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The Concept of Structure in The Analysis of Matter

The Analysis of Matter (1927) is perhaps best known for marking Russell's rejection of phenomenalism (in both its classical and methodological forms) and his development of a variety of Lockean Representationalism—Russell's causal theory of perception. This occupies part 2 of the work. Part 1, which is certainly less well known, contains many observations on twentieth-century physics. Unfortunately, Russell's discussion of relativity and the foundations of physical geometry is carried out in apparent ignorance of Reichenbach's and Carnap's investigations of the same period. The issue of conventionalism in its then contemporary form is simply not discussed. The only writers of the period who appear to have had any influence on Russell's conception of the philosophical issues raised by relativity were Whitehead and Eddington. Even the work of A. A. Robb fails to receive any extended discussion,[1] although Robb's causal theory is certainly relevant to many of Russell's concerns, especially those voiced in part 3, regarding the construction of points and the topology of space-time. In the case of quantum mechanics, the idiosyncrasy of Russell's selection of topics is more understandable, since the Heisenberg and Schrödinger theories were only just put forth. Nevertheless, it seems bizarre to a contemporary reader that Russell should have given such emphasis[2] to G. N. Lewis's suggestion that an atom emits light only when there is another atom to receive it—a suggestion reminiscent of Leibniz, and one to which Russell frequently returns. In short, the philosophical problems of modern physics with which Russell deals seem remote from the perspective of postpositivist philosophy of physics.

But if the observations on philosophy of physics seem dated, this is not true of the theory of theories that the book develops. As Grover Maxwell emphasized,[3] it is possible to extract from the book a theory of theories that anticipates in several respects the Ramsey sentence reconstruction of physical theories articulated by Carnap and others many decades later.

I

The heart of the theory of *The Analysis of Matter* is the claim that our knowledge of the external world is purely structural. For Russell, this thesis was based on the contention that we are not "directly acquainted" with physical objects. On a more neutral reading, the basis for the thesis is the belief that the reference of the class of theoretical predicates has an explanation in terms of the reference of another family of predicates. Such an explanation, if it could be given, would be highly nontrivial since no definitional or reductive relation between the two classes of predicates is claimed, although there is for Russell something like a reductive relation between our knowledge of theoretical properties and our knowledge of perceptual properties. In this respect the theory of *The Analysis of Matter* stands in marked contrast to the phenomenalism of Russell's 1914 external world program.[4] The point of the latter, of course, is to assume only perceptual objects (sensibilia, or actual and possible sense-data), and perceptual relations (especially perceptual similarity relations), and to *explicitly define* all other objects and relations from these. In *The Analysis of Matter*, Russell wishes to exploit the notion of logical form or structure to introduce scientific objects and relations by means of so-called *axiomatic* or *implicit definitions*. Thus, if we represent a scientific theory by

(1) $\theta(O_1, \ldots, O_n; T_1, \ldots, T_m)$,

say, where $O_1, \ldots, O_n$ are observational or perceptual terms and $T_1, \ldots, T_m$ are theoretical terms, then Russell in 1927 is prepared to accept the Ramsey sentence

(2) $\exists \tau_1, \ldots \exists \tau_m \theta(O_1, \ldots, O_n; \tau_1, \ldots, \tau_m)$

as the proper statement of our scientific knowledge. And (2) is legitimate whether or not the theoretical terms in (1) are explicitly definable from the observational terms; indeed, the whole tenor of Russell's discussion in *The Analysis of Matter* is that theoretical terms will not be explicitly definable in purely observational terms.[5]

It is not entirely clear why Russell abandoned his earlier phenomenalism. His explicit discussion of the issue in chapter XX of *The Analysis of Matter* is rather inconclusive. Postulating nonperceptible events allows us to maintain a very desirable continuity in stating the laws of nature,[6] but it appears that "ideal" perceptible events (the sensibilia of 1914) would serve equally well.[7] A perhaps more interesting reason for Russell's shift emerges from his discussion of the then current physics in part 1. Briefly, the point is that a phenomenalist reduction in the style of the 1914 program does not do justice to the novel "abstractness" of modern physics. Compared with the knowledge expressed in classical physics or common sense, for example, the knowledge conveyed by twentieth-century physics appears to be on a higher level of abstraction: it is "structural" or "mathematical" in an important new sense. For Russell, this leads to a partial skepticism regarding our knowledge of the physical world:

> Whatever we infer from perceptions it is only structure that we can validly infer; and structure is what can be expressed by mathematical logic.[8]

> The only legitimate attitude about the physical world seems to be one of complete agnosticism as regards all but its mathematical properties.[9]

The view expressed in these passages is very strongly anticipated in Russell's *Introduction to Mathematical Philosophy* (1919), where its Kantian motivation is quite evident:

