

# Structuralism and coordination

Matthew McMillan

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## 1

A typical structuralist about science thinks we come to know the structure of the physical world; we come to know *that* the world has some structure. For example through general relativity that the world has the structure of a solution to Einstein's field equations, or through quantum mechanics that it has the structure (for the Everettian) of a solution to the Schrödinger equation on a Hilbert space.

The problem of coordination has been one of the most troubling for structuralist philosophies of science. The problem is that knowing merely *that* the world has some structure without knowing *how* it has the structure, knowing how the structure is coordinated onto the world of experience, is meaningless or trivial. By most accounts knowledge of coordination cannot be itself structural in form. The problem, then, for the structuralist, is to understand what this additional knowledge is, and how it works.

The various flavours of structuralism (metaphysical,<sup>1</sup> epistemological; realist, empiricist) come with various commitments and motivations which I shall not distinguish here. If an uncoordinated structure really is meaningless or trivial, all structuralists must confront the problem.

Perhaps the first modern manifestation of this problem came when John Newman pointed out [3] that Russell's structuralist reconstruction of the world from sense data ([4], [5]) was somehow vacuous. We will see the objection applied later ([6], [7]) against Carnap's structuralism ([8], [9]). The problem was noticed again for structuralisms drawing on the spirit of highly mathematised physics by Hermann Weyl [10]; and in more formally logical terms the problem underlies Putnam's Paradox [11].

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<sup>1</sup>Some, e.g. French and Ladyman ([1], [2]), advocate a structuralist *ontology*. I think the problem applies to them insofar as the structures that we do come to know could be mapped onto the world structure in a multiplicity of ways, but I do not want to push this point.

Bas van Fraassen very recently attempted a solution to this problem in the context of his (constructive) empiricist philosophy of science ([12], [13]). His solution is to require the *user* as a third term in the relation between representation and reality. Coordination is modelled on the *indexicals* of ordinary language (e.g. ‘here’, ‘now’). Like the ‘you are here’ mark on a map, this element allows the user to locate herself with respect to the structure, making its claim of representation significant and useful.

I aim with this essay to show that van Fraassen’s attempt does not succeed.

His solution (as given) fails because it does not carry the user deep enough into the theory structure, and I worry that a successful analogue of his approach will suffer from two problems. On one hand it will undercut his overall empiricist project, for it will require the user’s perspective at the deepest levels of theory; and on the other hand it will become a very strong kind of pragmatism, losing any semblance of context-independent meaning. Without completing the project in these directions the problem of coordination is for him outstanding.

The essay is in two parts. In the first we will warm up with the problem by a discussion of the debate over Carnap’s ‘Ramsey Way’, asking whether it suffers from something like the Newman objection. This will set the conceptual context for the main part, in which I present van Fraassen’s solution and explain why I think it isn’t satisfactory. I conclude with the vaguest sketch of the direction I would take for my own solution.

## 2

Let’s begin with the Ramsey sentence and Carnap’s alleged neutralism on the reality of unobservable objects. The Ramsey sentence is well known: write the conjunction of all true statements of a theory (or of some complete subset), replace all theoretical terms with variables, bind them with existential quantifiers.

Carnap explained the point of this procedure very clearly:

In Ramsey’s way of talking about the external world, a term such as ‘electron’ vanishes. This does not in any way imply that electrons vanish, or, more precisely, that whatever it is in the external world that is symbolized by the word ‘electron’ vanishes. The Ramsey sentence continues to assert, through its existential quantifiers, that there is something in the external world that has all those properties that physicists assign to the electron. ... The troublesome question it avoids is not, ‘Do electrons exist?’ but, ‘What is the exact *meaning* of the term “electron”?’ [9, p. 252]

The point, for Carnap, was to *define* (implicitly) the theoretical terms in

observational terms. They cannot be gotten at individually in proper explicit definitions, for reason of theory complexity if not Quine-Duhem wholism, but this trick can be used instead. The Ramsey sentence should be empirically equivalent to the theory since it should have the same consequences in observation terms. Carnap thinks of the Ramsey sentence as the empirical component, and the analytic component, the *meaning postulate*, is the further assertion of the implication from the Ramsey sentence to the theory proper; this *carries out* the definitions of theoretical terms.

