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Author(s): Lawrence Sklar
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I’d Love to Be a Naturalist—if Only I Knew What Naturalism Was

Lawrence Sklar[†‡]

Naturalists tell us to rely on what science tells about the world and to eschew aprioristic philosophy. But foundational physics relies internally on modes of thinking that can only be called philosophical, and philosophical arguments rely upon what can only be called scientific inference. So what, then, could the naturalistic thesis really amount to?

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1. Naturalism.

What is there in the world? And what is it like? “Let science be your guide.” Let’s call this “naturalism.” This slogan has both a positive and a negative side. The positive side is the assertion that the results of science are adequate to answer all of our ontological questions. The negative side is the denial that there is anything to rely on outside of science that can be a guide to our ontology.

But can we rely upon our best available science to provide us with a guide to what there is? What about the well-known philosophical skepticisms that entice us to withhold belief in even our best scientific claims? If scientific theories are established by inductive reasoning, we have the problems of the justification of induction and of the rationale for deciding which predicates to project. If they are established by inference to the best explanation, we have the objections to the method of hypothesis (it is just affirming the consequent after all), familiar since critical attacks on ancient astronomy and physics. And if our best science purports to tell us about unobservable entities and properties, doesn’t the long empiricist tradition provide us with objections semantic and epistemic against such hubris? On top of that there are the varieties of overall skepticism

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†To contact the author, please write to: Department of Philosophy, University of Michigan, Ann Arbor, MI 48109; e-mail: lsklar@umich.edu.

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that have repeatedly challenged our claims to knowledge, or even justified belief, scientific or not.

The naturalist's reply is clear—"Let them fret." If science, and only science, is our guide as to what to believe, it is also our only guide as to what to disbelieve. And it is our only guide to our doxastic and epistemic evaluations of our beliefs, our only guide as to when belief is reasonable and when it is not. If we are to be skeptical of some scientific claim, that skepticism must arise out of science itself and not out of aprioristic philosophical doubting.

The naturalist will be even less worried by wild claims of inherent perspectivalism in our theory framing and justifying modes that force us into some variety of social relativism that makes any serious belief in our scientific ontology dubious. Here, the naturalist will contend, we meet the eschewed philosophical apriorism blended with dubious social and psychological science. Hardly a reason for deep worries about the reliability of our belief in our naturalistic ontology.

But what about the notorious problem of the transience of our best accepted theories? We have the famous induction from past experience that accepted theories have their day in the sun and then are discarded in favor of improved science. And we have the quite persuasive arguments that the changes from older to newer fundamental theory often are radical in nature and force us to dismiss the ontology posited by the older theory as even some approximately correct description of the world. Worse yet, our current fundamental physics consists of a number of theories that cannot all be true since they contradict one another.

One could respond with the hopeful but, perhaps, dubious suggestion that the "maturity" of our current science gives us assurance that our current quantum fields and curved space-times won't go the way of crystalline spheres, caloric and phlogiston. Or we could modify our naturalism to some Peircean version of having the final, future, unchanging theory as the true guide to our ontology. Or we could play with some modification of our appropriate doxastic attitude taking ourselves not as believing in the ontology of our current theories but as believing their ontology a well-framed step on the way to a scientific ontology we could truly believe in.

2. Interpretations. But there is another problem for the naturalist, one that continues to nag even if we are satisfied that current science is good enough to direct us to our accepted ontology. Now science consists of lots of different kinds of theories. Some of them are fundamental and some derivative, phenomenological, or "special." And we are now all sensitive to the fact that simple-minded accounts of the ultimate "reducibility" of all the nonfoundational theories to the foundational are dubious. So in any account of our naturalistic ontology we will need some deft handling of the problem of how to fit the ontology of the special, limited, and nonfoundational sciences into the world to which we are directed by the foundational theories. Where, exactly, do apples and stock options belong in a world of relativistic quantum fields?

Still, there are very good reasons for focusing on our most fundamental theories when we are asking questions about what there is in the world. To start with, we should take as the entities and properties of the universe those entities and properties our most general and most foundational theories tell us are there. We can sort out the problem of how to think about the apparent ontologies suggested by the more limited and less foundational theories later.