> There has been a great deal of speculation in traditional philosophy which might have been avoided if the importance of structure, and the difficulty of getting behind it, had been realized. For example, it is often said that space and time are subjective, but they have objective counterparts; or that phenomena are subjective, but are caused by things in themselves, which must have differences *inter se* corresponding with the differences in the phenomena to which they give rise. Where such hypotheses are made, it is generally supposed that we can know very little about the objective counterparts. In actual fact, however, if the hypotheses as stated were correct, the objective counterparts would form a world having the same structure as the phenomenal world, and allowing us to infer from phenomena the truth of all propositions that can be stated in abstract terms and are known to be true of phenomena. If the phenomenal world has three dimensions, so must the world behind phenomena; if the phenomenal world is Euclidean, so must the other be; and so on. In short, every proposition having a communicable significance must be true of both worlds or of neither: the only difference must lie in just that essence of individuality which always eludes words and baffles description, but which, for that very reason is irrelevant to science. Now the only purpose that philosophers have in view in condemning phenomena is in order to persuade themselves and others that the real world is very different from the world of appearance. We can all sympathize with their wish to prove such a very desirable proposition, but we cannot congratulate them on their success. It is true that many of them do not assert objective counterparts to phenomena, and these escape from the above argument. Those who do assert counterparts are, as a rule, very reticent on the subject, probably because they feel instinctively that, if pursued, it will bring about too much of a *rapprochement* between the real and the phenomenal world. If they were to pursue the topic, they could hardly avoid the conclusions which we have been suggesting. In such ways, as well as in many others, the notion of structure. . .is important. (pp. 61–62)

To sum up, on Russell's "structuralism" or "structural realism," of "percepts" we know *both* their quality and structure (where Russell's use of the term "quality" includes relations), while of external events we know only their structure. The distinction is, in the first instance, between properties and relations of individuals and properties and relations of properties and relations. Structural properties are thus a particular subclass of properties and relations: they are marked by the fact that they are expressible only in the language of higher-order logic. Unlike Locke's distinction between primary and secondary qualities, the structure/quality distinction does not mark a difference in ontological status: external events have both structure and qualities—indeed when we speak of the structure of external events, this is elliptical for the structural properties of their qualities. Neither is it the case that one is "more fundamental" than the other or that qualities are "occurrent" while structure is a power. What is claimed is a deficiency in our knowledge: of external events we know only the structural properties of their properties and relations, but we do not know the properties and relations themselves. Of course physical knowledge falls within the scope of this claim, so that the theory of perception immediately yields the consequence that physical theories give knowledge of structure and *only* knowledge of structure.

Russell's emphasis on structure in *The Analysis of Matter* has close affinities with much other work of the period, especially with the two classics of early (pre-1930) positivism: Schlick's *General Theory of Knowledge* (1918, second edition 1925) and Carnap's *Aufbau* (1928). Indeed, the "critical realism" of *General Theory of Knowledge* is identical in almost every respect to Russell's structural realism of 1927. Schlick argues that modern physics deals with real unobservable entities (atoms, electrons, the electromagnetic field) which cannot be understood as logical constructions out of sense-data in the manner of Mach's *Analysis of Sensations* (1897) or Russell's 1914 external world program (§26). Such entities are not experienceable, intuitable, or even picturable; accordingly, Schlick goes so far as to call them "transcendent" entities and "things-in-themselves" (25). Nevertheless, this "transcendent" presents no obstacle to our knowledge or cognition, for *knowledge relates always to purely formal or structural properties*—not to intuitive qualities or content. Thus, while we cannot experience or intuit the entities of modern physics, we can grasp their formal or structural features by means of axiomatic or implicit definitions in the style of Hilbert's *Foundations of Geometry* (1899)—and this is all that knowledge or cognition requires (§§5–7). The similarities with Russell's view of 1927 are patent. However, there is one significant difference: Schlick draws a sharp contrast between knowledge (*erkennen*) and acquaintance (*kennen*). On his account *knowledge* by acquaintance is a contradiction in terms, for only structural features are ever knowable (§12). We are acquainted with or experience (*erleben*) *some* qualities (content), but we have knowledge or cognition of none. As we have seen, for Russell we know both the form and the content of percepts.

Carnap's *Aufbau* does not embrace structural realism. *All* concepts of science are to be explicitly defined within a single "constructional system" whose only nonlogical primitive is a "phenomenalistic" relation R_s of *recollected similarity*. Yet the form/content distinction and the notion of logical structure are equally important. To be sure, all concepts of science are to be constructed from a basis in the given, or "my experience," but the *objectivity* of science is captured in a restriction to purely structural statements about this given basis. Although the "matter" or content of Carnap's constructional system is indeed subjective or "autopsychological"—and therefore private and inexpressible—what is really important is the logical form or structure of the system. For it is logical form and logical form alone that makes objective knowledge and communication possible:[10]

> Science wants to speak about what is objective, and whatever does not belong to the structure but to the material (i.e. anything that can be pointed out in a concrete ostensive definition) is, in the final analysis, subjective. One can easily see that physics is almost altogether desubjectivized, since almost all physical concepts have been transformed into purely structural concepts. . . . From the point of view of construction theory, this state of affairs is to be described in the following way. The series of experiences is different for each subject. If we want to achieve, in spite of this, agreement in the names for the entities which are constructed on the basis of these experiences, then this cannot be done by reference to the completely divergent content, but only through the formal description of the structure of these entities.[11]

