16, 1973, pp. 462-481.

FINE (1975) : 'How to Compare Theories : Reference and Change', NOUS IX, 1, March 1975, pp. 17-32.

GEACH (1962) : *Reference and Generality*, Cornell 1962.

HEMPEL (1958) : 'The Theoretician's Dilemma', in *Minnesota Studies, Vol. II* pp. 37-98, ed. Feigl, Maxwell and Scriven, Minnesota, 1958.

JAMMER (1974) : *The Philosophy of Quantum Mechanics*, John Wiley, N.Y., 1974.

KUHN (1962) : *The Structure of Scientific Revolutions*, (2nd edition), Chicago, 1970.

KUHN (1965) : 'Reflections on my Critics', in *Criticism and the Growth of Knowledge*, ed. Lakatos and Musgrave, Cambridge, England, 1965.

NAGEL, (1961) : *The Structure of Science*, N.Y., 1961.

PUTNAM (1973a) : 'Meaning and Reference', *J. Phil. LXX*, 1973, pp. 699-711.

PUTNAM (1973b) : 'Explanation and Reference', in *Conceptual Change*, ed. Pearce and Maynard, Dordrecht, D. Reidel, 1973.

PUTNAM (1975) : 'The Meaning of 'Meaning'', in *Minnesota Studies Vol. VII*, ed. Gunderson, Minnesota, 1975.