But now comes the kicker. We often don't have the faintest idea what entities and properties are being posited by our foundational physical theories. It is just those fundamental theories that are notorious for leaving us befuddled as to what kind of a world they are talking about. Our foundational theories usually exist in a scientific framework in which they are subject to multiple, apparently incompatible, interpretations. And given the interpretation you pick, your view of what the theory is telling us about the basic structure of the world can be radically unlike that of someone who opts for a different interpretation of the theory. And this is nothing new to foundational physics. Throughout the history of the science the foundational theory preferred at the moment has always been subject to the problem of multiple interpretations.

But what are interpretations of theories? Why do theories need interpretations? What gives rise to the problem of interpreting a theory in the first place? If interpretations are needed, how are they discovered (or are they created)? And when they are proposed, how are they to be evaluated for their correctness or adequacy or whatever it is that is supposed to be the positive value to be attributed to good interpretations and denied bad ones? Finally, and this is what I want to puzzle over here, what are the consequences for naturalism of the fact that foundational physical theories only exist in a morass of contending interpretations?

3. Motivations for Interpretations. First, it is crucial to observe that the demand for interpretation arises within theoretical science. It isn't something imposed on the scientist by some aprioristic philosopher. Yes, scientists themselves may have been corrupted by exposure to the specious demands of philosophy and imported this unnecessary puzzlement into their views on their science. But it would be hard to claim, historically, that it was merely an unfortunate education that included philosophy that left Newton puzzled about the nature of his needed absolute space, or Boltzmann in a quandary over the origins of temporal irreversibility in an atomistic and mechanistic kinetic theory, or, to invoke the most puzzling case of all, that drove de Broglie, Heisenberg, von Neumann and Schrödinger, and even Bohr and Einstein, to distraction over the proper interpretation of quantum mechanics.

The need for interpretation arises when, within science, there is a belief that something has gone wrong in our theorizing. What can go wrong takes on a very wide variety of guises.

1. Our theory invokes mathematics unacceptable to anyone concerned with mathematical rigor or consistency (Heaviside's notorious calculus or the invocation of delta functions in quantum mechanics).
2. Our theory makes "crazy" empirical predictions—not the sort of merely erroneous predictions that demand new theories in the ordinary sense—but bizarre consequences that suggest some kind of new way of looking at our existing theory (the infinite divergences in both classical and quantum field theories, the ludicrous vacuum energy of quantum field theory).
3. Our theory requires the positing of parameters that the theory itself tells us can never be determined by any observation or experiment, even if we use the full resources of the theory to infer parameter values from empirical data (absolute velocities in Newton's dynamics or absolute values of cosmic uniform gravitation in a Newtonian gravitational theory).
4. Our theory seems to lack an essential ingredient needed to derive a consequence we feel ought to follow from the theory. Indeed, the theory has fundamental posits that seem to make the derivation of this consequence impossible (the need in kinetic theory to derive a time asymmetric behavior of entropy that seems to conflict with the underlying time symmetry of the dynamical laws on which kinetic theory is based).
5. Our theory needs to posit a fundamental feature of the world that seems to be incompatible with its other general postulates (positing initial low entropy for the Big Bang in statistical mechanics when the general assumption is of thermal equilibrium for initial states of isolated systems).
6. Our theory implies features of the world that seem to be at variance with general features of the world well established by empirical experience and that fit harmoniously with our other physical theories (the apparent action at a distance in Newtonian gravitational theory, for example, or, more profoundly, the apparent correlation of outcomes at a distance entailed by quantum mechanical entanglement).

7. Our theory seems to require a basic process in the world that appears to have no natural place in a world described by that theory (the measurement process understood as involving nonunitary collapse of the wave function in standard quantum mechanics).
8. And this needed basic process, worse yet, seems plainly contradictory with universal nature of processes in the world as described by that theory (measurement in quantum mechanics again).

This is just meant to be a sampling of the kinds of internal difficulties faced by foundational physical theories within the science itself. One could certainly come up with other puzzling conceptual anomalies within theories that similarly leave us perplexed as to how such a theory could truly be a description of what our world is like.

