Structure and Perspective: Philosophical Perplexity and Paradox

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My topic today is structuralism in the philosophy of science. The name "structuralism" is often used in a narrow sense to denote specific programs, such as Bourbaki's in mathematics and the approach to science due to Sneed and Stegmueller. I will use it in a broad sense, for any of a variety of views to the effect that science describes only structure, that scientifif theories give us information only about the structure of processes in nature, or even that all we can know is structure. Since the very term "structuralism" comes from a by now fragile traditional distinction between form and structure, I will focus on the precise formulation of such views, with little regard to whether they honor that putative distinction. I shall describe some of the history of the general idea and its problems, and then ask what form of structuralism could be viable today.

1. Science as representation

The prehistory of structuralist views is found, it seems to me, in the nineteenth century discussions of the extent and manner in which science represents natural phenomena: selectively, as representation must always be, yet accurately to some 'relevant' extent.

1.1 The philosophical consensus ca. 1900

At the end of the 19th century, the common view of science was that science is representation. Helmholtz, Hertz, Boltzmann, Mach, and Duhem are now all often seen as forerunners of anti-realism about science. It does not surprise us that they speak of 'mere' represen tation, 'thought-pictures', vindicated only by their role in the classification of observable phenomena. But their main opponent Planck, who espoused an explicitly realist view in his controversy with Mach, wrote in the same terms.

Planck began by insisting that unlike 'ordinary' representations such as drawings or paintings, scientific theories or models must "correspond . . . literally in all respects with" their subjects (quoted Blackmore p. 127).

Why does Planck not say simply that science must be accurate and true? Silence is often the tacit admission of a problem, and so it is here. When science is admitted to be viewable as giving us pictures, likenesses, models (in the sense of similarities or analogies to familiar processes), the verbal concession lets in a problem. For if two things are independently known and independently described, then it makes sense to claim that they are like or unlike in some respect, and the claim can be evaluated. But if only one is described, what is meant by a bare claim of similarity? How can that be non-trivial or informative -- how could it even be true or false?

As example, take the anti-realist sounding dictum: "there is nothing to Maxwell's theory of electro-magnetism except Maxwell's equations." The realist wants to add something. The equations in question resemble those which describe certain familiar processes involving heat, sound, and motion in a fluid. The scientific realist wishes to add that the theory says that this resemblance is no accident, that electromagnetic phenomena are due to real waves in a real material medium. What is meant by this addition ? With no independent description available of those waves and that medium, claims that go beyond the noted resemblance (namely that Maxwell's equations have a certain formal character) cannot be evaluated. But in that case, after all, there is nothing more to the theory than those equations -- is there?

What Planck clearly had to do was to confront the question of what representation is, i. e. the conditions under which a theoretical model does in fact represent some part or aspect of nature. But he was so little able to give content or depth to his insistence on literal correspondence that he retreated to a neo-Kantian idealism. First he gives a list of famous scientists who were all, according to him, staunch realists. But then, to head off the philosophical problem of giving this realism some substance, he says:

> Is there any knowable [*erkennbarer*] difference between their world and our "world picture of the future"? Surely not. For there is not even a method to let us examine such a difference, [a thought] which through Immanuel Kant has become the common property of all thinkers. (tr. in Blackmore p. 132)

Can realists do better? I will leave this aside for now, to address the prior question: why was there this consensus, this agreement to talk about science as representation, and just what did this consensus signify?

We need to read between the lines of history here. I think this consensus resulted from reflections on geometry, which forced scientists to become conscious of the intellectual medium in which they do their creative work.

1.2 The problem of multiple geometries

The story of how geometry, and indeed mathematics over all, was revolutionized in the 19th century is a familiar one. Can we tell it as a prehistory of structuralism in the philosophy of science?

When non-Euclidean geometries were first created, the main reaction was that to understand them they had to be interpreted within Euclidean geometry -- that is, as strangely worded descriptions of parts or aspects of Euclidean space. This was done initially by Beltrami in his "Saggio di Interpretatione della Geometria Non--Euclidia" (1868) for hyperbolic geometry.

Note that this was interpretation in the strictest, most minimal sense. Beltrami himself expressed his goal as maintaining Euclidean geometry as the one true theory of space (cf. Coffa p. 9). Felix Klein perceived the limits of this effort and first attempted to interpret both Euclidean and non-Euclidean geometries within projective geometry. But very soon through Klein's work, and then even more radically through Riemann's, there came into being such a cornucopia of geometries that any true theory of space underlying all of them could be very little more than pure logic.

The mathematicians' response was that none were privileged, all geometries were on a par and certain to remain parts of mathematics. What was the philosophers' reaction? Everyone today knows of the reaction of Poincaré, who was both scientist and philosopher: he embraced the mathematicians' liberalism and held that physics could in principle be written in the language provided by any geometry. Not so well remembered is the reactionary philosophical response of two of our other heroes, Frege and Russell. For reasons which I'll explain later, I will call their view 'real property' realism.