Characteristically, Carnap does not rest content with a simple reference to logical form or structure, he turns his "objectivity requirement" into a definite technical program: the program of defining all scientific concepts in terms of what he calls *purely structural definite descriptions* (§§11–15). Such a definition explains a particular empirical object as the unique entity satisfying certain purely formal or logical conditions: the *visual field*, for example, is defined as the unique five-dimensional "sense class" (§§86–91). The point is that purely structural descriptions contain no nonlogical vocabulary; ultimately, we will need only variables and the logical machinery of *Principia Mathematica* (or set theory).[12] Thus, while Russell in *The Analysis of Matter* would formulate our physical knowledge in the manner of (2) (i.e., by turning all theoretical terms into variables), Carnap in the *Aufbau* goes much further: all terms whatsoever are to be replaced by variables. (The reader might very well wonder how Carnap's one nonlogical primitive R_s is to be itself eliminated in favor of variables. We shall return to this later.)

II

We believe there are insurmountable difficulties with the theory of theoretical knowledge just outlined. So far as we are aware these difficulties were first raised by M. H. A. Newman in an article published in *Mind* in 1928. Newman's paper—the only philosophical paper he ever published—is not as well known as it deserves to be. (The prose is quite delightful, and for that reason alone, it deserves a wider readership.) This paper contains much that is of interest today, and so we propose to discuss the structuralism of Russell's *Analysis of Matter* from the perspective of Newman's paper.

Newman begins his discussion with the observation that on any theory of scientific knowledge, the question of the truth of at least some propositions of physics should turn out to be nontrivial. Consider, for example, the question whether matter is atomic. Newman observes that on any account "this is a real question to be answered by consideration of the evidence, not a matter of definition."[13] In fact, whether matter is atomic is a question that concerns the holding of various "structural properties," although they are of a fairly high level of abstraction. (It entails, for example, that physical objects have isolable constituents, and that these constituents have a certain theoretically characterizable autonomy.) Newman's point is that whether the world exhibits such properties is a matter to be discovered, not stipulated, and we may demand of a theory of theories that it preserve this fact. The gist of his criticism of Russell is that (with one exception) Russell's theory does not satisfy this constraint. Let us see how the argument goes.

The difficulty is with the claim that *only* structure is known. On this view, "the world consists of objects, forming an aggregate whose structure with regard to a certain relation R is known, say [it has structure] W; but of. . .R nothing is known. . .but its existence;. . .all we can say is, *There is* a relation R such that the structure of the external world with reference to R is W" (Newman, p. 144). But "*any* collection of things can be organized so as to have the structure W, provided there are the right number of them" (p. 144). Thus on this view, only cardinality questions are open to discovery! Every other claim about the world that can be known at all can be known a priori as a logical consequence of the existence of a set of α-many objects. For, given a set A of cardinality α, we can, with a minimal amount of set theory or second-order logic, establish the existence of a relation having the structure W, provided that W is compatible with the cardinality constraint that $|A| = \alpha$. (The relevant theorem from set theory or second-order logic is the proposition that every set A determines a full structure, i.e., one that contains every relation [in extension] of every arity on A; such a structure forms the basis for a [standard] model for the language of second [or higher] order logic.)

It is important to be clear on the nature of the difficulty Newman has uncovered. The problem is *not* a failure of the theory to specify the domain of objects on which a model of our theories of the world is to be defined. The difficulty is not the one of Pythagoreanism raised by Quine[14] and John Winnie[15]; that is to say, it is not that Russell cannot rule out abstract models. Indeed Russell himself raises this problem in the introduction to *The Analysis of Matter*:

> It frequently happens that we have a deductive mathematical system, starting from hypotheses concerning undefined objects, and that we have reason to believe that there are objects fulfilling these hypotheses, although, initially, we are unable to point out any such objects with certainty. Usually, in such cases, although many different sets of objects are abstractly available as fulfilling the hypotheses, there is one such set which is much more important than the others. . . . The substitution of such a set for the undefined objects is "interpretation." This process is essential in discovering the philosophical import of physics.
>
> The difference between an important and an unimportant interpretation may be made clear by the case of geometry. Any geometry, Euclidean or non-euclidean, in which every point has co-ordinates which are real numbers, can be interpreted as applying to a system of sets of real numbers—i.e., a point can be taken to *be* the series of its co-ordinates. This interpretation is legitimate, and is convenient when we are studying geometry as a branch of pure mathematics. But it is not the important interpretation. Geometry is important, unlike arithmetic and analysis, because it can be interpreted so as to be part of applied mathematics—in fact, so as to be part of physics. It is this interpretation which is the really interesting one, and we cannot therefore rest content with the interpretation which makes geometry part of the study of real numbers, and so, ultimately, part of the study of finite integers. (pp. 4–5)

In this case we have a simple criterion for separating important from unimportant interpretations: the important interpretations are connected with our observations, the unimportant ones are *not*. Newman's problem arises *after* the domain has been fixed: in the case of abstract versus physical geometry we have to distinguish different relations on different domains. The problem is made easier by the fact that we can exclude one of the domains *and therefore* one class of relations. When the domain of the model is *given*, we must "distinguish between systems of relations that hold among the members of a given aggregate" (Newman, p. 147). This is a difficulty because there is *always* a relation with the structure W. Russell cannot avoid trivialization by claiming that the relation with structure W that exists as a matter of logic is not necessarily the *important* relation with structure W (as the interpretation in R^3 is not the important interpretation of geometry). That is to say, one cannot avoid trivialization in this way without some means of distinguishing important from unimportant relations on a given domain. But the notion of importance that must be appealed to is one for which Russell can give no explanation, and in fact, his own theory precludes giving any explanation of the notion.