Grover Maxwell expanded usefully on the idea, making a connection to Russell's *knowledge by description* (which will make a return in §4):

The Ramsey sentence refers to theoretical entities in exactly the way in which any description refers—by means of variables, quantifiers, logical connectives, and descriptive terms whose *direct referents* are other than the referents of the description. This, incidentally, may be taken as an explication of the claim of Russell and others that our knowledge of the theoretical is limited to its purely structural characteristics and that we are ignorant concerning its intrinsic nature. [14, p. 188]

Maxwell is an ardent follower of the Ramsey Way. 'In principle, talk of structural properties, intrinsic properties, etc., is superfluous; for a grasp of the meaning of the Ramsey sentence provides the whole story.' [14, p. 189] And he is convinced 'the Ramsey sentence has exactly the same observational consequences as does the original theory' [14, p. 187].

If we grant Carnap that we have access to a clear and distinct world of observation, that the meanings of observation terms give no trouble of their own, then it may seem he's succeeded in a sort of neutralism: those existential quantifiers make him realist about theoretical things, but they are logically dependent on the empirical substructure.

Stathis Psillos responded with a variant of Newman's objection against Russell, that nothing prevents the structure contained in the Ramsey sentence from being instantiated only *trivially* [6, p. 63]. No part of the Ramsey sentence requires 'natural' theoretical kinds and relations in the world; any random bijection between things and theoretical terms could serve to instantiate the Ramsey structure in a contrived way, cutting the world far from its joints.

(Psillos himself implicated the mathematical domain of Carnap's variables, but I think that's somewhat beside the point. Van Fraassen is right that the Newman objection, applied to Carnap's structuralism, is that the alleged relations could well be 'gerrymandered'. The same problem, he thinks, appears for first- and second-order syntactic approaches as for the semantic approach he employs, for which the problem was noticed by Weyl and Putnam.)

Psillos does admit that the Ramsey sentence is empirically equivalent to the theory, but he thinks that a realist should stress that it ought to be an open question whether the Ramsey sentence is *true* over and above the question of its empirical adequacy.<sup>2</sup>

Carnap would not be moved by this Newman objection. The purpose of the Ramsey sentence was precisely to write science in the observation language, for only these are *naturally* meaningful. There *shouldn't* be any independent question of truth; empirical adequacy is the only test of a theory. He goes beyond instrumentalism only by formally asserting the theoretical entities with existential quantifiers.

What then should we say? I think we should accept both sides of the dispute, both that nothing is missing from the Ramsey sentence, and that satisfaction is trivial. We can see this tension running quite deep, flowing ultimately from a dichotomy between perspectival experience and descriptive knowledge of general propositions. (See §4 for just a hint more.)

On the one side the strength of the Ramsey sentence cannot be overestimated. John Worrall saw this, and hoped the distinction between observation terms and theoretical terms could save the project:

It is in fact well-known that, in that case [that observational predicates aren't quantified over], the original theory  $T$  and its Ramsey sentence will be empirically equally powerful: that is, every sentence that is expressible purely in observational terms and is deducible from  $T$  is also deducible from its Ramsey sentence. [15, p. 150]

Worrall's response is good. The point of the Ramsey sentence is to make a theory into a logical construction out of observation terms. It can be *proved* that two Ramsey sentences are logically compatible *if and only if* they are equivalent in observation terms.<sup>3</sup> The objection Psillos raises seems not to take sufficiently seriously the empirical equivalence: just how does Psillos propose to go about establishing the truth of a theory, *independently* of all empirical consequences?

At the same time, on the other side we should not agree that Worrall's emphasis on the observational–theoretical distinction takes the bite out of the Newman objection. Any claims the theory makes that go beyond the observational substructure will still be trivialised by lack of coordination. For Worrall's dodge to succeed he needs a strong empiricism, to the effect that empirically

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<sup>2</sup>[6, p. 60]. Cf. also Demopoulos [7].