4. Kinds of Interpretations. Even a quick glance tells us that there is, at best, a family resemblance structure to the kinds of problems that demand interpretations for our foundational theories. The problems that interpretations are meant to address range from the seemingly technical, and in some cases, fairly simple and almost trivial, to those that give us grounds for serious worries about the very possibility of our finding a version of the theory that could play any kind of a stable role in our scheme of foundational theories.

And so, not surprisingly, the interpretations that are meant to address these problems also form a wildly varying lot. At one extreme are the programs designed to clean up the nonrigorous mathematics utilized by some theory. A typical example is the elimination of delta functions in quantum mechanics either by using distributions or by using spectral decomposition of operators. As Heaviside once said, "Even Cambridge mathematicians deserve justice" (1894, 10). There's not much here to bother the members of any philosophical camp.

At the other extreme, interpretation becomes hard to distinguish from "finding a new theory." Einstein, for example, frequently insisted that what was needed was not an interpretation of quantum mechanics, but the development of a new theory altogether, a new theory in which the conundrums of measurement would never arise. He dismissed Bohm's hidden variable approach, for example, even though it restored determinism to a quantum mechanical world, since the Bohm theory was, for Einstein, too much interpretation and too little theoretical innovation. The Ghirardi-Rimini-Weber interpretation of quantum mechanics, with its invocation of a stochastic subquantum level throwing systems into near-eigenstates, is an interpretation that is also the invocation of a new theory that goes beyond the existing theory that is crying out for interpretation. In this case the new interpretation goes so far as to make distinct empirical predictions from the original theory being interpreted.

Most interpretations, however, fall between the two extremes of mere mathematical "cleaning up" and the positing of a new theory altogether to replace the old problematic theory. These interpretations cannot be dismissed as merely clean-up jobs on some nonrigorous mathematics. Nor are they anything like the replacement of an existing theory by a novel theory. For one thing, those who present these intermediate interpretations often insist that their interpretation of the theory suggests no testable empirical consequences that differentiates it from earlier interpretations of the theory. On the contrary, they are alleged to be superior interpretations on systematic or conceptual grounds alone.

Here is a question that has, I think, not received anywhere near the attention it deserves: Are there interesting classes or kinds of interpretation that have structural features in common and whose advocates utilize similar arguments that cut across a wide range of specific theories in trouble and specific problems these theories face? I think that there are such general interpretive programs and that it can be useful to look at some of the common aspects of interpretation, while still recognizing that every unhappy theory is unhappy in its own special way.

5. Empiricist Interpretations and Space-Time Theories. The most glaringly evident family of interpretations are those that go something like this: There is something profoundly wrong with our theory as it is usually understood. But this isn't its failure to make adequate empirical predictions; it is purely a conceptual failing. Furthermore, that problematic aspect of the theory is a difficulty that occurs only at the level of the theory that deals with entities and properties that are, by the theory's own lights, forever immune from observational determination. Perhaps the solution is to focus on those parts of the theory that do have consequences that are empirically testable, and then to look to see if some refinement of our understanding of that which goes on at the purely nonobservational level can clean up our conceptual difficulties while leaving our success at the level of empirical prediction alone.

One version of such refinement at the nonobservational level is the systematic thinning out of the nonobservational structure of the theory. For, in a variety of cases, it is alleged, it is the existence of "too much structure" in the theory its usual interpretation that has led us into the conceptual difficulties.

This kind of interpretive program shows up, again and again, in attempts to deal with conceptual problems in theories of space and time. Curiously, much of the framework in which these interpretive programs take place was already present in Poincaré's notorious defense of the claim that Euclidean geometry could be retained on an a priori basis even in the face of empirical data that seemed to tell us that we live in a non-Euclidean spatial world. Our empirical data, Poincaré tells us, deal with coincidences between point of material rods, and, perhaps, intersections of light rays. And any pattern of such material coincidences can be fitted into any spatial geometry you like if you are willing to make enough changes in your assumptions about the relation of the behavior of the measuring device to the structure of space. You might do this, say, by allowing universal stretching-shrinking fields for the measuring rods and universal path bending fields for the rays of light. Poincaré went on to tell us that we would, therefore, never abandon Euclidean geometry for our conventionally chosen geometry of space.

