Representation: The Problem for Structuralism

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What does it mean to embed the phenomena in an abstract structure? Or to represent them by doing so? The semantic view of theories runs into a severe problem if these notions are construed either naively, in a metaphysical way, or too closely on the pattern of the earlier syntactic view. Constructive empiricism and structural realism will then share those difficulties. The problem will be posed as in Reichenbach's *The Theory of Relativity and A Priori Knowledge*, and realist reactions will be examined, but they will be argued to dissolve upon scrutiny.

1. Introduction.

> In general, . . . proponents of the semantic view of theories [say] that theories represent the phenomena just in case their models, in some sense, "share the same structure." . . . Underlying this [symposium] are two assumptions: i) that the semantic view of theories is the "framework" for structural realism and ii) that scientific representation is structural representation. (From Elaine Landry's proposal for this symposium)

Mutatis mutandis for constructive empiricism recast as an *empiricist structuralism* (van Fraassen 2006):

a) The slogan that *all we know is structure* is correct in the following sense:

> I. Science represents the empirical phenomena as embeddable in certain *abstract structures* (theoretical models).
> II. Those abstract structures are describable only up to structural isomorphism.

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b) The addition "*but this knowledge is cumulative . . .* " is also correct, but in the following sense:

> III. There is continual accumulation of empirical knowledge through theory change.

Accumulation in a minimal sense: the resources for prediction of empirically accessible circumstances are increasing, while the theories are not growing but being replaced: "the phenomena cannot be embedded in models of theory T1, but rather in models of T2, no, after all, those of T3"—and so on. (This is itself an empirical claim.)

But now for the perplexities principle I should engender. What does it mean, to embed the phenomena in an abstract structure? Or to represent them by doing so? The semantic view of theories runs into severe difficulties if these notions are construed either naively, in a metaphysical way, or too closely on the pattern of the earlier syntactic view. Constructive empiricism and structural realism will then share those difficulties.[1]

2. Reichenbach: How Can an Abstract Entity Represent?

> *How can an abstract entity, such as a mathematical space, represent something that is not abstract*, something in nature?

The perplexities appear clearly, it seems to me, in Chapter 4, "Knowledge as Coordination," of Reichenbach ([1920] 1965). *As stated*, that problem has nothing to do with knowledge. Perplexed, Reichenbach writes:

> The mathematical object of knowledge is uniquely determined by the axioms and definitions of mathematics. (Reichenbach [1920] 1965, 34)
>
> The *physical object* cannot be determined by axioms and definitions. It is a thing of the real world, not an object of the logical world of mathematics. Offhand it looks as if the method of representing physical events by mathematical equations is the same as that of mathematics. Physics has developed the method of defining one magnitude in terms of others by relating them to more and more general magnitudes and by ultimately arriving at "axioms", that is, the fundamental equations of physics. Yet what is obtained in this fashion is just a system of mathematical relations. What is lacking in such system is a statement regarding the significance of physics, the assertion that the system of equations is *true for reality*. (Reichenbach [1920] 1965, 36)

1. Compare Demopoulos (2003); I began to face these difficulties in van Fraassen (1997a, 1997b, 2001).

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Reichenbach is quite right to point out that a theory in mathematical physics is, if just taken in itself, a mathematically formulated theory, in just the way that, for example, Euclidean and hyperbolic geometry are. How could it have more content, to make it something different from pure mathematics? No use adding an extra axiom such as "the above axioms are true of reality."

At issue is the relation to be asserted by us, who display this theory (or the family of mathematical objects it defines) to physical objects, events, and processes. Reichenbach takes the term "coordination" from Schlick to indicate what sort of relation is meant, as at least a metaphor to guide us. But it might lull us back into the mistake indicated above in his "Offhand it looks as if the method of representing physical events by mathematical equations is the same as that of mathematics." Reichenbach tries out various responses to the problem, including the idea of success postponed to the end of the rainbow—uniqueness of coordination by comprehensive theory that makes sense of *all* the phenomena. But that makes sense only if the question of just what coordination can be, between something abstract and something concrete, has already been settled.

2. The 'Offhand' Realist Response. What did Reichenbach mean with his "Offhand . . . " remark? He imagines in effect the following naive reply: *what is called for is simply a function, a mapping, between mathematical objects and physical objects or processes—so what is puzzling about that?*

True, we have no difficulty with mappings between two sets of mathematical objects. But notice: to define such a mapping we must identify its domain and its range, plus the relation that effects the coordination between them.