<sup>3</sup>This was noticed by Jane English [16], and clarified (or corrected) by Demopoulos [7, p. 380].

equivalent theories are identical in *meaning*. In fact he may as well procure a formal equivalence relation, identifying theories iff they have the same observational consequences. But he should remember that this is hardly realism. ‘The identification of a theory with its Ramsey sentence therefore comes at a price: the notion that two theories might be compatible with the same data and yet conflict with one another must be given up’ [7, Demopolous, p. 380].

Returning to Carnap’s structuralism, it seems he can turn in two directions. Either he can emphasize the *structure* contained in the Ramsey sentence, and the assertion that it just is true in the world, or he can emphasize the immanence and accessibility of a naturally structured world of observation with attendant vocabulary. The first is more clearly a standard structuralism and faces the problem of coordination in full strength, the second (which I think is what he wants) will wind up a strong empiricism that denies ordinary meaning to a theory’s theoretical claims.

The basic problem of coordination is that the mere assertion of a structure is useless: the structure may be instantiated trivially, or oddly; the particulars of the way it’s instantiated cannot be specified structurally. The satisfaction by things in the world of a merely formal, logical structure of desired type makes that structure true of the world. (If an arbitrary bijection is only somewhere *written down*, one could employ the writing in a recipe of use of the theory that makes it true in its own way.) This underlies Weyl’s problem that structure is only defined up to isomorphism, and it underlies the Putnam Paradox, which just adds that even cardinality (given infinity) is no constraint.

I like van Fraassen’s illustration: ‘*There exists a function* that maps the main landmarks and streets of Paris more or less continuously into these marks and striations [on a blank sheet of paper], in a sufficiently detailed way to rival even the best commercially available maps’ [13, p. 233]. The mere existence of such a function, an isomorphism from the abstract structure to the world, means nothing.

## 3

### 3.1

In broadest possible outline, van Fraassen’s proposal is first to narrow the field on the abstract structures that represent any phenomena to the substructures he calls ‘data models’, and second to make a Wittgensteinian ‘indexicality’ move whereby these data models represent phenomena *for us* because we take them to. What makes a data model the model *of* some phenomenon is that it is *our* model of that phenomenon. And then the data model, already abstract, plugs

directly into the abstract structure of the full theory.

The abstract theory structure, the complete model, he imagines like a map representing the world. It represents the world for particular users with certain purposes. To be usable, like any map, it needs a ‘you are here’ marker, which lets the users locate themselves on the map, or within the theory structure. The proposition that ‘you are here’ corresponds on his analogy to the proposition that given data models *represent* given phenomena in the world. And how does that representation work? Since the data substructures lie in the periphery of the theory structure, this representation is safely credited to pragmatics.

We should give this in a bit more detail. The key moves are (1) the distinction between total theory structure (the model of reality) and the embedded substructures (data models representing observable phenomena), and (2) the anchoring of total model to reality by the ‘indexical’ attachment of the phenomena to the data models. (The total model is ‘pinned down’ to the world at the data models.)

The division in (1) is (meant to be) of a piece with his division between observable and unobservable, and he doesn’t take it to be particularly troublesome from the point of view of coordination. All the coordinating work, i.e., the substance of his solution to our problem, is done in (2) by his pragmatic move with the data models.

He does not dwell on (1) so I can’t say much, but it is equally important for my argument, so let me say this. Once we have the data models, we have attained the level of abstract structure, and such structures are easily compared and embedded. His thought is that at this point theory models can be matched to the data models, and will compete (red in tooth and claw) for best match [13, pp. 251-2]. The reason no more coordination is needed (coordination of the data substructures into the theory structure) is that this matching is simply done by ‘best fit’. A mathematician can enumerate the ways we might try to embed our data in a proposed theory, and whatever works best (nicest theory fitting most data) is accepted. If the shoe fits, wear it.