The real interpretive problems are about theories of space-time, not theories of space. And not all of them have to do with the issue of flat versus nonflat geometries. Furthermore, the last thing the real interpretive programs want to do is to entrench the old and familiar against empirical refutation by means of programmatic gimmickry. Instead, the fundamental aim of the programs is to resolve the conceptual difficulties with the existing theories by developing more radical interpretations of them. And these radical interpretations all work in exactly the opposite direction of Poincaréan invocations of universal distorting fields. Rather, they propose getting rid of the excess structure of an existing space-time theory as the price to be paid for conceptual clarity. Let us look at some of these programs, using a (more or less) historical order in which they occurred, even at the cost of some conceptual tidiness.

Let's start with the special theory of relativity. Remember the positivistic elements in Einstein's critique of the notion of absolute simultaneity for distant events. A relativized notion of temporal duration that comes along with relativized simultaneity fits with a relativized notion of spatial separation for nonsimultaneous events already familiar to physics. Together they give one a compatibility of the equivalence of inertial frames with the invariance of electromagnetism and especially the invariance of light speed. No claim is made to the effect that novel empirical predictions differentiating this theory from the compensatory aether theories of Lorentz, Fitzgerald, and Poincaré follow from this new theory (notoriously leading Whittaker to relegate Einstein to a footnote to his chapter "The Relativity Theory of Poincaré and Lorentz" [1951]).

But suppose we ask why we should accept the Einstein theory as opposed to that of Lorentz, or, should we say, accept the Einstein interpretation as opposed to the Lorentz interpretation—even though, of course, the Lorentz account had much to do also with the electronic structure of matter and all that? Surely a natural reply is to point out that in the Einstein account there is no aether frame relative to which apparent light velocity is real light velocity. And since there is no empirical method for determining which of the inertial frames *is* the aether frame within the compensatory account, we are well rid of that base frame for electromagnetism. Special relativity is better than the compensatory theory since, while not differing from the older theory in its empirical consequences, it rids physical theory of a value for parameters in a theory which are, on that theory's own terms, beyond empirical determination.

Struck by the success of Minkowski space-time as the proper home for special relativity, one can go back to earlier physics that has an undeterminable parameter just like that eliminated by Einstein and see if one can get rid of that bad parameter. The first role for an absolute rest frame was, of course, that espoused by Newton, who thought he needed absolute rest in order to make sense of absolute acceleration. And absolute acceleration had more than enough empirical consequences to make it essential to dynamics.

But once one thinks in terms of point event locations as the irreducible components of a space-time, one can see if one can perform some sort of Einstein-Minkowski maneuver to rid Newtonian dynamics of absolute rest and its concomitant, empirically undeterminable, absolute velocities. And, of course, you can. Keep Newton's absolute time with absolute (up to a linear transformation) time intervals between events. Take space at a time as any fully extended class of mutually simultaneous events and posit that this space is flat, Euclidean three-dimensional space. Drop the Newtonian idea that one can say of nonsimultaneous events what their spatial separation is, so that one can no longer speak of what amounts to "the same place at different times." With that gone so are absolute velocities. But don't get rid of absolute acceleration. Retain that by introducing a colinearity relation that may or may not hold among any triple of nonsimultaneous events. That is enough to restore the affine structure of the space-time needed to make the notion of inertial frames well defined.

And what makes this Galilean (or neo-Newtonian) space-time better than Newton's? Once again it certainly isn't any improvement in empirical adequacy. By locating the conceptual difficulty with a theory in the realm of the empirically unobservable (whether that be distant simultaneity as in special relativity or absolute velocity as in Galilean space-time) and by thinning out the useless structure of the theory that is all at the unobservable level, we get a better interpretation of an existing theory.

In interpreting general relativity we seem the same strategy at work. Beyond question general relativity is an empirically novel theory compared to Newtonian gravitation. Gravity's effects on dynamics of moving particles is modified; an effect on the travel of light is predicted that differs even from a naive Newtonian prediction that would arise from attributing mass to light, and, most shocking of all, the new theory predicts its famous metric effects on space and time. But does the theory require a curved space-time, the guise in which it usually appears? We know what Poincaré would say: put in enough dynamic, optic, and also stretching-shrinking and clock speeding-slowing fields, and flat space-time will do, as far as empirical consequences are concerned.