Both Frege and Russell saw what had happened as a sort of betrayal: geometry had forsaken the task of describing space, and turned to exercises in pure logic. In response to Klein's initial ranging of Euclidean and non-Euclidean geometries under projective geometry, Russell held that there must be a unique real relation of congruence. Geometry, at least the geometry used in physics, must be a non-vacuous theory of real spatial relations.

But how are those real relations identified? We are directly acquainted with them, through intuition. When pressed to elaborate on this by Poincaré, Russell dismissed the question. That is, he said, like asking me to spell the letter 'A' (cited Coffa, pp. 20-21).

Frege elaborated his similar response in a controversy with Hilbert. The theory of space must be non-vacuous and true. But the question of truth can't arise unless the primitive terms have an independent meaning which fixes their reference. They certainly cannot get that meaning from the axioms or theorems, as Hilbert asserted. For those axioms and theorems are incapable of having a truth-value unless their terms have referents. Pressed to explain how we identify those real spatial relations, Frege also retreated to intuition or direct acquaintance -- just the sort of thing which neither Poincaré nor Hilbert professed to understand.

Now it was not very much later that Russell appeared to change his mind, and began to develop a structuralist view. Before explaining that, we should ask why 'real property' realism had so little appeal at the time. The answer is exactly what usually precipitates relativism -- namely, that realism about geometry precipitated objectively unanswerable questions, questions that seemed to make a mockery of the subject altogether.

1.3 Relativity of representation

As mentioned, Beltrami had interpreted hyperbolic geometry within Euclidean geometry. In view of this he thought to have justified Lobachevskii's non-Euclidean geometry by identifying "a real substra tum for that doctrine, instead of admitting the necessity of a new order of entities and concepts", and thus leaving Euclidean geometry as the true doctrine of space.

If this sort of interpretation could proceed only in a single direction, it would indeed have established an important asymmetry in the representation of nature. But that was a big "if", and it is not how things turned out.

The point was graphically illustrated by Helmholtz. His example concerns two geometries which result from a common basis by distinct choices of a metric, as in Klein's initial results. Helmholtz imagines that we are making measurements to determine whether our space is Euclidean. Do the interior angles of a triangle add up to 180 degrees? Suppose they do. At the same time he imagines that we are reflected in a huge concave mirror. In that mirror we see little people moving around with rulers and 'doing the same thing' as we do. Of course, they get the 'same' results, and announce that they live in a Euclidean space.

We want to disagree; they are moving around on a concave surface, and their measuring rods change length as they work. They, on the other hand, could say something similar to us! The fact is that there is a simple function which establishes a correspondence between points in their space and in ours. It carries our geometric planar structure over to their surface. What is the true geometric description is relative to the choice of which measuring rods shall be regarded as remaining of the same length. But with that choice brought to light, both descriptions are adequate for each of the two spaces.

The isomorphism defeats any attempt to correct the mirror people's judgment within geometric language. Now a realist like Frege or the early Russell can insist that one description is true and the other false. But we can retort that whichever alternative is chosen, the other will be true under proper interpretation, and in any case just as useful. Nor will there be any experiments or empirical predictions which will rule out one in favor of the other. Frege and Russell may insist that there are real spatial relations, which are the semantic values of terms in one geometric language and not in the other. But the question 'Which ones?' (i. e. the challenge to identify those relations and single them out from among all sets of pairs, triples, and so on) is beyond human ken, and will be one of those bits of ontological fact that makes no difference to science at all.

This is an uncomfortable ending for the realist story. It leaves one with that single consolation all philosophers have, of consistency and logical invulnerability; but that is cold comfort indeed. Thus it will not surprise us that Russell wished to move to a more comfortable position -- and structuralism is the position to which he moved.

2. Structuralism, its initial (mis)fortunes

The history of the structuralist idea in 20th century philosophy of science is a complicated story of many characters and many adventures. I shall here focus on Russell's very moderate structuralism and its collapse, partly because it was moderate but mainly because it gave us a very precise formulation of such a view. As has been pointed out repeatedly in the literature, the argument precipi tating its collapse is related to Putnam's more recent 'model-theoretic argument' against metaphysical realism. Some see a paradox there, inescapable except by realism -- but I shall suggest that the paradox can be dissolved. However, this will still leave us with the question whether structuralism can survive in some contemporary form.

2.1 Russell's structuralist turn

It is really no wonder that Russell began to move in the direction of a less stringent representationalism for science. When physics describes the world, how much does it describe? Perhaps there are real spatial relations and gerry-mandered, artificial metrics. Which of them are denoted by the geometric language used in physics seems not to matter, in principle anyway. But then, better to say, perhaps, that science does not describe nature down to that level of detail. Better to say, perhaps, that science describes only structure without content.

We see Russell taking this line, though not explicitly with reference to geometry, in his *Problems of Philosophy* (1912). He insists, as before, that to understand a proposition (so that it may be capable of being judged true or false) we must be acquainted with every item in it. This includes concrete individual entities, but also the properties and relations expressed by its predicates. However, we are acquainted only with those things which are part of our direct experience. Everything else, including material bodies, belongs to the External World. Unfortunately, it is exactly that External World which science purports to describe. How is this possible?

