

The Rational Structure of Physics:
What Allows for Consistency with Empirical Reality?

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1. Kant, Friedman and the new synthetic a priori

Consistency in our scientific image is not much of a problem if one is a Kantian: that's the way we must conceive things. But what about the consistency of our scientific image of the world with the world itself? Perhaps, following the teachings of the Vienna Circle, we should take that question to be a senseless one. There is no conceivable way to compare our accounts of the world with the world itself, so there is no proper way to answer the question. Even without this rather operationalist approach to what is and is not sensible one might doubt that the world comes, ala Wittgenstein's *Tractatus*, neatly divided into objects and properties in the right way to underwrite the idea of a match between descriptions and facts.

This worry about talking about the consistency of our scientific image with the world itself certainly accords as well with a concern expressed throughout this volume, that we cannot sensibly talk about the consistency of one thing with another, but rather must consider the consistency of the two under a specific description of each, or as modelled in specific ways. I am happy then to abandon the question in this form and repose it in terms more acceptable: is our scientific image of the world consistent with the world as we construct our experience of it? Indeed I agree that we should do so. This however makes my criticisms of the Kantian approach easier.

Kant thought that reality is preadapted to our forms of cognition in so far as our forms of cognition require a rational structure and that our experience of the world must already be in the terms and patterns of a rational structure. Perhaps so, if we have a loose enough sense of 'pattern' and of 'rational structure'. The concepts with which we describe the world as experienced are indeed repeatable: they apply again and again, in a large variety of loosely systematic ways and there is a good deal of what Hugh Mellor calls 'connectivity' among them.¹ But that is a far cry from the rational structure of our advanced mathematical physics. I have long argued that the rational structures of contemporary mathematical physics do not fit the world as experienced.² They may fit loosely, in limited domains, if we do not look too closely. But there is a trade-off: the more accurate we wish our accounts to be to what occurs in experience, the less they will fit into the rational structure of modern physics. And this is true even if we allow highly sophisticated concepts into our descriptions of experience.

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² Cf. *How the Laws of Physics Lie*

For the Kantian, part of the job of ensuring that our empirical knowledge fits the world of experience is done by the synthetic a priori. This provides the rational framework within which we experience the world. As Michael Friedman puts it, “synthetic a priori knowledge (typified by geometry and mechanics) . . . functions as the presupposition or condition of possibility of all properly empirical knowledge.” (*Dynamics of Reason*, 1999, Stanford, California: CSLI Publications, p. 26) Michael Friedman is keen to resurrect the role of the synthetic a priori, but not as a once-and-for-all framework necessary for empirical experience. Rather each proper theory in modern physics has its own framework that is held, relative to it, as a priori and that makes possible the genuinely empirical knowledge within that theory.

One of Friedman’s principal examples are the three laws of Newtonian mechanics, which are a priori in his sense in the Newtonian scheme as currently understood. The law of universal gravitation – “that there is a force of attraction or approach, directly proportional to the two masses and inversely proportional to the square of the distance between them, between any two pieces of matter in the universe” (*Dynamics*, p. 36) – is the one empirical law in the scheme. This, he points out, talks about *acceleration*. Newton defined acceleration relative to absolute space. Since we do not believe in absolute space, we cannot do this. We say rather that the law of universal gravitation holds in any *inertial frame*, “where an inertial frame of reference is simply one in which the Newtonian laws hold (the center of mass frame of the solar system, for example, is a very close approximation to such a frame.” (p. 36) This is why in our current rendering of Newtonian theory Newton’s three laws must be taken as a priori:

It follows that without the Newtonian laws of mechanics the law of universal gravitation would not even make empirical sense, let alone give a correct account of the empirical phenomena. For the concept of universal acceleration that figures essentially in this law would then have no empirical meaning or application: we would simply have no idea what the relevant frame of reference might be in relation to which such accelerations are defined. (p. 36)

In calling these laws *a priori* Friedman points to a difference he claims in our empirical warrant for them. These are not testable in the same sense in which the law of universal gravitation is because they must already be supposed in order for the concepts in the law of gravitation to be empirically meaningful, which is surely a precondition for being testable. I have two comments on this doctrine.