So why is general relativity with its curved space-time a better theory (or interpretation of a theory) than its flat space-time empirical equivalents? The best answer is now the familiar one. Many empirically equivalent possible worlds within the class of allowed possible worlds of the flat space-time surrogates for general relativity collapse into a single curved space-time in the interpretation of general relativity. And, surely, we ought to prefer the theory that eliminates the nastiness of empirical underdetermination of worlds posited by a theory on the theory's own terms.

Once again, having learned the lessons of the modern theory—in this case general relativity—we can apply its conceptual lessons to clear up ancient worries about an ancient theory. Just as Newton was aware of the empirical inaccessibility of absolute velocity (Corollary V to the Laws in the *Principia*), he was also aware of the empirical inaccessibility of uniform fall of a system in a gravitational field (Corollary VI to the Laws). He needed Corollary VI to justify his application of the Laws of Motion to the Jovian system of Jupiter and its moons, since, after all, that whole system was in accelerated motion about the sun. In this case the empirical nondeterminability of a parameter is due to the strange nature of gravity as having inertial mass as its charge, a fact whose importance was realized by Galileo, Newton, and Einstein. Specific paradoxes of Newtonian gravity became well known. Maxwell noted in the late nineteenth century that a Newtonian cosmos could not be empirically distinguished from that same material cosmos embedded in a universal, uniform gravitational field (1954, 85). And, more trenchantly, models of the universe as uniformly filled with a dust of matter of constant density would have observers at every point declare themselves central to the universe. They would claim that all other observers were deceived in that, while they were really accelerated, their accelerometers would declare that they were not, due to the special nature of the gravitational acceleration of an object as independent of its constitution and of its size.

But having learned general relativity, clever theorists could go back to Newton. Take the Galilean space-time already noted. Keep its absolute time and its flat, Euclidean three-spaces at each time. But drop straight-line inertial paths as its timelike geodesics, replacing them with timelike geodesics as the paths of "free" particles not acted upon by any "force" other than gravity. Once again the virtue of the move is clear. The undetectable accelerations of Newton vanish away. All the Maxwell universes become one universe, and in the dust-filled cosmology all apparently equivalent observers really are equivalent free-fall observers.

Finally, again in general relativity, there is Einstein's response to the "hole" problem. Take a region of space-time devoid of ordinary matter-energy. Fix the metric of the remaining space-time outside the hole. The metric remains indeterminate within the hole, threatening general relativity with indeterminism. But, as Einstein noted with relief, the behavior of measuring devices sent through the hole and observed outside it remains invariant under these in-hole metric shifts. And what is general relativity, really, except a theory devised to make just such predictions about free particle paths and light ray paths and their intersections and about the readings of clocks transported along various timelike paths? Technically, take general relativity to be a theory of equivalence classes of diffeomorphically equivalent space-times, and not of space-times themselves.

So the interpretive scheme we have been looking at applies again. Eliminate a degree of underdetermination that is trapped at the level of unobservability by going to the observable and looking for a way of thinning the ontology down at the level of unobservables while keeping the empirical predictions of the theory constant.

6. Empiricism Elsewhere in Foundational Physics. The examples I have just discussed show that there is a general type of interpretation that is applied in a number of distinct cases of dealing with issues in space-time theories. But is any interpretive program of the same type ever employed outside of the space-time cases? Yes and no. Interpretive programs that have some elements in common with the one discussed so far do appear elsewhere, but, not surprisingly, these nonspace-time interpretive programs have aspects that differentiate them sharply from the program as applied to space-time theories.

The theory that screams out loudest for interpretation is, of course, quantum mechanics. And some of the proposed interpretations are quite wildly different from those we have been looking at in their structure. Positing many worlds to deal with measurement, or positing local hidden variables and instantaneous nonlocal causation to deal with entanglement, enriches the unobservable structure of the theory rather than thinning it down. But, perhaps, we can find some elements in common with the space-time interpretations in Bohr's attempt to deal with the measurement problem. The program there with only the observable as the really real, with the observable now being the classically characterizable behavior of "measuring instruments," and with the relegation of the unobservable (the wave function) to a realm of a merely correlation tracking device for propensities that are actualized probabilistically in measurement, has at least some overall elements in common with the space-time programs.