Russell could have said that science describes nature simply by saying that there exist entities, with which we are not acquainted, but which have the same properties and stand in the same relations, as enter our direct experience. But he does not say that, for science had moved too far and too fast for this to be plausible. Every analogy with familiar things, like waves in water, planets, and billiard balls was already heavy with disanalogies. So Russell says: we can only infer the properties of the properties, and the properties of the relations -- the type of structure:

> We can know the properties of the relations required to preserve the correspondence with sense data, but we cannot know the nature of the terms between which the relations hold.
>
> [...] although the relations of physical objects have all sorts of knowable properties, ... the physical objects themselves remain unknown in their intrinsic nature ...." (Russell 1912, pp. 32, 34)

When he makes this precise in *The Analysis of Matter* (1927), he says explicitly that this structure is exactly, no more and no less, what can be described entirely in terms of mathematical logic. The logic in question is strong, and today we would say higher order logic or set theory. But still note how little this is! Science is now interpreted as saying that the entities stand in relations which are transitive, reflexive, etc. but as giving no further clue as to what those relations are.

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

> The only legitimate attitude about the physical world seems to be one of complete agnosticism as regards all but its mathemati cal properties. (Russell 1927, pp. 254, 270)

Russell's younger contemporary Ramsey made explicit what this amounts to if a theory is formalized. On the face of it, the theory does describe all sorts of entities in terms of unfamiliar properties and relations. But really the physicist who says "atom", "electron", "ether", "charge" and "spin" is like the mathematician who says "there must be a fixed point, call it x". The only difference is that "atom" and "spin" are more mnemonic than "x" and "y". So the theory, formally stated, says only that there exist properties and relations instantiating a certain higher-order structure. That formal statement is generally called the theory's Ramsey sentence.

For a quick if simplistic example, consider the theory axiom-atized by "Water is composed of hydrogen atoms and oxygen atoms". Its Ramsey sentence is "There exist three properties, of which the first and second are mutually exclusive, such that water is composed of things which have the first and third property and things which have the second and third property." Even adding in the initially tacit logical relationship between "hydrogen" and "oxygen" does not seem to make the Ramsey sentence very informative, and this impression obstinately remains in less simplistic examples. Is the impression correct or mistaken?

2.2 The collapse of structuralism?

The mathematician M.H.A. Newman made the crucial critical point in a review article concerning *The Analysis of Matter*:

> ... it is meaningless to speak of the structure of a merecollection of things . . . . Further, no important information about the aggregate A, except its cardinal number, is contained in the statement that there exists a system of relations, with A as field, whose structure is an assigned one. For given any aggregate A, a system of relations between its members can be found having any assigned structure compatible with the cardinal number of A. (Newman p. 140)

The importance of this article, its devastating import, and its relation to more recent discussions of realism, was pointed out by Demopoulos and Friedman.

We can put Newman's point in terms of models and equations quite easily. Suppose Maxwell's equations have a model in which there are N distinct entities. Choose a set of the same cardinality in the world. Because same cardinality implies the existence of a correspondence, we have an implicit transfer of the relations in the model to that chosen set. Therefore the world satisfies Maxwell's equations! The reasoning is more abstract, but the point is no different from that of Helmholtz' concave mirror. But for Russell's view of science it has a corollary: if that view is correct, it seems that we can assert Maxwell's theory of electromagnetism to be true practically without engaging in empirical investigation.

Russell capitulated. In a letter to Newman, he reverted to the 'real property' realism of his early days. The only difference is an update from real spatial relations to real spatio-temporal relations:

> Dear Newman, [...] 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 spatio- 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 co-punctuality I regarded as a relation which might exist among percepts and is itself perceptible. (Russell 1968, p. 176)

Russell's response clearly re-introduces his earlier 'real property' realism, slightly updated, with the claim that we are directly acquainted with the crucial spatio-temporal -- rather than spatial -- relations. Science does not merely assert that there are relations which have the requisite formal character, but gives information about those very relations themselves.

Newman had suggested that Russell could repair his position in this way, by distinguishing between 'important' and 'unimportant' relations. The structuralism would of course have to be restricted to the 'unimportant' part! The effect of the modification would be to charge the transfer of the model's structure, which "reads it into" the world, with mistakenly treating arbitrary sets as real properties and relations. A realist can object to that. Russell accepted this suggestion, adding that the 'important' relations are exactly those of which we have direct acquaintance, and postulating that they include enough relations to make his construal of science a success. Newman had also derided his own suggestion as a counsel of despair, and in fact Russell appears to have had nothing to present by way of support for his postulate.

The form of answer Newman suggested could of course be left more abstract, as purely ontological distinction. If only certain relations are important and quantification in the Ramsey sentence is restricted to those, then that sentence is no longer vacuously true. This was the escape Russell utilized; but one could take it without adding some criterion, like direct acquaintance, to single out the membership of the domain of quantification. David Lewis took this course. The theory's Ramsey sentence says that the relational structure described by the theory is instantiated. Lewis suggested that we identify the content of the theory with something somewhat stronger, namely the assertion that the described structure is uniquely instantiated. This is clearly still a purely structural assertion.