> For example, if two sets of points are given, we establish a correspondence between them by coordinating to every point of one set a point of the other set. For this purpose, the elements of each set must be defined; that is, for each element there must exist another definition in addition to that which determines the coördination to the other set. Such definitions are lacking on one side of the coördination dealing with the cognition of reality. Although the equations, that is, the conceptual side of the coördination, are uniquely defined, the "real" is not. (Reichenbach [1920] 1965, 37)

So here is the problem baldly stated. If the target is not a mathematical object then we do not have a well-defined range for the function.

Mea culpa: in *The Scientific Image*, constructive empiricism was presented in the framework of the semantic view of theories, but seemingly

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in the shape of the above 'offhand' realist response.[2] For empirical adequacy uses unquestioningly the idea that concrete observable entities (the appearances or phenomena) can be isomorphic to abstract ones (substructures of models). That this offhand way of talking is most readily interpreted in metaphysical terms is clear also in comments by such acute and careful readers as Stathis Psillos and Michel Ghins.[3] Rather than try to excuse or explain my obliviousness to these issues at that time, let's see how we can do better.

3. Vacuity of the Offhand Realist Response. The natural temptation is simply to impose a parallel vocabulary and declare victory. One might say, for example, that a point in Minkowski space corresponds to a real or physical space-time point, which is a compact convex part with zero measure of the WORLD (to use Putnam's derogatory term).

Reichenbach's illustration is Boyle's Law, $PV = rT$. To give this physical meaning, its terms must be 'coordinated' with physical quantities. But what is, for example, the temperature of a gas? It changes with time and differs from one body of gas to another, so isn't it a function that maps bodies of gas and times into the set of real numbers? I can try to cut the Gordian knot by saying, "There exist physical quantities P^*, V^*, and T^*, which pertain to bodies of gas, and when 'P', 'V', and 'T' refer to these respectively then '$PV = rT$' is true." That is precisely to create a parallel vocabulary and declare victory. But it is an empty victory.

The metaphysical realist's response depicts nature as itself a relational structure in precisely the same way that a mathematical object is a structure. Hence, if the mathematical model represents reality, it does so in the sense that it is a *picture or copy*—selective at best, but accurate within its selectivity—of the structure that is there:

> there is no problem, because this depicted physical system *is* a set of parts connected by a specific family of relations—so of course there are functions defined on this set of parts with range in the model and vice versa.

A function that relates A and B must have a set as its domain. If A is, for example, a thunderstorm or a cloud chamber—a physical process, event, or object—then A is not a set. *Fine*, the realist answers, but A has

2. See Van Fraassen 1980, Chapter 3, Section 9, page 64. Demopoulos (2003) comments on this passage. It should, however, be read in the context of the principle that a scientific theory, including its theoretical terms, is to be understood literally (insofar as possible; see note 6 below).

3. Psillos (2006, in this issue) remarks on correspondence and structures *in re*; see also Ghins (1998, 328).

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parts, and the function's domain is the set of these parts. Moreover, there are specific relations between these parts, and these relations have as their extensions sets of sequences in that domain. The function provides a proper matching provided that the images of these relations are relevant relations in the model.

So the function relates two structured sets, call them S(A) and B:

> S(A) = <SA1, SA2>, where:
> SA1 = the set of parts of A,
> SA2 = the family of sets that are extensions of relations on these parts,
> B is a mathematical object, representable correspondingly in the same general form <Set, Relations>.

What is taken for granted is the relation between A, the physical entity, and S(A), a relevant mathematical representation of A. But why is the relation between A and S(A) any different from the one we asked the question about, namely, the relation between A and B? We cannot very well answer "*how can an abstract mathematical structure represent a concrete physical entity?*" by saying this is possible if we assume the latter is represented by some other mathematical object.

Let's let the realist speak up again. First of all, s/he says, there is no question that the sets S(A), SA1, and SA2 are real if A is real. When we say that B is an adequate representation of A we mean simply that S(A) is isomorphic to, embedable in, or homomorphically mappable into B.

So far, so good—we can agree to all that (modulo whatever view of mathematics we have, this use of an elementary part of set theory must be legitimate).