His discussion of (2) is much lengthier, but I hope to make it as clear. He has reduced the problem to that of explaining how a data model represents a phenomenon:

[I]n the chain [*theory*]-[*data model*]-[*reality*] the last link is one that is expressed in indexical judgments. [13, p. 257]

Van Fraassen is not committed to a single observation language for all theories, doesn’t believe in sense data, and in fact abjures universals of any kind. So data models are not coordinated to phenomena all at once as they were for Carnap, but rather on a case-by-case basis. How does *a* data model represent a phenomenon? He insists:

*There is nothing in an abstract structure itself that can determine that it is the relevant data model . . . .* [13, p. 253, his emphasis].

Nor does the phenomenon, what it is like, taken by itself, determine which structures are data models for it. That depends on our selective attention to the phenomenon . . . . [13, p. 258]

There is no such thing as representation apart from or independent of our practice. [13, p. 249]

And his answer to the problem is that:

. . . construction of a data model is precisely the selective relevant depiction of the phenomena by the user of the theory required for the possibility of representation of the phenomenon.

. . . the relation to be identified is not a 2-place relation between data model and phenomenon but a 3-place relation that involves the user. [13, p. 253]

We can sum up the relevant point quite simply: in a context in which a given model is *someone's* representation of a phenomenon, there is *for that person* no difference between the question *whether a theory fits that representation* and the question *whether that theory fits the phenomenon*. [13, p. 260, his emphasis]

In other words, it is the *use as such*, by practitioners, of data models to represent phenomena that makes them to do so. It's a 'pragmatic tautology', as he calls it, that a data model represents a phenomenon for an individual.

Before commencing my critique I should clarify his analogy to the 'indexical' of ordinary language.

The indexical, for him, the 'you are here' mark on the map of theory, *just is* the individual's use of data models. This use anchors the scientist's world, world of experience, into the theory structure:

The assertion that a given graph represents the phenomenon I have been observing and measuring is on a par with the assertion that a certain spot on a map is the point *where I am*. The conditions of correctness in both cases pertain to the use to which the structure—whether abstract or physical—is being put, and how. [13, p. 257]

We have to locate our situation in the theory's logical space, in a way that is similar to our 'We are here' with respect to a map. [13, p. 261]

Indexicals in ordinary language are terms whose meanings are radically context-dependent. Some, like ‘I’, point to whomever is speaking (and are also *referential*); others like ‘here’ and ‘now’ invoke the location of utterance (in both senses). I do not think van Fraassen means the referential indexicals; for he does not mean that every scientific proposition explicitly involves the scientist, as part of the *subject matter* of the proposition. Rather, more like ‘here’, he means an indexical of *place*. Now he *also* doesn’t mean ‘place of the data model in the theory superstructure’. (One might have been thinking of the place of a reference frame in spacetime.) For again, that is taken care of by (un-)natural theory selection, in purely structural terms. He means the place of a phenomenon in a data structure; or rather, the place of ourselves, our world, in the multiplicity of data structures (each case separately).

### 3.2

These are deep waters, and I won’t pretend a fair deconstruction will be easy to give. But I will try to be clear.

I would challenge each part of his program. First, I would challenge the possibility of confining the coordination problem to the data model substructures. Second, I would challenge what seems to be a Wittgensteinian move without real Wittgensteinian commitment (if that’s a thing). The second challenge comes in complementary pieces: (1) the indexical feature that connects data models to the world does so even for data that is *of* highly unobservable elements, reaching deep into and infecting the whole theory, and (2) the pragmatic explanation is a poison that now kills the whole theory; for it is far too powerful a tool. He’s invoked a sledgehammer, and the principles of his philosophy give no natural way to contain it.

Before expanding these it will help to record some of van Fraassen’s other commitments which I take my argument to push him into. Van Fraassen’s empiricism has it that we needn’t *believe* what theories say about the unobservable, but that we *should* take them at *face value*, shunning fancy hermeneutics of metaphor and mythology. In other words, he takes it the *meanings* of theoretical claims are of the quite normal variety, and the empiricism comes only at the level of knowledge and belief [17, pp. 10-11]. An implication is that theories can indeed say (and mean) distinct things, while having exactly the same observational consequences. This puts him in a very different position than Carnap and (apparently) Worrall.