If the quantifiers in that assertion range over all classes (i.e. relations extensionally construed as is usual in mathematics) then Newman's argument establishes that, while the Ramsey sentence will be almost always true, Lewis' strengthening will be almost always false. But if the quantifiers are restricted in the way Newman suggested, then those conclusions cannot be drawn. Lewis too accepts Newman's suggestion, in effect, with his distinction of 'natural' versus 'arbitrary' classes. The natural classes are the extensions of the real properties and relations; the structure described by physics is uniquely instantiated among them if and only if physics is successful. But which classes are natural? Well, if we assume that physics is successful then they are such as to yield a unique instantiation of the structures described in physics -- and that, I suppose, is pretty well all we can know about them, even on that assumption. Is this still structuralism? No, it is not, since among the 'arbitrary' classes there will be many relational systems isomorphic to those which are constituted by the 'natural' classes. In other words, the 'important' relational systems in the world, those which successful science describes, are not singled out through structure alone.

2.3 Putnam's model-theoretic argument -- a paradox?

It is now (anno 1995) already twenty years ago that Putnam presented his model-theoretic argument against metaphysical realism, but there is no consensus on what it establishes.[1] Various writers have pointed out connections between this argument and Newman's objection to Russell. You can see for yourself. Here is the argument as presented in 1976:

> So let T1 be an ideal theory, by our lights. Lifting restric tions to our actual all-too-finite powers, we can imagine T1 to have every property except objective truth -- which is left open -- that we like. E. g. T1 can be imagined complete, consistent, to predict correctly all observation sentences (as far as we can tell), to meet whatever 'operational constraints' there are . . . , to be 'beautiful', 'simple', 'plausible', etc. . . .
>
> [...] I imagine that THE WORLD has (or can be broken into) infinitely many pieces. I also assume T1 says there are infinitely many things (so in this respect T1 is 'objectively right' about THE WORLD). Now T1 is consistent . . . and has (only) infinite models. So by the completeness theorem . . . , T1 has a model of every infinite cardinality. Pick a model M of the same cardinality as THE WORLD. Map the individuals of M one-to-one into the pieces of THE WORLD, and use the mapping to define relations of M directly in THE WORLD. The result is a satisfac tion relation SAT -- a 'correspondence' between the terms of [the language] L and sets of pieces of THE WORLD -- such that theory T1 comes out true -- true of THE WORLD -- provided we just interpret 'true' as TRUE (SAT). So whatever becomes of the claim that even the ideal theory T1 might really be false? (Putnam 1978, pp. 125-126; italics omitted)

I display the original text, for two reasons. The first is that it is couched in terms of the so-called syntactic, axiomatic view of scientific theories. It relies on identification of a theory as a set of sentences in a vocabulary which is divided into 'observation terms' and 'theoretical terms'. That aligns Putnam's argument with the Ramsey sentence formulation of Russell's structuralism -- and may in the end also be our main clue for how we can move philosophy of science out of this so very arid puzzle area.

The second and more important reason is to highlight the anthropomorphic way of talking about mathematical entities. Putnam commands us: "Pick a model M . . . Map the individuals of M one-to-one into the pieces of THE WORLD". That way of speaking, as if we humans are actually carrying out specific tasks, may be harmless when we are doing pure classical mathematics, but is it really so harmless here?

What he commands us to do here may or may not be possible. He has said that model M and THE WORLD have the same cardinality. Therefore there certainly exists a one-to-one map between them - indeed, very many such maps exist. But can we identify or pick one of them?

Here is an analogous example. Consider a geometric object, a sphere in Euclidean space. Can we coordinatize the surface of this sphere? Our first inclination is to say Yes; for their certainly exist many functions that map the surface points into triples of real numbers in the right way. But I asked: can WE do this? The answer is No, for this object has perfect symmetry. We would like to give one point the coordinates (0,1,0) and call it the North Pole. But this point is not distinguished from any other on the sphere, so we cannot do it. Of course, if the sphere is not a mathematical object but, say, the Earth, then we can do it. The reason is that we can independently identify a spot to designate as North Pole. Similarly, we may be able to do it if the sphere is already related to some other mathematical object, has some functions defined it, and so on -- for the same reason. But we cannot do it unless we have some independent way to describe points on that sphere.

Does this distinction -- between the existence of a function and our being able to carry out the mapping -- matter here? It does indeed if Putnam's argument is meant to apply to real science formulated in real language. We shall be able to grasp that theory, on Putnam's implicit view of understanding language, if we can grasp even one interpretation of the sort he describes. But whether we can do that depends on whether or not we can independently describe THE WORLD.

With this clarification in hand, we are ready to answer Putnam. To Putnam we must say:

(A) if we cannot describe the elements of THE WORLD, neither can we describe/define/identify any function that assigns extensions to our predicates in THE WORLD;

(B) if we can describe those elements then we can also distin guish between right and wrong assignments of extensions to our predicates in THE WORLD.