Newman's summary is well worth quoting in full:

> In the present case [i.e., in the case where we must choose among relations on a given domain rather than choose among different relations on different

domains] we should have to compare the importance of relations of which nothing is known save their incidence (the same for all of them) in a certain aggregate. For this comparison there is no possible criterion, so that "importance" would have to be reckoned among the prime unanalyzable qualities of the constituents of the world, which is, I think, absurd. The statement that there is an *important* relation which sets up the structure W among the unperceived events of the world cannot, then, be accepted as a true interpretation of our beliefs about these events, and it seems necessary to give up the "structure/quality" division of knowledge in its strict form. (p. 147)

Recall that giving up the structure/quality division in its strict form means giving up the idea that we do not know the qualities of unperceived objects. This of course would rob Russell's theory of its distinctive character, and so far as we can tell, Russell remained equivocal on this issue for the remainder of his philosophical career.

It might be thought that Newman's objection depends on too extensional an interpretation of relations, and that once this is given up, a way out of the difficulty is available. The idea is that logic (or logic plus set theory) only guarantees what Newman calls *fictitious* relations—relations whose only extrinsic property is that they hold between specified pairs of individuals—e.g., binary relations whose associated propositional functions are of the form "x is a and y is b, or x is a and y is c, etc.," where a, b, and c are elements of A. But in fact this is not so. Given some means of identifying the individuals of the domain A, we can always find an isomorphic structure W' which holds of a nonfictitious relation R' on domain A'. We may now define a nonfictitious relation S whose field is included in A: S is just the image of R' under the inverse of the isomorphism that correlates A and A'. Newman's example is a simple combinatorial one. Consider a graph $\Gamma(R)$ of a relation R on A x A, where $A = \{a, \alpha, \beta, \gamma\}$

$$\begin{array}{ll} & \alpha \\ & / \\ \mathrm{a}- & \beta \\ & \backslash \\ & \gamma \end{array}$$

$\Gamma(R)$

Define a *new* relation S on A by letting S hold of (x, y) if $f(x)$ and $f(y)$ belong to different alphabets where "a" $= f(a)$, "α" $= f(\alpha)$, "β" $= f(\beta)$, "γ" $= f(\gamma)$. Then S is nonfictitious. It is clear that this strategy is perfectly general. Following Newman, call a nonfictitious relation *real*. Then reality in this sense is "preserved under isomorphism"; i.e., any extensionally specified relation may be regarded as the image of a real (nonfictitious) relation. And this real relation exists if the fictitious one does. Thus a claim of the form "There exists a *real* relation R such that. . ." will be true given a claim of the form "There exists a relation R such that. . ." Of course S may nonetheless be *trivial* or *unimportant*. But this just shows that eliminating extensionalism does not solve our problem.

We might try to add further constraints on the notion "R is a real relation." But then we move away from the relatively clear intuition that motivated its original characterization, viz., that a relation with only a purely extensional characterization is fictitious. (Notice that certain obvious candidates such as "projectability" cannot be appealed to since it is precisely the class of relations and properties which enter into laws that we are attempting to delimit.)

The conclusion Newman draws from this analysis is, we think, the right one: since it is indisputably true that our knowledge of structure is nontrivial—we clearly do not stipulate the holding of the structural properties our theories postulate—it cannot be the case that our knowledge of the unperceived parts of the world is *purely* structural. The unobservable/observable dichotomy is not explicable in terms of the structure/quality division of knowledge without giving up the idea that our knowledge of the unobservable parts of the world is discovered rather than stipulated. Of course it is also possible to give up the naïve intuition on which Newman's conclusion from his analysis depends. In this case one would *accept* the stipulative character of the theoretical components of our knowledge, and indeed much of the philosophy of science that immediately preceded and subsequently followed *The Analysis of Matter* did just this. Russell is unique in wanting to preserve a nonconventionalist view of the world's structure, while retaining a structure/quality or form/content division of knowledge that is intended to more or less correspond to the division between theoretical and observational knowledge.

By the way, Newman's objection also explains the intuition that despite its intention, Russell's structuralism collapses into phenomenalism. The difficulty is that every assertion about unperceived events is trivially true, i.e., true as a matter of logic plus an empirical assumption concerning cardinality. Now the phenominalist claims that statements about the external world are "reducible" to statements about perception. Whatever *else* this may mean, it requires that, given the appropriate reductive definitions, statements about the physical world are entailed by statements about perception. But if Russell's view is accepted, this characteristic consequence of phenomenalism is almost guaranteed: if statements about the physical world are true as a matter of logic, then they are implied by every proposition, and, in particular, they are implied by the statements of perception. Thus *modulo* the single nonlogical assumption concerning the cardinality of the external world, Russell's structuralism guarantees the truth of this phenomenalist thesis!