The interpretative program here differs markedly from the program we just looked at in the space-time cases. The observable/nonobservable contrast is no longer that between material objects and space-time itself. Nor does locality—restricting observables to coincidence relations—play a crucial role. Instead, the contrast now is between "classical" measuring instruments and quantum conditions such as those summed up in a wave function. And the aim of the interpretive program is no longer the elimination from the theory in question of parameters whose values remain, according to the theory itself, eternally immune from observational specification. Now, rather, the aim is to find a way of characterizing the ontology of a world described by the theory that resolves the excruciating puzzles of trying to make sense of just what kind of properties a system can have when it is characterized by the theory as being in a superposition of ordinary properties.

More recently, at least one proposal to deal with nasty problems in the foundations of quantum field theory has, once again, resorted to interpretive moves that have at least a family resemblance to those applied in the space-time cases. When you have a theory with an infinite number of degrees of freedom, the restraints of the commutation principles no longer will guarantee that any two representations that satisfy them will be unitarily equivalent. This implies that they will no longer have the same expectation values and so won't be empirically equivalent either. Many proposals have been made about how to deal with this situation, but at least one interpretive programs seeks to find a restricted class of observations, those that are appropriately local in some sense and that are also appropriately finitistic, on which all the inequivalent representations will agree. So, once again, focusing on some notion of a limitation of the consequences of the theory that are to count as properly observable, and looking to thin out those portions of the theory whose content doesn't play a proper role in establishing correlations among those observables, is central to an interpretive program.

7. Philosophy within Physics. What I want to focus on here is not the detailed nature or the satisfactoriness of any of these interpretive programs. Rather, I want to emphasize the pervasive elements in the interpretive programs that are of a "philosophical" sort.

A general pattern to the space-time class of intrascientific interpretive programs is clear. A profound conceptual difficulty is noted in the theory. An intuitive distinction is made between the observable consequences of the theory framed in the concepts of the theory dealing with observables, and the, in principle, unobservable consequences and concepts. The assumption is made that the conceptual difficulties are all “trapped” at the unobservable level. Then, against the background of an implicit assumption that it is really the observable consequences of a theory that count, a move is made to eliminate the conceptual difficulty by a “thinning out” of the obnoxious parts of the theory at the unobservable level, or by retaining them and accompanying the theory by an interpretive “annotation to the reader” about how the undeterminable parts of the theory are to be properly understood as not fully on a par with the determinable elements.

Where does the observable/unobservable distinction come from in these programs? There is no question that the theory itself plays a crucial role. Once you have implicitly assumed that relative motions of material objects are, in the context in question, to be counted as observables, and that motion relative to “space itself” is not, then, yes, it is Newtonian dynamics itself that tells you that absolute velocity is undeterminable in the theory. But the initial assumptions about the observability of relative motions and the unobservability of absolute motions hardly comes from Newtonian dynamics itself. Do all of these assumptions come from “science” in some more general sense then? Say from a science that includes details of the construction of our sensory apparatus and the causal relations from external world to these? Or are they imported into the scientific discussion from vague portions of empiricist philosophizing? Or, rather, is it really possible to discriminate between such vague and general background “science” and traditional empiricist philosophy here?

Notice, in particular, the recurring idea that the observable is restricted to the local. Even if one constrains observability to relations among material measuring instruments, denying it to space-time features themselves, a further restriction is applied in all of these familiar interpretations of space-time theories. It is coincidence features of the relations of material objects to one another, relations that are well defined at a point, that are taken as the only true observables. Does that constraint come from our scientific account of observation, or is it, rather, a grand philosophical presupposition that is being imported into our interpretive program within science here?

Next consider the “within science” modes of reasoning that are applied once some context-relative observational/nonobservational distinction has been drawn. In the space-time cases one principle is used over and over again. This is to prefer theories that are adequate to the data and that contain no nonobservational parameters whose values, on the theory’s own terms, cannot be fixed by any observational facts whatsoever, to theories, observationally adequate though they may be, that contain such idly floating parameters. Alternatively one can take the rule to prefer the theory that allows only one model of the world relative to a totality of possible observational data, to a theory that is observationally equivalent but that leaves a manifold of models as possibilities for the cosmos even after all possible observational data is accounted for.