Indeed, if we can describe those elements we can say that an interpre tation, if applied to our language, is wrong unless it assigns the set of cats to the words "cat", "gatto", "kat" etc. We learned more from Tarski than Putnam lets on here! [2]

What I have just done is not to solve Putnam's paradox, but to dissolve it. But it went a little fast, so I suppose I had better explain just what I did and what I assumed.

What I did was to read Putnam's argument as applying to real scientific theories formulated in OUR language, the very language in which we state his argument. If we read his argument as applying to an arbitrary given language, it may be objective and universal in form, but it also restricts itself to semantics, and abstracts from all pragmatic aspects. In that case, if we try to draw consequences for our own case, we deal with our language as if it were an alien syntax. If we do so and then try to discuss truth, we can indeed get no further than truth-relative-to some interpretation. But the situation changes entirely if we understand the argument throughout as concerning our language, in which we state it, and on which we rely implicitly.

In that case, the discussion of truth is not elliptic, and 'true' does not mean 'true relative to some interpretation'. But while "true" is then not elliptic, it is indexical -- tacitly indexical. The tacit indexical reference is to our language. The criteria for proper understanding of our own language express themselves in pragmatic tautologies. Consider the sentence:

"cat" denotes cats.

This is undeniable by me if I acknowledge "cat" to be a word in my language. The semantic content of this (to me undeniable) assertion is however not a necessary proposition (for if our language had developed differently in a certain way then "cat" would have denoted gnats, rats, or bats). The example belongs to the class highlighted by Moore's paradox ("It is snowing in Peru but I do not believe that it is snowing in Peru", which is often true, so its semantic content is a contingent proposition; yet I cannot coherently and truly assert it).

It is only if we lose sight of this pragmatic aspect of the argument that we are led into the fallacy of thinking that there is a gap to be filled by metaphysics. For then the adequacy of a theory seems to have to derive in part from the adequacy of the language in which it is stated, which must then seemingly derive from something that makes our language especially blessed among languages or privileged in some objective, use-independent way. The road into this fallacy is clear, and it is quite clear how the fallacy engenders metaphysics.

Even if we see through this, there are problems left for us of course -- for example, to explain how such pragmatic tautologies can be combined with coherent admissions of our fallibility. It is not easy to state such admissions coherently. That is a common problem with pragmatic tautologies: once again, exactly the problem highlighted by Moore's paradox. I will assume that we can solve that without creating more ontological debris. In any case, these problems are not peculiar to our subject matter here.

So I have asserted that there is a paradox only if we try to treat a problem in pragmatics on the level of semantics, i. e. by illegitimate abstraction from pragmatic aspects. The dissolution of Putnam's paradox is not a solution based on a specific ontology. Does it still belong to some sort of philosophical position? If so, it is not to be called absolute relativism (to coin a phrase), since it does not refuse to acknowledge any and all sorts of distinctions among languages. But still one might call it a sort of relativism, or of perpectivism perhaps, because the privilege it notes is the historically conditioned, contingent status of being our own language. This privilege obviously bestows no security and no guarantee of adequacy; it cannot function as foundation. So perhaps "anti-foundationalism" is the best label; though best of all, I think, would be to discard such labels altogether.

If a paradox is dissolved, it does not need a solution. The large question that remains for us now is: did we only dissolve a problem peculiar to the syntactic/axiomatic view of theories? Does Newman's problem persist, so as to defeat any other form of structuralism, mutatis mutandis? Putnam in effect put the problem raised by Helmholtz and Newman in explicitly linguistic form -- and my dissolution pertained to it in that form. Will the problem just re-emerge if we leave this (in my opinion, overly) linguistic approach behind?

3. Structuralism: the semantic approach

In the second half of this century an alternative approach has developed within philosophy of science: the so-called semantic approach or semantic view of theories. Its antecedents go back fifty odd years to Herman Weyl and Evert Beth, as well as to work in the fifties and sixties by Patrick Suppes, and in fact to a large diversity of related work by philosophers and scientists in Poland, Italy, Germany, and other countries. After that initial period we have seen two distinct developments, on the one hand by Fred Suppe, Ronald Giere, myself, and others, and on the other hand, by Sneed, Stegmueller, Moulines, and others. The latter group actually uses the name "Structuralism". Finally, and this is very heartening, during the last ten years a new group has drawn on all these developments and taken them further. I am referring to Da Costa and French and their collaborators.

By common, if often tacit, consent among all these rather disparate writers, the semantic approach is the current form of the general idea of structuralism. Let us first of all examine why.

3.1 Structural description of nature

According to the semantic approach, to present a scientific theory is, in the first instance, to present a family of models - that is, mathematical structures offered for the representation of the theory's subject matter. Within mathematics, isomorphic objects are not relevantly different; so it is especially appropriate to refer to mathematical objects as "structures". Given that the models used in science are mathematical objects, therefore, scientific theoretical descriptions are structural; they do not "cut through" isomorphism. So the semantic approach implies a structuralist position: science's description of its subject matter is solely of structure.