The only reference to Newman's paper of which we are aware is in the second volume of Russell's autobiography. Russell very graciously acknowledges Newman's criticism in a letter he reprints without comment. The letter is worth quoting at length. Dated April 24, 1928, it is identified simply as "To Max Newman, the distinguished mathematician."

> Dear Newman,
>
> Many thanks for sending me the off-print of your article about me in *Mind.* I read it with great interest and some dismay. You make it entirely obvious that my statements to the effect that nothing is known about the physical world except its structure are either false or trivial, and I am somewhat ashamed at not having noticed the point for myself.
>
> It is of course obvious, as you point out, that the only effective assertion about the physical world involved in saying that it is susceptible to such and such a structure is an assertion about its cardinal number. (This by the way is not quite so trivial an assertion as it would seem to be, if, as is not improbable, the cardinal number involved is finite. This, however, is not a point upon which I wish to lay stress.) It was quite clear to me, as I read your article, that I had not really intended to say what in fact I did say, that *nothing* is known about the physical world except its structure. I had always assumed spacio-temporal continuity with the world of percepts, that is to say, I had assumed that there might be co-punctuality between percepts and non-percepts, and even that one could pass by a finite number of steps from one event to another compresent with it, from one end of the universe to the other. And co-punctuality I regarded as a relation which might exist among percepts and is itself perceptible.[16]

To our knowledge, Russell never discusses the puzzle in any of his later work. He seems to give up the idea that our knowledge of the physical world is purely structural, but there is no account of how, on his theory of knowledge (e.g., the theory developed in *Human Knowledge: Its Scope and Limits* [1948]), such non-structural knowledge can arise. Yet all the elements of the earlier and later theories are the same—the only difference is in the conclusion drawn. Thus either the original claim (that we are restricted to purely structural knowledge) was theoretically unmotivated or the argument of the later theory contains a lacuna. The difficulty in adjudicating between these alternatives is that the theoretical development is made to depend on what we regard as falling within "acquaintance." And this makes the resolution quite artificial: in the earlier theory we could not assume acquaintance with (what Maxwell used to call) a cross category notion such as spatiotemporal contiguity or causality, but in light of the difficulties of that theory we now find that we *can* assume this![17] We are not saying that one *cannot* resolve the issue in this way. But it seems quite clear that without a considerable advance in the theoretical articulation of this rather elusive Russellian concept, no such resolution of the difficulty can be very compelling.

III

There is another consequence that Newman draws, although it is much less explicit in the article. Russell's structuralism can be viewed as a theory of how the reference of the theoretical vocabulary is fixed. On this view the reference of a T-predicate is fixed by two things: (1) its connection with observation, and (2) its structural properties. As we have seen, the connection with observation fixes the domain (or at least, establishes that very many sets are *not* the domain of interpretation). Structure is supposed to complete the task while preserving the nonstipulative character of the truth of our theoretical knowledge. As we saw, Newman observes that within Russell's theory, there is no analogue for relations of the important/unimportant criterion for domains. From a contemporary, model-theoretic standpoint, this is just the problem of intended versus unintended interpretations: Newman shows that there is always some relation, R (on the intended domain) with structure W. But if the only constraints on something's being the intended referent of "R" are observational and structural constraints, no such criterion for distinguishing the intended referent of "R" can be given; so that the notion of an intended interpretation is, in Quine's phrase, provided by our background theory and hence cannot be a formal or structural notion in Russell's sense. Now, in fact, something strikingly similar to this argument has recently been rediscovered by Hilary Putman, and it has been used by him to pose a puzzle for certain "realist" views of reference and truth.

Putman's formulation of the argument is model theoretic and general rather than informal and illustrative: suppose we are given a theory T all of whose observational consequences are true. We assume, for the sake of the argument, that the observational consequences are true. We assume, for the sake of the argument, that the observational consequences of T may be characterized as a subset of the sentences generated from a given "O-vocabulary." It is assumed that the interpretation of the language $L(T)$ of T is specified for its O-vocabulary, so that T is "partially interpreted" in the sense that the interpretation function is a partial function. Assume that T is consistent and that M is an abstract model of the uninterpreted sentences of T of the same cardinality as T's intended domain. Following Putman we let "THE WORLD" denote the intended domain of T. Now extend the interpretation to the theoretical vocabulary of T by letting each predicate of the theoretical vocabulary of T denote the image in THE WORLD of its interpretation in m under any one-to-one correspondence between M and THE WORLD. Call this interpretation (which extends the partial interpretation) "SAT." Clearly SAT is completely arbitrary, and should be an utterly unacceptable extension since it trivializes the question whether T is true. Any theory of knowledge and reference that is incapable of rejecting SAT, i.e., any theory that is incapable of distinguishing truth from truth under SAT–TRUTH (SAT)–is committed to the implication, T is true if T is TRUE (SAT). But T is TRUE (SAT) as a matter of logic! That is, "*T* is TRUE (SAT)" follows, via the completeness theorem, from "*T* is consistent." So any theory that cannot exclude SAT as an intended interpretation of $L(T)$ cannot account for our naive confidence in the belief that our theories, if true, are "significantly true."[18].