First if he is right it makes a huge problem for those of us who feel that where the stakes are at all high we should only use laws that have survived severe empirical tests in similar applications. On Friedman’s scheme the law of gravity can be tested – and that’s a good thing since this is an important law used daily in millions of applications. But Newton’s second law, $f = ma$, cannot be tested; it functions as a constitutive principle to give meaning to the concept of acceleration due to gravity. Yet $f = ma$ is at the heart of the applications of Newtonian mechanics. It is how we calculate motions, which are the object of study of mechanics. Moreover, the law we can test is almost useless unless it is deployed in conjunction with the second law. So Friedman’s scheme seems to be just

back-to-front with respect to empirical warrant: the second law, which we can hardly do without in putting Newtonian mechanics to work, is untested, but the law of gravity, which helps us calculate one universal but often insignificant component of the force function needed for the second law can be severely tested.³

This kind of situation was not so bad for Kant, whose elaborate scheme was meant to show that synthetic a priori knowledge is indeed knowledge. But Friedman has no such guarantees. There are of course reasons for adopting a given set of constitutive principles and some constitutive principles are clearly better than others. But on Friedman's view the reasons for the far more widely used second law must be markedly less good than those for using the special-case law of gravity since the law of gravity benefits from all the reasons in favour of the Newtonian system and in addition it has been severely tested.⁴

My second comment is that the need for the kind of definitions that Friedman's constitutive principles underwrite is generated by his vision of the theories in question as rational structures – that is, as a small set of consistent, unambiguous claims (plus their rational consequences) involving precise, well-defined and unambiguous concepts – the system. So, for instance, *acceleration* needs a once-for-all characterization, a single characterization that we cling to across all applications and all tests of the law of gravity; from that follows the need for one single reference frame that is always referred to when the law of gravity comes into play.

But that does not seem to be how it happens. Each use of the concept of acceleration is necessarily tied to a specific frame within which the acceleration is supposed to be measured. This is naturally different when one computes the trajectory of a cannonball from when one computes the trajectory of an electron being bumped up along the Stanford linear accelerator, and different again in every real circumstance. On the view

³ The light principle and the equivalence principle are constitutive principles for GTR according to Friedman. In defending the claim that these are not testable in GTR he points out that the putative tests can have, indeed have had, alternative interpretations. But that seems to be true of any test, whether of a law he counts as constitutive or of one he counts as empirical. He also sometimes speaks of them not being tested *as playing the role they do* in GTR. This remark may hold the answer to my worries. But so far as I understand it, I would dispute it. When we discuss warrant, it is application where it matters, and here I would argue that a responsible view demands that we have warrant, as much as possible, for exactly what is presupposed in the application. If that includes something about the general role of the principle in the theory, then we had better have warrant for that. (For instance, in my own work I treat the law of gravity as an ascription of a capacity and I point out that we regularly test our claims about the strength of this capacity that the law describes. It is, however, a far more difficult thing to test the claim that it is *capacity*. i.e. that what masses do in attracting other masses when no other forces are acting (the conventional test situation) is the same as what they will contribute to the total force when other forces are present. Nevertheless, this claim too must be tested if it is to be used, whether or not it serves to show how the law is treated in the theory.

⁴ Nor does he want guarantees that they are genuine knowledge. Friedman explicitly separates his views from scientific realism; what he looks for is a historical convergence of rational community opinion. Nevertheless we should have warrant for our views, especially, I wish to stress, warrant for their use. Perhaps Kant's synthetic a priori, being something without which we cannot perceive or reason, did have a claim to warrant. The question still remains for Friedman as to how it can be acceptable that our more widely used principles are less warranted than those more narrowly used.

that sees one concept of acceleration defined in a consistent, unified rational structure, the use of each specific occasion-dependent frame requires a series of justifying assumptions. First, acceleration is defined relative to those frames in which Newton's laws are true – of which there aren't any. Next we suppose that defining acceleration relative to the frame of the fixed stars will give a good enough approximation to the values the acceleration would have were it measured in a (non-existent) frame in which Newton's laws are true.⁵ Then we suppose, on each occasion, that the frame we have picked for that occasion should yield values for the acceleration that are close enough to those yielded in the frame of the fixed stars.