A fine point is in order here, and must be noted. If one structure can represent the phenomena, then so can any isomorphic structure, mutatis mutandis. This last clause is important. We can represent temporal sequences by means of the less-than-or-equal relation on the real numbers -- as we do in fact in the calendar -- to take one example. Now the greater-than-or-equal relation, i.e. the converse of the former, is isomorphic. So it too can be used to represent temporal sequences of events (we could use a reverse order calendar, for instance by multiplying our dates by -1). But the use must be 'mutatis mutandis': this point about isomorphism certainly does not mean that if A precedes B then we can also say, without furthermore, that B precedes A!

Properly understood, it is entirely correct to say that models represent nature only up to isomorphism -- they only represent structure. Therefore, it really is a consequence of the semantic view that science describes only structure. But what is the import of that fine point about representation by distinct but isomorphic structures being "mutatis mutandis"? We distinguish the two relations of less and greater -- or more generally two isomorphic models -- by noting their relations to each other in a larger structure in which they appear, while calling them isomorphic when considering each solely by itself. What this brings out very graphically is that representation is using (something) to represent (something). There is no such thing as 'representation in nature' or 'representation tout court'; the question whether one given object is a representation of another is an incomplete question. Specifically, in science models are used to represent nature, used by us, and of the many possible ways to use them, the actual way matters and fixes the relevant relation between model and nature -- relevant, that is, to the evaluation as well as application of that theory.

This point, though somewhat banal at first sight, leads to a qualification of the structuralism here noted; a qualification, but not, I shall insist, a weakening. I will have to discuss this issue in two stages. Philosophers have in the past wanted to ask immediately, "But is the theory true?", "Under what conditions is a theory true?" and so on. The elaboration of the semantic view has, however, mainly concentrated not on truth in toto, but on some lesser relation of greater practical interest. We must discuss both.

3.2 Adequacy to the phenomena

Within the semantic approach, attention has focused not so much on truth per se, truth in toto, but on lesser relations of adequacy. Polish and Italian writers used terms like "empirical truth"; I have used the term "empirical adequacy"; Da Costa and French use "partial truth". These terms are not synonymous but closely related, though the guiding idea is that of adequacy in respects which can be made subject of practical investigation.

Patrick Suppes had long emphasized that theories do not confront the data bare and raw. The experimental report is already a selective and refined representation, a 'data model' as he calls it. This is especially true today, as Fred Suppe has emphasized, now that scientists routinely process gigabytes of data. It was already true in Newton's time when he claimed to deduce laws from the phenomena -for of course he used as basis very smooth functions distilled from thousands of astronomical observations. But it is true even of the idealized, simple observation report discussed by the logical positivists, as they themselves came to agree after some debate.

When we claim adequacy of a theory to the phenomena, that is adequacy to the phenomena as thus described. Note well that this claim is not restricted to actual data models or actual descriptions issuing from observation. It makes sense to claim that a certain graph depicts fluctuations in the dinosaur population, which was certainly never observed. That claim is true or false, whether we know it or not. The theory's adequacy requires that the correct graph -- whichever it be -- matches a theoretical model. More generally then, those structures which represent phenomena in terms of the relevant parameters must "fit" a theoretical model -- that is what constitutes adequacy with respect to the phenomena.

Obviously this is a fallible process, and not only fallible but history and theory conditioned. The phenomena are in principle observable by anyone, but the form of description is chosen, taught, and learned by human beings who are thoroughly immersed in their inherited background theories, opinions, and assumptions. That there is danger of failure inherent in this situation goes without saying, and I won't say it again.

The important point for us here is that this claim of adequacy too is in the first instance a structural claim. To be matched are two models, a data model and a theoretical model. The matching in question may be as simple as an embedding or partial isomorphism, or it may need to be some measure-theoretic refinement thereof to allow for approximation. But it is in any case a mathematical relationship, and therefore purely structural. The claim of adequacy is in the first instance a claim about how two structures are structurally related.

Do we have Newman's problem here? Let us take an example. The same exponential curve might be the shape of two data models, one from bacterial population growth and the other from radio-active decay. But data from radium samples are not relevant to a theory about bacteria. It is simply not relevant that a data model obtained in studies of radio-activity is thus structurally related to the bacterial population model, even if in this respect it is not different from what was found in bacteriological experiments.

Thus, something more about the phenomena, besides their struc ture, matters to the claim of adequacy for the theory. The data model is important not in itself, but in its role of representation of the phenomena. As with Putnam's paradox, we must insist that this role does not consist simply in having a certain structure. The claim of adequacy is with respect to the structure of real phenomena described in terms of the relevant parameters of the theory.

3.3 Structuralism lost after all?

This last point may sound like an admission of defeat. Is structuralism lost here, and must it give way to 'real property' realism? I do not think so. One way to state the challenge is to say:

> "Oh, so the theory does not confront the observable phenomena, (those things, events, and processes out there) in and by themselves, but only certain descriptions of them. Empirical adequacy is not adequacy to the phenomena pure and simple, but to the phenomena as described!"

There is something wrong with this objection, once again an error which cannot show up if we keep our discussion on the level of semantics.