But the realist's next problem is that this sounds like there is more uniqueness than there is—that there is less selectivity in what S(A) can be than there must in fact be. (This point is made and explored by Stathis Psillos [2006, in this issue].) The respect in which B represents A may be one thing or another: that depends precisely on how we 'divide up' this entity A and which aspects of its relational structure we select—that is, what we choose for SA1 and SA2.

But what indicates here the relationship between A and S(A)? It is the description or denotation of SA1 as "the set of parts of A," and so on. Now the word "the" is a misnomer, given that A can be 'divided up' in various ways. And just what is this dividing up? It is the *act of representing* A *as having* SA1 *as its set of parts*—that is, of A as consisting of the members of SA1. All might be well for the realist response (which thinks of the coordination as a simple two-place relation between the two items)

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if there were only a single, unique way of representing A in this manner, but of course there is not.[4]

The realist's final gambit comes as no surprise.[5] S/he insists that there is an essentially unique way of "carving nature at the joints," with an objective distinction 'in nature' between, on the one hand, arbitrary or gerrymandered and, on the other hand, *natural* divisions of A into a set of parts, and similarly between arbitrary and natural relations between those parts.

This is a postulate (if meaningful at all). What can it possibly mean? The word "natural," its crucial term, derives its meaning solely from the role this postulate plays in completing the realist response. *Honi soit qui mal y pense* they will tell us. But the point will be moot if the problem dissolves upon scrutiny, as I shall maintain.

4. Courting an Illusion of Reason. Reichenbach was of course focused on how mathematical spaces can be 'coordinated' to natural processes and events. Popular presentations (including Einstein's) aside, neither perception nor individual cognition is relevant to the question of how Minkowski space can be used for the representation of rigid motion, electric current, magnetic field, and transmission of light. That can have an answer only in a context with resources already at hand for a description of light and motion. Reichenbach lets himself be led into the question of how such use is possible *outside any such context*.

Hence *there is a basic mistake begging to be made right at the beginning*. The question of how a specific mathematical object can be used to represent specific phenomena makes sense only in a context in which some description of the latter is at hand. Reichenbach, it seems to me, mistakenly pursues the 'profound' 'foundational' question of how such use is possible *outside any such context*—as if theories are received by babies or primitives before the acquisition of language.

The use of graphs, functions, and mathematical spaces within scientific practice is not a case of applying concepts to the unconceptualized but to structures that already have a description admitted as acceptable in context. To repeat: we can use any suitable entity, abstract or concrete, to represent something else, and we can represent it as thus or so, but only provided

4. Some such relationship between A and B will certainly exist provided only A can be represented as having a set of parts of the same size as that of B. It is the point that (by now, famously) trivialized Russell's structuralism and drives Putnam's model theoretic argument against metaphysical realism (Demopoulos and Friedman 1985; van Fraassen 1997a).

5. Reminiscent of responses to Putnam's model-theoretic argument by, e.g., David Lewis (who calls it "Putnam's Paradox").

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that we have a pertinent description of both items. The description must be in our own language, in our language-in-use. Reichenbach expresses his puzzlement in words like these:

> The coördination performed in a physical proposition is very peculiar. It differs distinctly from other kinds of coördination. . . . For each element there must exist another definition in addition to that which determines the coördination to the other set. Such definitions are lacking on one side of the coördination dealing with the cognition of reality. Although the equations, that is, the conceptual side of the coordination, are uniquely defined, the "real" is not.

He is explicitly addressing a situation in which there is no description at hand for what is to be represented.

Why would he put himself into this situation? My conjecture: because he assumes that the description must be 'independent', that it must be in an 'empiricistically hygienic' language, not theory laden, not theory infected. He imagines us in the following unreal predicament:

> We have a theory stated in its own new language (only overlapping our language in use), and we must somehow give it intelligible content by linking it to something described in a part of our language devoid of mathematical or theoretical terms.

But he has talked himself into a corner, for in the second passage quoted he demonstrates in effect that the 'solution' in the third quoted passage is impossible.[6]

5. The Problem in Concrete Setting. For familiar natural phenomena such as daybreak and starlight, electric light or electromagnetic effects, science provides us with models. Often the initially startling event is that a model of a sort we have been trusting does not save them. The question how an abstract structure can represent something, transposed to such a context, is just this: *how, or in what sense, can such an abstract entity as a model 'save' or fail to 'save' this concrete phenomenon*? What is the pertinent relation that holds or does not hold between the mathematical structure described by our equations and that natural or artificially produced process?