Except for its metalogical flavor, this argument parallels Newman's. Where Putman argues from the consistency of a set of sentences, Newman argues directly from the existence of a relational structure satisfying the intuitive conditions of a model. (Recall that Newman was writing in 1928 and in the logicist tradition of Russell and Whitehead—a tradition to which the metalogical turn in mathematical logic was quite alien.)[19]

The chief difference between Putman and Newman is in the use made of the argument. Putman, as we have said, employs the argument against a certain form of realism—one which is rather difficult to isolate, although there are some suggestions. For example, Putman says it is that form of realism which holds that even an epistemically ideal theory might be false; or any view which regards truth as radically nonepistemic; or any theory of truth and reference which "treats language as a 'mirror of the world,' rather than one which supposes merely that 'speakers mirror the world'—i.e. their environment—in the sense of constructing a symbolic representation of that environment."[20] All these remarks, though certainly suggestive, are vague and elusive.

In a recent book, Putman expresses the significance of the result slightly differently.[21] There he characterizes the argument as supporting Quine's observation[22] that fixing the set of true observation sentences together with the set of accepted theoretical sentences will not determine the reference of the theoretical vocabulary. Putman shows that the indeterminacy affects the reference of the T-vocabulary in every possible world, and in this respect extends Quine's observation.[23] The formal connection with Newman's argument is this. Newman shows that fixing the domain and models up to isomorphism does not fix the intended reference of the *T*-vocabulary. Putman shows that fixing the domain and models up to elementary equivalence does not fix the intended reference of the *T*-vocabulary. (Thus formally Newman's argument is stronger than Putman's, since isomorphism strictly implies equivalence, although we do not wish to lay much stress on this fact.)

It may be that Newman's argument contains an important observation about realism in *general*—not merely about *structural* realism. The recent work of Putman, to which we have drawn attention, is intended to suggest that it does. But we have been unable to find or construct a clear statement of the connection. Certainly the argument poses difficulties for partial interpretation accounts of theories that exactly parallel the difficulties confronted by Russell's theory: both accounts of theoretical knowledge and reference fail to square with our naive beliefs regarding the nature of the truth of theoretical claims. The conclusion we have suggested is a conservative one: the argument shows that neither Russell nor the neopositivist doctrine of partial interpretation has gotten the analysis of theoretical knowledge and reference quite right. A satisfactory account of these notions must do justice to such "obvious facts" as that the world's structure is discovered rather than stipulated. We are skeptical of our ability to do this on any view of reference and truth that does not take the language of physical theory as the "ultimate parameter" within which reference is fixed.

IV

The difficulty Newman raises for Russell's use of the notion of structure can be clarified and deepened if we look at an analogous difficulty that arises for Carnap in the *Aufbau* and then take a brief glance at Wittgenstein's *Tractatus*. Newman's problem can be put this way. Russell wishes to turn theoretical terms into variables by Ramsification; accordingly, physics becomes the assertion that there *exist* properties and relations having certain logical features, satisfying certain implicit definitions. The problem is that this procedure trivializes physics: it threatens to turn the *empirical* claims of science into mere *mathematical* truths. More precisely, if our theory is consistent, and if all its purely observational consequences are true, then the truth of the Ramsey sentence *follows* as a theorem of set theory or second-order logic, provided our initial domain has the right cardinality – if it does not, then the consistency of our theory again implies the existence of a domain that does. Hence, the truth of physical theory reduces to the truth of its observational consequences, and as we saw earlier, Russell's realism collapses into a version of phenomenalism or strict empiricism after all: *all* theories with the same observational consequences will be equally true.

We have observed that Carnap attempts to go Russell one better in the Aufbau by turning *all* scientific terms into variables by means of purely structural definite descriptions. So Carnap's program is faced with a closely analogous difficulty, a difficulty he himself articulates with characteristic honesty and precision in §§153–155. Up to this point in the *Aufbau*, all scientific concepts have been introduced via structural definite descriptions containing the single nonlogical relation R_s of recollected similarity: the visual field, for example, is the unique five-dimensional "sense class" *based on* R_s. However, the goal of "complete formalization" – converting all definitions into *purely* structural definite descriptions – can only be achieved if the basic relation R_s is itself eliminated (§153). How is this to be done? Well, R_s also has a logical form or structure: it has a *graph* $\Gamma(R_s)$. For example, R_s is asymmetrical (§108), and there is one and only one "sense class" based on R_s that is five-dimensional (§§117–119). To be sure, we can know these formal properties of R_s only empirically, but once we know them, we can express them in a purely formal schema $\gamma(R_s)$, where R is now a *relation variable*. The idea, then, is to define R_s as the unique relation with this graph:

(3) $R_s = (\iota R)\Gamma(R)$.