I think instead we only make, only need to make and are generally only justified in making one assumption for each occasion of use: in the frame we choose on that occasion, Newton's laws are true enough to support the conclusions we want to draw. That assumption I take it will almost always be more secure than the series of assumptions needed for use of the univocal concept of acceleration required by the rational structure. And where warrant matters, it is to be hoped that it will be supported by reasons far stronger than those supporting that series.⁶ Among these – if we are to trust to our conclusions – had better be a great deal of experience in similar situations and also enough experience to argue that those situations are relevantly similar and to do so without resorting to a highly abstract label that catches, or seems to catch, a lot of diverse phenomena by being thin in content.⁷

2. Rational structures: instruments versus descriptions

Friedman explicitly contrasts his view with what he describes as an *instrumental* view of theories. I am not sure what he means by 'instrumental' but surely my view falls under this description. Theories for the most part do not make claims about empirical reality. They may in their core principles – like Newton's laws and the law of universal gravitation – take the linguistic form of claims. But the claims that constitute the rational structure of physics describe only a world of our imagination, a fictional world that, when interpreted in the right ways can provide a rough template (to use a concept of Bill Wimsatt's⁸) for organizing bits and pieces of the real world. When it comes to empirical

⁵ Note that it is not good enough to assume simply that in the frame of the fixed stars Newton's laws are approximately true, since the closeness of the three laws to the truth is not enough, without further assumption, to guarantee that the values of any particular quantity in the two frames will be approximately the same. The closeness of the first approximation does not guarantee that of the second without more ado.

⁶ Of course some of the reasons we might try to offer at the last step of the series might, cast somewhat differently, be much the same as some of the reasons we give for the immediate assumption that Newton's laws will be well enough satisfied. For instance, that we are in a frame whose rotation will have negligible (for these purposes) effect, an assumption that can be made without having to fix the rotation to the fixed stars.

⁷ "Force" for instance is such an abstract label. It gets (more) concrete content only when it is fleshed in with a description of what constitutes the source of the force, like a particular arrangement of charges or the presence of a massive planet. See 4.1 below.

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reality, this rational structure is best viewed as a tool for constructing the million-and-one genuine claims that make up the body of our physics and engineering knowledge.⁹

In so far as we should think of physics theories as making claims, the claims – or, the *warranted* claims – are severally complex and not simple to state even given a mix of various theoretical and practical languages (say those of quantum and classical physics and of laser engineering) and of mathematical forms of representation. They constitute a vast interrelated network – a tangle – with hugely overlapping family resemblances. These resemblances depend on shared vocabulary; shared basic structural forms, especially those provided by what are usually identified as ‘the fundamental laws’ of the theory; shared techniques; and analogies not only of form but of content.

The image of theory that I support is similar to the picture of theory-nets endorsed by the German structuralists. Except that for the structuralists generally all the claims in the theory-net fall under and are consistent with the basic laws, where the basic laws are thought of as making general claims in abstract language. But looking at what in fact happens, they quite apparently do not; and it is a huge stretch – a leap of faith, I would say – to think that somehow most could be made to fit if only we knew more, could calculate better... (Here we see the idea that the failures of fit are all due to post-lapsarian sinfulness rather than to genuine diversity in the world.) The picture I paint is far closer to what I see whenever I look at physics theories in use. This matters because it is only the theory as used and tested that can be counted as warranted.

3. *Warrant*

I speak about the *warranted* claims of physics; it is the demand for warrant, stringent warrant where stakes are high, that drives my image of theory in physics. I call the conventional view about warrant for claims in the theory-net the *ghost-train view*: We have inductive evidence of various kinds for the principles of the theory. The abstract language in which these principles are cast carries the warrant silently and swiftly to new cases far away and far different from the original inductive base. This seems to me a dangerous picture and one that we do not take seriously when there is much to lose if our verdicts about the new case are wrong.

Consider a well-known historical case. Newton insisted that mass is mass, whether terrestrial or celestial. He thereby allowed us to apply the same abstract concept to both. This was a huge conceptual breakthrough that provided the tools – the concepts, the basic equation forms, the mathematical techniques and so forth – for treating planets and cannonballs in analogous ways. But calculations of the trajectories of cannonballs are hardly warranted by observations on billiard balls, let alone by observations on the locations of the planets. If it really matters to us where our cannonballs will land, our confidence in the calculations for their trajectories should be based on a great number of successes, as well as our failures and our corrections for them, on vast numbers of trials. It is only then that we have good (enough) reason to suppose that *this* is a kind of case

⁹ For a somewhat lengthier discussion, see Cartwright, Suarez and Shomar (“The Toolbox...”)

where Newton's laws and Newton's concepts provide a central tool and that we have made the right use of it.