Another commitment is that not everything is language games. Semantics, classically conceived, concerns the element of meaning that is largely insensitive to context. While in some cases (like here) van Fraassen emphasizes pragmatic considerations, he does not mean to deny the *possibility* of semantics; he does



not mean entirely to deny the possibility of *propositions*, expressed in language, about the world.

Now let's begin with the first challenge. I said he thinks theory selection takes care of the embedding of a data model in the theory model. We should grant that does happen in practice; theories fitting the data are chosen. But it's a red herring. We have not been interested in how theories are chosen. If we were, why not have an explanation of how specific *data models* are chosen, while we're at it? (Such a request would really tickle, for the Achilles Heel of pragmatism is that it can't explain, 'useful *for what?*' It will be hard on his view to explain *why* a scientist should take some data model to be the data model of a phenomenon, since the reason can't include the model fitting the phenomenon independently of the choice at issue.)

No, we should assume there's one theory model at hand, and the data model plugs right in. Now, the question is, has he solved the problem of coordination for *the whole package*, by solving it for the data models? Let us assume with him that the data models represent only a thin observable substructure of the theory. So the theory makes intelligible claims about the world that go far beyond the data models.

(Of course he will insist we needn't believe these claims, we accept them; but again, that's a red herring. The problem is *coordination*. How is the theory structure coordinated onto the world? This must be solved for meaningfulness.)

So it seems that if some claims of a theory go beyond the observable (a point we have said he grants in a materially stronger sense than Carnap does), then those claims need to be coordinated independently of observation—they need additional coordination—to be non-trivially meaningful.

(William Demopoulos raised an analogous criticism against the original constructive empiricism [7, p. 389-90]. He did not write in terms of coordination, which is the novel element of [13], but thought the Newman objection to Russell and Ramsey applies with full force to constructive empiricism, where truth beyond adequacy is trivial, so van Fraassen's abstinence from belief is pointless.)

In this respect I think van Fraassen has dug a fairly deep hole. For his empiricism keeps the observable substructure fairly shallow, and his pragmatic solution to coordination is *by its very character* only applicable to the observable substructure. It is not possible for him to solve the problem of coordination for the complete theory model without breaking his empiricism or changing his solution method.

In an interesting way this puts him close to the Carnap and Worrall discussed above. For he might simply retort that coordination doesn't matter for the unobservable bits; after all, how could we ever tell if it 'went wrong', as it were? Isn't there something mistaken about thinking our theories could be strictly empirically equivalent and yet meaningfully distinct? (Isn't that kind of 'world'

better lost? [18, Rorty] But he explicitly eschews the thought [13, p. 254].)

I don't think he can in good faith make this reply, for again, he *does* think we should take theories at face value; theories do say things about the world, and they might say *different* things, even if we can't observe the difference.

Now for the second problem. It is a little harder to put. Let me start with a comment about (2), then develop (1) and return to (2).

Here is a methodological maxim that I don't think is controversial: if a philosophical tool is very powerful, and is employed in a special circumstance to solve a problem, an explanation of its restriction to that problem is generally called for. As an example, if I invoke teleology to explain the growth of an organism to maturity, I should justify the fact that I don't use it to explain, say, a body falling to the earth.

(Pseudo-)Wittgensteinian 'use' theories are very powerful. They can be used to explain *all meaning*, everywhere. Forget data models, maybe the whole theory is just a game we're playing! Or all of science. Science is an activity, not a source of 'knowledge'. Let's add the user to 3-part meaning relations everywhere, and call the problems (all of them) solved.

Of course van Fraassen wouldn't say that. He wouldn't abandon semantics just because the context and user are necessary to explain certain pragmatic phenomena. So, I say, he needs to tell us *how it is* that the 'use' tool is naturally limited in this case: why this method of explanation is important for data models in particular but isn't needed for everything else in the neighborhood. I can't see how he could do this. (Or, *is* he willing to abandon context-independent meaning altogether? Is that not a different project?)