What kind of rule of inference is this? Well, it tells you to prefer the simpler theory to the less simple when the alternatives on offer are observationally equivalent. Reichenbach would, at this point, insist that we were talking about "mere descriptive simplicity" and not "inductive simplicity" (1958, 35), since the hallmark of that latter, genuine, simplicity would be the empirical refutability of the choice of the simpler theory over the less simple. And indeed, some, reflecting on the empirical equivalence of the theories, might understand the choice made as merely being one of expression or representation. But others, of a realist bent, would not, taking the choice to be one on a par with other choices of one theory as more plausible than another *inequivalent* to it that are made throughout theoretical science.

The point here is that it seems pretty implausible to claim that it is "science itself" that tells us, in an internal way, that such simpler theories are "more likely to be true," or are preferable for some other reason, than their less simple alternatives. But this is the kind of reasoning that is found, explicitly or implicitly, throughout philosophy with a traditional empiricist orientation. Some aspects of reasoning in science do seem to have rationales that rely in part on internal science—the choice of which predicates are projectible in inductive reasoning in science, for example. But here the grounding of the reasoning seems as philosophical as such grounding can be.

8. Science within Philosophy. If science, then, is internally full of motivations, presuppositions, and strategies that remind us of what goes on in the minds of traditional empiricist philosophers, that philosophy is also replete with features that must, by any reasonable standard, be considered scientific.

Take a look at the famous conclusion of many philosophers that the contents of our immediate perceptual awareness are not features of physical objects themselves but, rather, features of "ideas in the mind," or some variant of that. This is a view shared by Descartes, Leibniz, Locke, Berkeley, Hume, Mill, Poincaré, and Price. Whether they be representative realists, idealists, or phenomenalists, all agree that what we immediately perceive are ideas in the mind, or percepts, or sense-data.

But why should we believe such an astonishing claim? We know the familiar panoply of arguments. Perceptival variation shows us that what we directly perceive of a single object with fixed properties varies with perspective and condition of the perceiver. Illusions tell us that we often perceive something very unlike in its nature the features we attribute to the object that is poorly perceived. Hallucinations and dreams tells us that we can have perceptual contents that are just like the perceptual contents we have when we are genuinely perceiving, but that cannot be features of some directly perceived physical object, since there is no physical object there at all to be directly perceived.

My own favorite when teaching introductory students is the "finger in the side of the eye" trick, so that everybody in the class sees two images of a book, say, when there is only one book. Then you instruct them to slowly remove the finger so that the two images converge to one. How could that one book image, then, be "the book itself" that is directly perceived?

Add to this the fact that, from Descartes on, part of the argument against direct realism was the insistent reminder of all intermediate physical and biological process that went on between perceived object and direct awareness of some content of perception. How then could that content be the perceived object itself or any of its features?

But are these arguments against the viability of direct realism in perception philosophical or scientific? The arguments are founded upon a rich variety of observations from our common experience. In the "finger in the eye" case you might even say they rest upon the results of an experiment. In the case of the arguments from the complex intermediate causal chain intervening from perceived aspect of object to content of direct awareness, the arguments rest upon our reasoning being embedded in a rich background of scientific knowledge of how the world works.

Maybe you don't like any of the arguments against direct realism. Perhaps you believe that all of the putative arguments that invoke observations and experiments and chains of reasoning simply do nothing to refute direct realism, do nothing to establish some realm of sense-data or sensory contents, and do nothing to justify the claim that it is these and these only that we directly perceive. Fine. Maybe you think all of this traditional empiricist philosophy is bad science. But it is hard to see anything that would allow you to deny that it really is science. So is it science or philosophy? Again I am at a loss to know what is really being asked here or what the distinction is supposed to be between philosophy and science.

9. Some Historical Motivations. From the middle of the nineteenth century on, scientists themselves have sometimes offered us empiricist ruminations on their science. Mach, Duhem, and Ostwald are notorious for having denied the existence of atoms, arguing from a position generally skeptical of inference to explaining unobservables in physics. Is this just bad science resulting from the error of importing a priori philosophy into science?