On the one hand: the claim that the theory is adequate to the phenomena is not the same as the claim that it is adequate to the phenomena as described by someone, nor by everyone, or anyone. On the other hand: if we try to check a claim of adequacy, then we will compare one representation or description with another -- namely, the theoretical model and the data model. The tension between these two obvious points can quickly induce the temptation to think that there must be a specially right, objectively right, or naturally privileged description. This privilege will then be a great mystery, begging for special ontological status.

But if we put ourselves into the picture -- us, the users of the theories, the ones who use mathematical structures to represent phenomena -- then the picture changes radically. For us the claim

(A) that the theory is adequate to the phenomena

and the claim

(B) that it is adequate to the phenomena as described, i.e. as described BY US,

are indeed the same! That is a pragmatic tautology. It is a truism of the rank of such sayings as that any description we give, we give in our language in use, that we can't describe without using language or some equivalent medium of representation. From a tautology, even a pragmatic tautology, nothing follows for ontology.

So what is there to the data model, besides its structure, which makes it important to a scientific theory? We must add, using our own language, that for example this data model summarizes certain findings about bacteria, or about radio-active decay, as the case may be. Because representation is something we do, and not something that exists in nature independently of what we do, our claim of adequacy for the theory must involve reference to how we are using both models -- data model and theoretical model -- to represent our subject matter.

The best analogy I know is still between a person looking at a theory and one looking at a map. The latter has to be able to say "this is what things are like around HERE where we are -- so THAT is where we are on the map". It would be no use putting extra information into the map (such as an inscription "you are here") to try and obviate the need for such self-attributions by the user, for the same point would still apply to that "extended" map. Because the needed sort of judgement is indexical, it lies outside the domain of semantics. This point is of crucial importance when any sort of theoretical product is to be related to our practice.

3.4 Truth beyond the phenomena

Questions concerning the adequacy of a theory may be restricted or (putatively) unrestricted. The former, discussed in the preceding section, may be philosophically motivated -- naturally, empiricists favor a focus on empirical adequacy, i. e. adequacy with respect to the observable phenomena. A restricted question of adequacy may also be philosophically neutral, e. g. a question concerning adequacy with respect to processes of a certain type, or in a limited domain. The form of question will be the same in the two cases. So will be the form of answer, according to the semantic approach: whether the claim is one of isomorphic embeddability of the phenomena or processes in the theoretical model, or some less strict relation, that claim will be construed as asserting a structural relationship.

Now we have seen that this view needs a qualification, though at the pragmatic level. To make the claim (that there is such a structural relation) relevant, we must know how the models in question are being used to represent something. The required extra knowledge is a self-attribution of the user, expressed by him or her in indexical language, and cannot be obviated by building more into the models or theories themselves.

But what if the question concerning the adequacy of the theory purports to be unrestricted? What if it is, so to speak, the question of truth in toto? The suspicion has been raised in several quarters that within the semantic approach to science, at least when allied with empiricism, we cannot make sense of truth (simpliciter) at all.
If that were so it would indeed be a disaster for empiricism.
For the basic idea of empiricist philosophies of science, that theory is in general under-determined by empirical fact, requires a meaningful distinction between truth and empirical adequacy. I will try to show that the suspicion is not well founded. Indeed, I shall argue that it may derive from the fact that today, paradoxically enough, it is exactly the scientific realists who still yearn for an 'internal' logical connection between truth and verification.

Let us examine this suspicion, that we cannot, on our approach, make sense of a theory being empirically adequate but not true. At first sight the suspicion may seem easy to alleviate. Suppose a theory is empirically adequate exactly if it has some model in which all the observable phenomena are isomorphically embeddable. Can we then not equally and meaningfully assert that the theory is true exactly if it has a model in which not only all observable phenomena, but all systems and processes to be found in nature, can be so embedded? We would of course have to add the same discussion of pragmatics: an actual claim of truth, by us, for a theory, relates the theory's models to nature as described in our language. But at this point -- when we mention description of nature in our language -- the suspicion pricks up its ears, so to speak, and rears its ugly head much higher; for now we have touched on its very sources in the recent history of philosophy.

We must admit that it has for the most part been empiricists who tried to keep a logical connection between truth and empirical verifiability or access. Among not only the logical positivists but also among their antecedents and heirs there has been a distinct tendency to tie the possibility of description to the possibility of knowledge or even acquaintance. Here is a very early example, in the writings of Boltzmann:

> Formally ... we can raise questions of this kind: does only matter exist and is force one of its properties or does the latter exist independently of matter or, conversely, is matter a product of force? None of these questions, however, has any meaning, since all of these concepts are merely thought-pictures that have the purpose of correctly representing appearances. (tr. in Danto, p. 245)

In this passage Boltzmann implicitly shoulders us with a dichotomy. If what he says is right there must be a dichotomy between concepts that are theoretical and those which are purely neutral with respect to all theory. For if those questions about matter and force have no meaning and do not arise exactly because the concepts involved are mere 'thought pictures', then what? Then we must also have concepts which are not like that; else no question at all would have any meaning! Today we cannot accept such a dichotomy that, for we realize that all description of nature is non-trivially theory-laden. That is history: empiricists and scientific realists alike have learned that lesson. But representation makes sense; even representation of things never encountered in experience.