The view here goes along with my earlier arguments about the choice of frames of reference. If we are going to use Newton's law then we need good empirical evidence that they are accurate enough in the frame we have chosen, and accurate under just the specific detailed interpretation we are giving them in this case. The kind of empirical evidence we need, and the kind we generally insist on for practical belief, belief where it matters, is evidence in situations very like the one to hand, with very much the same kind of claim at stake.

I do agree with the conventional picture that warrant is carried from old cases to new cases by inductive generalizations. But I want to keep firmly in view that small inductions are more secure than grander ones and detailed local experience – trial and error, with both successes and failures – really matters.

One of the results of these truisms about induction is that we do not really know what is in the theory-net – what is a warranted claim of the theory – and what is not. Knowledge of what constitutes the theory does not lie with the theorists. Instead it is dispersed among the vast number of those practitioners who put the theory to use to model concrete situations. Only those who grapple with the attempt to construct local models will know which are warranted and which are not, hence severally what are and are not acceptable claims of the theory. This is a far different story from that of the rational structure, the axiomatizable or even axiomatized structure, that is there for all with the requisite mathematical skills to know. What the theory says is no easy matter to find out.

My own image of our attempts to carry warrant in the conventional way is very different from transport via the ghost-train. First, the inductive climb up is insecure; for our best theories there are a lot of routes, but none of them is all that stable or secure. Then the route across even within the theory itself can be shaky. And getting down again to the concrete details of the new case is usually far more insecure and slippery even than the inductive climb up into the theory.

Defenders of the rational structure may seize on this picture in their own defence. It is, or so many say, not the rational structure that is insecure but rather the links to more concrete, more usable, descriptions of situations. This is a response that I do not understand. When the links are insecure – we are not sure of them, we make do, we need to make *ad hoc* adjustments, approximations, improvements, often even just guesses – then so too is the conduit for warrant. The principles of the structure cannot be warranted by an induction on behaviours that are not clearly known to fall under them. The situation is even worse for warranting claims that we are not really sure follow from this structure.

Friedman is explicit that he is not interested in realism. But the disagreement between us about what theory is has a familiar form from the realism debate. The philosophical thesis that the rational structure is useful in just the way we find it to be in constructing claims that can be highly warranted is weaker than the thesis that the rational structure itself

makes highly warranted claims, and it has all the same evidence in its favour without having to make excuses that lack solid evidence for many and pervasive failures.

4. *An alternative locus for constitutive principles*

According to Friedman constitutive principles, like those defining the frame of reference of the concept of acceleration, make the “empirical application of the theories in question first possible”.¹⁰ (*Dynamics*, p. 49) I think this is a misdescription. The principles that Friedman calls ‘constitutive’ make the concepts *intelligible*, not *empirically applicable*. They provide univocal and precise definitions that fit the concepts into the relevant rational structure but they are not the principles that make possible the empirical application of these concepts.

When it come to the presuppositions for empirical knowledge using the concepts of physics, as opposed to presuppositions for fitting these concepts into the rational structure, I think there are three better candidates:

1. *Bridge principles*. Many concepts in physics are abstract relative to another specific set of more concrete concepts. *Force*, for instance, is abstract in the sense I mean: it never applies to an empirical situation except via some one or another of a collection of more concrete descriptions. Bridge principles spell out the link between the abstract and the concrete. The law of gravitation, for instance, tells us what form the abstract concept *force* will take – Gm_1m_2/r^2 – when we can suppose that the more concrete description “one massive object is located a distance r from another” holds; the law of electromagnetic attraction and repulsion links the form $\epsilon_0q_1q_2/r^2$ for the force with the concrete description, “a charge is located a distance r from a second”; and so forth.¹¹ (Notice that this picture is just opposite to Friedman’s. He takes Newton’s three laws to be constitutive, the law of gravitation as empirical. I take the three laws to be empirical and the law of gravitation to provide empirical content for them.)