Ronald Giere and Paul Teller have both discussed this with reference

6. Scientific theories are to be understood literally (van Fraassen 1980, 11). That is possible only for however much of them is stated in our language in use, the language we live in—which does not of course preclude a healthy agnosticism about the existence of entities described therein.

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to detailed accounts of empirical research.[7] Let's take Young's 1802 double slit experiment, which together with Fresnel's "light spot" in a shadow were the crucial phenomena that Newton's particle models for light could not save. Somewhat more than a hundred years later, once the photoelectric effect was discovered and its quantum character appreciated, such experiments needed to be revisited and reinterpreted.

The reinterpretation focuses on a variation that could not be performed by Young, in which the source is so slow that only a single black spot appears on the screen during nonnegligibly large time intervals. (Experiments in which individual photons were isolated could actually not be performed until the last quarter of the twentieth century.) If carried out in this form, the interference pattern will appear only gradually. Similarly of course for each of the two separate single slit experiments resulting with either of the slits closed.

So let us see what the data could be. Imagine the screen divided into N small areas, indexed as $x = 1, 2, 3, \ldots$, and consider for each the proportion of hits in that area after the first n hits on the screen. Then the data gathered are recorded in three relative frequency distributions: $\mathrm{rel}(n, x)$, $\mathrm{rel}_A(n, x)$, and $\mathrm{rel}_B(n, x)$. For example, $\mathrm{rel}_A(50, 17)$ denotes the proportion of hits, among the first 50 hits, that occurred in area 17 when only slit A was open.

This record is the outcome of the specifically carried-out experiment, but it is certainly not yet the data model that any theory about the phenomenon must confront. To construct the data model, a 'curve smoothing' program replaces the three relative frequency functions with three probability functions: p, p_A, and p_B. (This involves an empirical hypothesis—future repetitions of the experiment could conceivably yield a bad fit with these functions.)

> The data model is actually $<p, p_A, p_B>$.
>
> **Theoretical model T1.** p is a mixture of p_A and p_B. Empirically refuted.
>
> **Theoretical model T2.** p, p_A, p_B are determined by a geometric probability model. This works.

Our diagnosis of this procedure: the *theory to phenomena relation* displayed here is an embedding of one mathematical structure in another one. For the data model, which represents the appearances, is a mathematical structure.

So there is a 'matching' of structures involved, but it is a matching of

7. Giere (1988, Chapters 3, 7–8; 1999, Chapters 6–7) and Teller (2001), discussed in van Fraassen (2001).

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two mathematical structures, namely, the theoretical model and the data model.

6. The Problem Revisited and Dissolved. How can we answer the question of how a theory or model relates to the phenomena by pointing to a relation between theoretical and data models, both of them abstract entities? The answer has to be that the data model represents the phenomena, but why does that not just push the problem one step back? The short answer: construction of a data model is precisely the selective relevant description (by the user of the theory) required for the possibility of representation of the phenomenon. But this may not be obviously satisfactory.

> CHALLENGE: "Oh, so you say that the only matching is between data models and theoretical models. Hence the theory does not confront the observable phenomena—those things, events, and processes out there—but only certain representations of them. Empirical adequacy is not adequacy to the phenomena pure and simple, but to the phenomena *as described*!"

Indeed, how can we answer the question of how a theory or model relates to the phenomena by pointing to a relation between theoretical and data models, both of them abstract entities? The answer has to be that the data model represents the phenomena, but why does that not just push the problem one step back?

To defeat the misunderstanding we need a positive account of the sense in which a theory or its models can be said to accurately or inaccurately represent physical phenomena. That account will mention a matching, namely, between parts of the theoretical models and the relevant data models—both of them abstract entities. But the punch comes in the word "relevant." *There is nothing in an abstract structure itself that can determine that it is the* ***relevant*** *data model, to be matched by the theory*. That is why our talk of data models 'between' the theoretical model and the phenomena does not simply push Reichenbach's question one step back, to be faced all over again in the same way. The assertion that *the data model, an exponential function, represents in smoothed summary form the growth of that bacterial colony* is, despite appearances, an indexical statement. It is not made true by anything that can be formally described within semantics, understood as limited to word-thing relations.[8]

That is, the phenomenon, what it is like, taken by itself, does not determine which structures are data models for it. That depends on our

8. That is not to deny that this three-place relation can be described, assigned a set as its extension, and so forth. But an indexical sentence is not meaning equivalent to an nonindexical one, except within or relative to a context of use.