Let us look more closely at Mach. To be sure he is coming from a background of Hume and Mill. And he has written a volume called *The Analysis of Sensations and the Relation of the Physical to the Psychical* (1959). But he is also someone trying as hard as he can to make sense of the most important foundational physical theory available, Newtonian dynamics. And from its inception that theory has been subject to merciless critique. Newton puts on a bold front in the *Principia*, defending an absolute rest frame, but is well aware, as the corollaries to the Laws show, that it induces undeterminable features into his theory. And Newton himself expresses his doubts that his account of gravitation will do as the final explanatory word.

Mach wanted to clean up Newton's act. He initiated an attack on the problem of absolute rest and absolute velocities that remains only vaguely formulated in his work but that leads to more concrete results in later efforts of others. Mach's treatment of mass and force are only the barest beginning of the program that is not yet complete of trying to formalize the Newtonian theory and explore it from the perspective of a Ramsey account of its terms referring to unobservables. What is clear, though, is that the impetus toward empiricist and positivist modes of thinking about a foundational physical theory come to Mach not simply from an adherence to empiricist philosophy imported into his science from the outside, but as a natural extension of the empiricist elements that arise in an organic way from an internal critical exploration of Newton's dynamics. There the contextual acceptance of relative motions as the domain of the observable, with force, mass, and absolute motions as the epistemically, semantically, and ontologically dubious elements in the realm of the unobservable, comes from the historical, critical exploration of dynamics itself, and not from some bias to empiricist philosophy.

An even more trenchant example of this kind of blending of traditional philosophy and internal science appears in Einstein's first 1906 paper on special relativity. The problems there are understanding electrodynamics in the light of the Michelson-Morley negative results in the attempt to determine the velocity of the observer through the aether, and making better sense of the existing mathematical transformation laws of Lorentz and Fitzgerald. The key that resolves the problems is, of course, to accept the relativity of simultaneity to inertial reference frame. Einstein's paper is famously replete with arguments that rely upon a posited need for some kind of "operationalist" definition for distant simultaneity.

It is certainly true that over his lifetime Einstein himself was all over the map when it came to philosophical views about the nature of theories. Sometimes he is radically positivist and sometimes realist to the extreme. But what matters here is the fact that in his critical revamping of electrodynamics, a revamping that ultimately changed our whole view of the nature of space and time and required a reconstruction of dynamics as well, Einstein is engaged in the process noted above of reconstructing a theory by eliminating from it an undeterminable elements at the non-observational level. And in performing that essential task, a task internal to science, he relies upon presuppositions and methods that seem all too familiar from traditional empiricist philosophy.

And, whatever his changing views about the nature of science expressed in his lifelong running commentaries, Einstein's later dismissal of the difficulties raised by the hole argument in general relativity is grounded in just the same positivist mode of critical thinking about theories.

10. Conclusion. So to summarize, foundational physical theories are replete with conceptual conundrums. These require that the theories be "interpreted" in order that they be understood—epistemically, semantically, and ontologically. Such interpretations often take the form of localizing the conceptual difficulty at the level of postulated unobservable entities and properties posited by the theory and then looking for an alternative rendering of the theory that eliminates the unobservable structure that gave rise to the difficulty. Typically one seeks to eliminate from the structure of the theory some parameter whose value is underdetermined by all possible observations.

To carry out this program one must make a context dependent observable/unobservable distinction. Although the structure of the theory under interpretation plays part of the job of doing this, reference to a vague sort of distinction, familiar from empiricist philosophy, plays a crucial role as well. And the rationale for preferring the interpretation of the theory that eliminates the undesirable unobservable parameters is itself grounded not in internal science as is usually understood but on general philosophical principles of parsimony, simplicity, and the like.

Furthermore, a significant component of what is traditionally thought of as philosophy, that part of philosophy with empiricist aspects, relies upon observation, experiment, and background scientific knowledge. Furthermore, in inferring to conclusions from these observational and experiments this philosophy uses modes of reasoning that occur over and over again within fundamental science when that science is dealing with its crucial conceptual difficulties.

Naturalism is sometimes thought to be the program of confining one's reasoning to only those modes of thought acceptable to the internal processes of ongoing empirical science. Naturalists often tell us to eschew the siren call of philosophy with its attempts at restraining, controlling, or supplementing internal science. I'd love to be a naturalist, if I only knew what naturalism was.

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