Alright, now let's take (1) in detail. Notice first the criterion of membership in the 'data model' category. He means for this category to be linked to the 'observable' category, but the criterion should be defined by his usage. The data models are those for which the indexical solution applies: a scientist has a pragmatic tautology that, for example, 'the deer population growth in Princeton' is XYZ. If challenged on the deer population, the scientist will be *confused*, initially assuming the challenge is aimed at data collection methods etc.; realising the challenge is deeply metaphysical, she will say (having read van Fraassen) that it just is the population, as far as she's concerned.

The same game can be played with elementary particle data in the LHC. They collect some data, which *they take* to be data *of* some lepton leaving a hadron collision. They could respond to a similar challenge in kind. The data model just is (for them) data *of* the lepton. The data will be fitted into the theory structures just like data models of deer populations. Granting this much, it seems clear that data models (defined by his usage) actually run very deep into theory, and aren't tied to observation after all.

Let's return to (2). We might have thought that the indexical solution, the

invocation of ‘use’ to solve the coordination problem, at least only applied to observable phenomena, where we tend to think it plausible and safe. Now we see that isn’t so. The whole theory is steeped with it. Are we to say that it’s just *my use* that makes the lepton model a model *of* the lepton? I can agree that a model in the abstract isn’t *of* anything. But is *merely being used* sufficient to make it *of* the lepton, in the relevant respects? I do not see how, unless both leptons (and then deer in Princeton!) are human constructions; but that would be the fancy mythological hermeneutics.

My two challenges are interdependent. The first, and (1) of the second, drive him to break the empiricism and allow the pragmatic explanation even for leptons. But it is evident for leptons that his pragmatic account doesn’t really *explain* the coordination. Finally we can recognise the same failing even for observables; and he can’t escape without giving up semantics altogether.

Before concluding I’ll remark on the indexicality analogy. I think it’s a subtle way of hiding the ‘use’ sledgehammer. Yes, we need some way to locate ourselves, and our experiences, in a theoretical representation of the world. But it should enable us to understand *the whole map*, and not just what the map says about where we are located in it! It is not enough to know where we are, where our observable world is, in the map, if we think the whole map does speak about the world. We need to know how to read maps, *big* maps; how to understand the significance of the rest of their features. *That* is what it means for the map to be coordinated to the world. Why not say it’s just ‘use’ that makes the whole map relevant? Well, we could say that. We could say it’s a map of the world because that’s how we use it. But we might like to say something more. Something about how that works.

## 4

With minor exceptions along the way my argument has been negative. I would like to end with some inchoate musings about the positive direction I’d recommend taking against the problem of coordination.

Often in mathematics a hard problem is solved by ‘making it harder’: by making it a special case of a more general conjecture. The method works by attempting to uncover the ‘real’ problem beneath the case of interest. I would recommend taking a similar tack here.

It seems to me the problem of coordination is not peculiar to scientific structuralism, and it will be a mistake to look for a special solution there. One way to see this is to notice how the amorphous, nebulous nature of the concept of ‘structure’ seems not to prevent or even impede formulation of the coordination problem. (Carnap’s structures are logically-linked sentences; Weyl had

groups and geometries in mind.) This suggests the problem is not idiosyncratic to some definition of structure, and one wonders what element of the concept drives the problem.

I'd like to suggest that the same problem appears with respect to any kind of abstract and discursive knowledge, knowledge expressible in 'general' propositions as opposed to 'singular' propositions about particulars. We saw already from Grover Maxwell the tie between Russell's structuralist 'knowledge by description' and the Ramsey sentence. I think that on closer inspection any kind of knowledge by description will suffer the coordination problem.

If this is true, if the problem is one with the old question of meaning, with the connection between mind, language, and reality, then it will be fair to admit that I can't offer a solution of my own; but at the same time I think those with a generally optimistic outlook who do believe in truth and meaning can safely transpose their confidence to structures in science and fear no problem of coordination.

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