However, as Carnap immediately points out, there is a serious problem with this procedure. Since $\Gamma(R)$ is now a purely formal schema containing no nonlogical primitives, the *uniqueness* claim implicit in (3) will never be satisfied. As long as the condition Γ is consistent, there will always be infinitely many distinct relations with this graph, even if we confine ourselves to relations on the field of R_s:

> Our assumption is justified only if the new relation extensions are not arbitrary, unconnected pair lists, but if we require of them that they correspond to some experienceable, "natural" relations (to give a preliminary, vague expression).
>
> If no such requirement is made, then there are certainly other relation extensions [besides R_s itself] for which all constructional formulas can be produced. . . . All we have to do is carry out a one-to-one transformation of the set of basic elements into itself and determine as the new basic relations those relation extensions whose inventory is the transformed inventory of the original basic relations. In this case, the new relation extensions have the same structure as the original ones (they are "isomorphic"). (154)

The analogy with the Newman problem is evident.

How does Carnap handle the problem? He introduces the notion of *foundedness* to characterize those relation extensions that are "experienceable and 'natural,' " and argues that one can view this notion as a primitive concept of logic:

> [Foundedness] does not belong to any definite extralogical object domain, as all other non-logical objects do. Our considerations concerning the characterization of the basic relations of a constructional system as founded relation extensions of a certain kind hold for every constructional system of every domain whatever. It is perhaps permissible, because of this generality, to envisage the concept of foundedness as a concept of logic and to introduce it, since it is undefinable, as a *basic concept of logic*. . . let us introduce the class of founded relation extensions as a basic concept of logic (logistic symbol: *found*) without considering the problem as already solved. (§154)

In other words, Carnap proposes to do precisely what we saw Newman recoil from: he wants to reckon "importance" (foundedness) as "among the prime unanalyzable qualities of the constituents of the world"! Once this is done, the problem of defining R_s is easily solved. We replace (3) with

(4) $R_s - (\iota \mathrm{R})(\mathit{found}(\mathrm{R})$ and $\Gamma(\mathrm{R}))$.

(See §155.) It is clear, however, that we must side with Newman here: the idea that (4) is a purely logical formula *is* absurd.

Actually, it is not entirely obvious why Carnap needs a definition like (3); it is not clear why he needs to make a *uniqueness* claim. He could express our empirical knowledge, as Russell does, with a purely existential claim

(5) $\exists R\, \Gamma(R)$,

and (5) fulfills the goal of "complete formalization" just as well as (3): both contain no nonlogical vocabulary. Of course (5) is vulnerable to the Newman objection in precisely its original form – it again threatens to turn empirical truth into mathematical truth – but let us slow down and reflect for a moment. For, when applied to our current physical theories, the assumptions needed to prove (5) turn out to be rather strong. In particular, we will need some version of the axiom of infinity and some version of the power set axiom applied to infinite sets (that is, we will need the continuum). Contemporary model theory for second-order logic makes both these assumptions, and that is why (5) is a *theorem*. Suppose, however, that we are transported back into the 1920s. The line between logic and mathematics is much less clear, and it is equally unclear whether strong existence assumptions like the axiom of infinity and the power set axiom are to be counted as part of logic *or* mathematics.[24] (In *Principia Mathematica*, for example, Russell carries along the axiom of infinity as an undischarged hypothesis.) Accordingly, the status of (5) is much less clear-cut.

To see some of the issues here, consider the strictest and most rigorous version of logicism devised in this period, namely, Wittgenstein's *Tractatus* (1921). The *Tractatus* is clear and emphatic about the axiom of infinity and set theory: neither is part of logic (5.535, 6.031). In fact, logic makes no cardinality claims whatsoever: there are no privileged numbers (5.453). By the same token, however, such cardinality claims are not part of mathematics either, for mathematics is a "logical method" involving only the simplest forms of combinatorial equations (6.2–6.241). Although Wittgenstein is not very explicit in the *Tractatus* about the precise scope of mathematics, it is apparently exhausted by a rather small fragment of elementary arithmetic. On this kind of conception, then, formulas like (5) will *not* be theorems of logic or mathematics; if true at all they can only be empirically (synthetically) true. Hence, on a Tractarian conception of logic and mathematics, we could perhaps make sense of Carnap's program of "complete formalization" after all. And, for all we know, Wittgenstein could have had something very much like this in mind. (See *Tractatus* 5.526–5.526, for example, where Wittgenstein says that we can describe the world completely by means of *fully generalized* propositions. See also 6.343–6.3432.) In any case, whether or not these considerations have any connection with Wittgenstein's intentions in the *Tractatus*, they do illustrate some of the intellectual tensions produced by logicism's attempt to account simultaneously for both pure mathematics and applied mathematics (mathematical physics). In general, it appears that we can account for the distinctive character of one only at the expense of the other. This is perhaps the final lesson of the Newman problem.

Notes

1. Cf. J. Winnie, "The Causal Theory of Space-Time," *Minnesota Studies in the Philosophy of Science*, Vol. VIII (Minneapolis: University of Minnesota Press, 1977), pp. 156ff. for a discussion of Russell's remarks on Minkowski space-time, and their relation to Robb's theory.