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selective attention to the phenomenon, and our decisions in attending to certain aspects, to represent them in certain ways and to a certain extent.[9] So once again we arrive at the conclusion: there is nothing of use to be found in two-place structure-phenomenon relations—what we see by way of such relations are abstracted from the three-place relation of *use of something by someone to represent something as thus or so*.

I have to complete this account by addressing what does make such an assertion true, and why it should be called indexical. So, back to the above challenge! The empiricist reply must be:

1) The claim that the theory is adequate to the phenomena *is indeed not the same* as the claim that it is adequate to the phenomena as represented by someone (nor as represented by everyone, or anyone).
2) But 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.
3) *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 represented, that is, *as represented by us*, are indeed the same!

This last point is a pragmatic tautology. That it holds depends crucially on how the indexical word "us" functions in an assertion.[10] Appreciating that the equivalence for us is a pragmatic tautology removes the basis for the challenge.[11]

9. As Psillos (2006, in this issue) also emphasized. Perhaps Reichenbach means something like this when he says that reality (i.e., the phenomenon that we are describing) is first defined by the coordination—which was, as suggested above, his solution to the question how models represent the world. But it could only be 'something like this', since this way of putting it implies that what the models represent is something like 'the physical objects as described' rather than the physical objects. The "as" in that phrase is the traditional "qua," and one is meant both to identify and distinguish the referents of "the X, which is F" and "the X qua F"—not a resource the later empiricism can draw on, to put it mildly.

10. As analogy consider the two questions: "Are electrons negatively charged?" and "Is our sentence 'Electrons are negatively charged' true?" They are not the same question. After all "Electrons" could have been our word for sheep, snakes, or stars. But given that this sentence is *our* sentence, as things actually are, the questions amount to the same thing *for us*. That is why for us, the people who speak this language, "The sentence 'Electrons are negatively charged' is true if and only if electrons are negatively charged" is a pragmatic tautology.

11. Although my approach here has been to finesse the objection in question, rather than to meet it head on, my response to it does not land us in subjective idealism. The point for me is *definitely not* that our theories are about *the world as represented by us*. That would be like saying that my statement "Snow is purple" is about purple snow (i.e., snow as depicted in that very sentence) rather than a falsehood about snow.

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A pragmatic tautology is a statement that is logically contingent, but undeniable nevertheless. Logical contingency pertains to its content, while deniability pertains to use in assertion, denial, or calling into question. The two diverge when the statement is indexical or context dependent. Consider the phrase, "There are no statements": although it contains no indexical terms explicitly, it is logically contingent but unassertable—a pragmatic inconsistency.

How does this apply here? Suppose that I have represented the deer population growth in Princeton, New Jersey, by means of a graph. I point out that theory T provides models that fit very well with the structure displayed in that representation—call it S.

> CHALLENGE: "Yes, T fits well with this representation S, but does it fit the actual deer population growth in Princeton?"

Although I can see the logical leeway, there is no leeway for me in this context. For if I were to opt for a denial or even a doubt, I would in effect be saying:

> "The deer population growth in Princeton is thus or so, but the sentence 'The deer population growth in Princeton is thus or so' is not true, for all I know or believe."

This is as paradoxical as such Moore's paradox forms as "It isn't so, but I believe that it is" or "It is so, but I do not believe that it is so."

To sum up: in a context in which a given model is my representation of a phenomenon, there is for me no difference between the question of whether a theory fits that representation and the question of whether it fits the phenomenon.[12] If we treated pragmatic inconsistencies like logical ones we would infer from the inconsistency in Moore's paradox to the certainty of, for example, *It is so, if I believe that it is so*. Similarly, to postulate metaphysical structure *in re* to save structuralism would be like 'saving' Moore's paradox by postulating clairvoyance.

12. Now we can see why the offhand realist response sounds plausible at first, because it does get something right—namely, that in the end there is no problem, precisely because we can (*a*) correctly describe relevant parts of nature and mathematical objects and (*b*) say how they are related to each other. But this plausibility hides the mistake of replacing "we can" with a relation independent of the user (the "we") and ignores the selectivity exercised by the user for the user's specific purpose.

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