So what prompted Boltzmann's difficulty? We have such terms as "force", "position", and "momentum" in use, and have personally encountered some things to which these terms apply. Whether they apply or not depends on what those things are like. The question is now: do these terms apply to unobservable entities in the same way?

It would be better for us to ask what that question is supposed to mean! It sounds like the echo of the medieval debate about whether human predicates apply to God literally or analogically. The consequent theory of analogical predication was, in my opinion, an untenable answer to an incoherent question, for there cannot be a meaningful analogy if there is no available literal description. The theory becomes no more coherent if extended to electrons and quarks rather than God or gods. In any case, the very discussion seems fueled by a 'thought picture' of what it means for language to apply to things -- something like labels being stuck on to things according to some rule followable by a user of that language. The point is that whatever Boltzmann's personal difficulties were (and whether or not they were the very fallacies from which Neurath had to rescue the early Carnap), neither empiricism nor the semantic approach to science is still tied up in these knots.

The entanglement of semantics and epistemology did not die easily, and it took constant re-immersion in Tarski's work to keep the two even reasonably separate. It is a credit to the realists of the sixties that they showed us the logical fallacies involved when the two are entangled. But the sources of metaphysical realism also exhibit a deep desire to keep the very idea of truth intimately allied with knowledge. Russell, after all, was a leader of the revolt against idealism. But Russell immediately opted for the doctrine that we can understand a proposition only if we are directly acquainted with each constituent. With such a doctrine, which remained influen tial in philosophy of language to this very day, semantics is grounded in epistemology and truth is logically linked to human access.

This story continues. Today's metaphysical realism derives in part from a theory of language which did indeed lead in the revolt against positivism. But the theory entails that we understand a proposition only if, after all definitions and definite descriptions are eliminated, reference of other singular and general terms is to entities with which we are 'causally connected' -- a mysterious place holder for 'whatever it takes' to secure our epistemic access to the world.

In my opinion, these epistemic motives and their metaphysical train have no place in semantics. They enter as surrogates when it is attempted to simultaneously make semantics autonomous from pragmatics and also to infuse it with some independent cogency. Much better, I say, to study empirical adequacy overtly and explicitly for epistemic reasons, and to let truth have its sense derivative from our actual language in use, accepting without qualms our historical contingency and fragility.

So, what about truth? What about the sense of questions such as whether current atomic physics is true? Given the character of today's physics, and its highly mathematical form, we cannot sidestep issues of interpretation altogether. But we must approach them exactly as in our discussion of Putnam's paradox. Consider the statement

(*) Electrons always have a (precise) position.

Epistemologically this is an interesting example, for it is false according to certain interpretations and true according to others (such as Bohm's). Given that those interpretations yield empirically equivalent versions of quantum mechanics, it appears that if quantum mechanics is true (under any of those interpretations) then there is no way for us to know or confirm whether or not (*) is true!

But this does not affect in any way the question of whether it makes sense to ask whether (*) is true. If this is a statement in our language in use, then whether it is true or not simply depends on what electrons are like. That is, in that case,

"Electrons always have a (precise) position" is true if and only if electrons always have a (precise) position.

Of course, if the statement does not belong to our language, then the question of whether it is true (as opposed to "true under some interpretation") does not and cannot arise at all. This becomes clear when we replace 'electrons" by "sharks", "boojums", or "jabberwocks" (with a bow to David Mermin as well as Lewis Carroll of course).

Let me sum up this conclusion about structure and truth. A scientific theory gives us a family of models to represent the phenomena. But it represents the phenomena as fragments of a larger and simpler structure -- the world as it is according to the theory, if you like. These models are mathematical entities, so all they have is structure, the only thing they can represent is structure. There is nevertheless a right and a wrong in representation.

The question whether a theory as a whole is true or false makes sense. It just doesn't make sense if we separate this question from the language in which we ask it, our language in use.

If we cannot describe the world to be represented, then we cannot understand how a theory represents it at all. If we can describe the world, then we can understand that; but then we automatically have also a criterion of correctness or accuracy; that is, of truth. More moderately, questions of truth make sense exactly and only to the extent that we have words to describe what things are like. At this point we can rely on our own language or we can distrust it, but we cannot get behind it - - at this point, as Carnap used to say, the question we face is no longer theoretical but practical -- or as perhaps we should say, more precisely and less faintheartedly, existential.

ENDNOTES

[1] For the following discussion I am especially indebted to the papers by Lewis and Elgin, whose conclusions are of course diametri cally opposite to each other.
[2] Perhaps Putnam is supposing that we can describe THE WORLD when he writes "I imagine that THE WORLD has (or can be broken into) infinitely many pieces", for "broken into" may be a metaphor for "described". This would then be the assumption that we can describe THE WORLD, but only to the very limited extent of singling out individuals and counting them. Not surprisingly then, our theories about the world contain no real information which goes beyond cardinality!

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