2. See *Analysis of Mind*, specially pp. 125ff.

3. See especially "Scientific Methodology and the Causal Theory of Perception," in I. Lakatos and A. Musgrave (eds.), *Problems in the Philosophy of Science* (Amsterdam: North Holland, 1968), and "Structural Realism and the Meaning of Theoretical Terms," *Minnesota Studies in the Philosophy of Science*, Vol. IV (Minneapolis: University of Minnesota Press, 1971).

4. As articulated in "The Relation of Sense-Data to Physics," *Scientia* (1914) and *Our Knowledge of the External World* (1914).

5. Notice that whether the terms are implicitly or explicitly definable depends on the logical framework within which the definitions are to be constructed. Is (1) a first-order theory with identity? An extension of first-order set theory? A formulation in some higher-order logic? These differences matter because, if one is working in a sufficiently strong background language, one can often *transform* axiomatic or implicit definitions into explicit definitions (cf. Quine, "Implicit Definition Sustained," *Journal of Philosophy*, 61 [1964], pp. 71–73, for one such strategy). Of course this way of expressing the issue is anachronistic, since these distinctions were not available to Russell. Consequently the distinction between implicit and explicit definition is not so clearcut for Russell. Thus, for example, Russell appears willing to allow implicit definitions even in 1914 when, in section IX of "The Relation of Sense-Data to Physics," he defines physical things as "those series of appearances whose matter obeys the laws of physics."

6. *The Analysis of Matter* (1927), pp. 216f.

7. Ibid., pp. 210–13.

8. Ibid., p. 254.

9. Ibid., p. 270. See also Chapter 14, especially the discussion of Eddington on pp. 136f.

10. I.e., one finds the form/content distinction drawn in such a way that form is objective and communicable, whereas content is neither. Schlick came to view the form/content distinction in much the same way in his later writings, while Russell came to place much less stress on this (somewhat romantic) view of the distinction. (For Schlick's view see "Experience, Cognition and Metaphysics" [1926] and *Lectures on Form and Content* [1932], in vol. 2 of his *Collected Papers* [Dordrecht: Reidel, 1979].) Schlick explicitly acknowledges the influence of the *Aufbau* and of Wittgenstein's *Tractatus*.

11. *Aufbau*, §16. In the explanatory references to this section, Carnap refers to the passage from Russell's *Introduction to Mathematical Philosophy* quoted earlier. Immediately before this section, he refers to Schlick's doctrine of implicit definition in *General Theory of Knowledge*.

12. As Carnap himself emphasized, this kind of program for individuating concepts by means of purely formal properties only begins to make sense in the context of modern, *polyadic* logic. Monadic concepts correspond to unstructured sets whose only formal property is their cardinality. Polyadic concepts have such diverse formal properties as transitivity, reflexivity, connectedness, dimensionality, and so on. In Russell's terminology from *Introduction to Mathematical Philosophy* and *The Analysis of Matter*, only polyadic concepts have "relation-numbers."

13. Newman, "Mr. Russell's Causal Theory of perception," *Mind* (1928), p. 143. All references to Newman are to this article.

14. Quine, "Implicit Definition Sustained."

15. John A. Winnie, "The Implicit Definition of Theoretical Terms," *British Journal for the Philosophy of Science*, 18 (1967), theorem 2, pp. 227ff.

16. *The Autobiography of Bertrand Russell, Volume 2* (London: Allen & Unwin, 1968), p. 176.

17. Maxwell opted for this solution. (See the papers cited in note 3.) Unlike Russell, Maxwell was quite explicit about the nature of the problem.

18. John Winnie, in the paper cited in note 15, employs an argument of this form in the proof of his theorem 1. Winnie's discussion focuses on formal features of the puzzle: that there is not a unique interpretation of the theory and that there is always an arithmetical interpretation. Newman and (we believe) Putman emphasize a different point, viz., that *mere consistency* seems to be sufficient

for *truth*; i.e. since T has an arithmetical interpretation it has a true interpretation *in the world* (i.e., in THE WORLD).

19. See Warren Goldfarb, "Logic in the Twenties: The Nature of the Quantifier," *Journal of Symbolic Logic*, 44 (1979), pp. 351–68.

20. Putnam, *Meaning and the Moral Sciences* (Boston: Routledge & Kegan Paul, 1978), p. 123. The other suggestions may also be found in this chapter, which is a reprint of Putman's 1978 APA presidential address.

21. *Reason, Truth and History* (Cambridge: Cambridge University Press, 1981), pp. 33f.

22. W. V. Quine, "Ontological Relativity," in *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969).

23. See the appendix to *Reason, Truth and History*.

24. Carnap, for one, seems remarkably sanguine about this. He is apparently willing to count both *AxInf* and *PowerSet* as logical. In §125, for example, he introduces and n-dimensional real number space with a completely innocent air. Apparently, then, he is willing to count virtually all of set theory as logic (see also §107). This is why (5) could not possibly serve as an expression of our *empirical* knowledge of Carnap.