These (more) concrete descriptions are in a richer vocabulary that allows in addition for more liberal descriptions that do not link by bridge principles with the abstract concepts of the rational structure. Hence we can include “additions” and “corrections” that do not fall under the abstract theory in any grounded way (i.e. in any way that is not “ad hoc”).¹²

2. *Measurement auxiliaries*. When we tie our physics concepts to empirical reality by measuring them in a specific circumstance, our procedures presuppose the accuracy of a

¹⁰ This of course, as he points out, does not guarantee that the empirical principles that we formulate using them will be true, just that they are candidates for truth or falsity.

¹¹ For more on this see NC’s *The Dappled World*.

¹² Note though that the more concrete vocabulary that fits out the abstract concepts of high theory is not all the vocabulary available for the formulation of empirical knowledge about the systems that physics helps treat. It does not, for instance, include names of materials nor much of the many technical vocabularies of engineering and technology. It should be no surprise that the most accurate expressions of empirical knowledge are in a rich mixed vocabulary.

host of laws about the situation that justify the measurement techniques. This is already a much-discussed topic in science studies and I won't pursue it here, except for one caveat.

One might want to argue about measurement auxiliaries that they are not constitutive in Friedman's sense: they are not presuppositions whose correctness is necessary for the *applicability* of the concepts in question, but only for our ability, case-by-case, to measure them. The concepts still apply, but we would be very limited in our ability to measure them. This contrasts with the case of bridge principles since, without bridge principles abstract concepts – i.e. concepts that require more concrete descriptions to hold – would not apply to empirical reality but only have a place in the rational structure. The third candidate, to which I now turn, also has more claim to the title “constitutive” than do measurement auxiliaries.

3. *Representation theorems.* We represent features of the empirical world with specific mathematical forms that have specific properties. These forms tend to be far more universal across applications than is any (univocal) interpretation or definition of the related concept. *Acceleration* for instance: no matter what frame of reference we define it relative to, we almost always represent it as d^2x/dt^2 . So length itself must be represented as a quantity twice-differentiable with respect to time. But it also has a number of other built-in features as well. Probably the simplest is that length is represented by an additive measure. Can a mathematical representation with these characteristics adequately represent the phenomena to be associated with “length”?

The answer depends on the structure of the phenomena to be represented. In the case of length, the phenomena might include what happens to sets of measuring rods. For instance, as Patrick Suppes puts it¹³, the collection A of rods is longer than B “if and only if the set A of rods, when laid end-to-end in a straight line, is judged longer than the set B of rods also so laid out.”¹⁴ Formalizing that fact, along with a couple of other obvious features we ascribe to the empirical concept of length (for instance, that any collection of rods is at least as long as the empty set), we can characterize the structure consisting of the set of rods and the longer-than relation as a *finite equally-spaced extensive structure*.

Now we are in a position to show that an additive measure is an appropriate representation for length by proving a *representation theorem*. In this case the theorem tells us that for any finite equally-spaced extensive structure, there is an additive measure μ such that for every pair of sets of rods, A and B, $\mu(A) \geq \mu(B)$ iff A is longer than B. That is only a start of course. In order to guarantee the empirical applicability of the concept of length as we represent it in mathematical physics, we need a representation theorem relating all the qualitative features we assign to length to its mathematical representation. And similarly for all the quantities of empirical reality and their features for which we provide mathematical representations.

It is, then, I urge, in the representation theorems for the mathematical representations we offer in physics that we find our best candidates for “constitutive principles”. This is true

¹³ *Representation and Invariance of Scientific Structures*, 2002, Stanford, California: CSLI Publications.

¹⁴ P. 64

for both my image of the theory-net (or “tangle”) as well as for Friedman’s image of the rational structure of theory. Representation theorems are the preconditions for the application of our concepts to empirical reality. Our representations are consistent with the features we ascribe to empirical reality only if the appropriate representation theorems are true.

5. Conclusion

Rationality requires consistency within the system – within the abstract theoretical structure. Warrant for applying a theory requires consistency of the system with the empirical world. Kant thought that reality is pre-adapted to our forms of cognition. As an empiricist looking at the successful and unsuccessful tests and applications of contemporary mathematical physics, I do not find that claim well supported. We have, I am afraid, two demands for consistency that pull in opposite directions.