The Diversity of the Sciences: Global versus Local Methodological Approaches

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Philosophers of science as different as Popper, Lakatos, Carnap, and to name more recent figures, Wolfgang Stegmüller and Richard Boyd, have proposed models of theory acceptance which are supposed to fit all of science. Sometimes these models are also suposed to «demarcate» the scientific from the non-scientific, or to «account for the growth of knowlegde». I think it is generally recognized that none of these models fits scientific theories perfectly, but that is as one would expect. Even if philosophy *were* a science, one would not expect to be able to *perfectly* model as complex a phenomenon as theory acceptance, even in a single case; and philosopy —dare I say?— is very far from being a science in any sense of «science» which does not border on vacuity. But what I want to call attention to today is something that goes beyond the fact of life just mentioned, the fact that our pictures and models fall short of their target in various ways. I believe that, in fact, each of the «global» models just mentioned does have a suggestive kind of «fit» to *some* theories (the ones that suggested the model in the first place), while fitting other theories very badly. I want to point out that certain theories tend, in this sense, to be *extreme cases* for philosophies of science. Among the extreme cases are Darwin's Theory of Natural Selection, Relativity Theory, and Quantum Mechanics. My purpose in these remarks is to point out the features which make them extreme cases for one or another global approach to philosophy of science. Thereby, I hope to suggest something about the nature of the activity of philosophizing about science, and to suggest a way in which philosophers of science may begin to come closer to the subject they are supposed to be addresing —the actual methodology of science.

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INDUCTIVE LOGIC IS NOT FORMAL

Goodman has shown that inferences of the same logical form can differ with respect to inductive validity/invalidity, no matter how finely we analyze logical form. The suggestion Carnap made to deal with this problem was to postulate that some properties are in themselves «purely qualitative», and to require that the primitive predicates in the languages to which formalized inductive logic is applied be of this sort. This represents an intrusion of metaphysics into inductive logic —an intrusion, moreover, of the sort of metaphysics which Carnap himself combatted in other areas, the sort that throws us back on intuitions of an inexplicable kind.

Goodman's own suggestion was to consider logical form *plus* the history of prior projection of the predicates involved in the inference, as well as certain related matters (e.g., «entrenchment» and «overriding»).

This problem of choosing a criterion of projectibility is robust; it appears in Bayesian theories of confirmation disguised as the problem of the choice of a «prior». Bayes own solution resembles Goodman's, in spirit if not in detail: Bayes thought the «prior» comes from previous experience. This leads to an obvious regress. The regress may be accepted as unavoidable; but then we give up hope of a global, unchanging formalization.

Goodman's own solution is worked out only for very simple cases of inductive inference. While I agree with Goodman's claim that fit with past practice is important in induction, Goodman's present system cannot, I think, be regarded as more than a first approximation. It too blatantly ignores other epistemic priorities that we have —call them «prejudices» if you like, but they are part of our notion of reasonableness. For example, according to Goodman, a culture which had *always* projected such crazy predicates as «grue» would now be justified in doing so— their inferences would now be «inductively valid»! I cannot agree.

POPPER AND THE THEORY OR EVOLUTION

One of the main approaches which I would classify as «informal» (in spite of the pretensions it sometimes had to being a formal treatment) is Sir Karl Popper's. I criticized this approach in muy contribution to the Popper volume, where I argued that this approach does not well fit *the* paradigm example of a physical theory: Newton's Theory of Gravitation. Be that as it may, there is at least one example of a major theory outside of physics that it does not fit even to the extent that it fits physical theories, and that is Darwin's Theory of Evolution by Natural Selection.

Popper, to be sure, has shifted his position on this theory. Although at one time he classified it (along with Marxian social science and Freudian psychoanalysis) as pseudoscientific, he now accepts and even appeals to Darwinian ideas —not, however, as «science», but as a prescientific metaphysical research program, or something of that kind. Be that as it may, even Popper is aware that he cannot stretch his schema to cover *this* theory— and most of us would consider it to be one of the central contemporary examples of a «scientific theory». What has gone wrong?

It might be useful to begin by making some distinctions which Popper surprisingly fails to draw. The idea of Evolution in a non-specific sense (that is, the idea of evolution *minus the causal mechanism)* is pre-Darwinian. This is well known, but perhaps still worth saying because even today (e.g., in our current replays of the Scopes trial) the suggestion is sometimes encountered that the gradual appearance of more complicated species of organisms —let me refer to this as the *fact of evolution*— depends for its credibility *entirely* on Darwinian theory, as if the discovery that there was something wrong with our present ideas about the *mechanism* of evolution, if it should occur, would suddenly make it an open question whether the geological layers came into existence five thousand seven hundred and fortyfive years ago, or whenever. Even the *fact of evolution* is not quite «falsifiable» in Popper's very narrow sense —the statement that the species came into existence in a certain order, starting with, say, viruses, and were not all created in a seven day period does not, by itself, imply a «basic sentence», but it *is* falsifiable in a sense that a physicist would recognize (precisely the sense, I would argue, in which Newton's Theory of Universal Gravitation is falsifiable): *in combination with auxiliary statements which are appropriate in the various contexts of geology, paleontology, etc., in which it is applied,* it leads to testable predictions. (Note that «falsifiability» in *this* sense is not sharply distinguished from *confirmability*— nor should it be, *pace* Popper!) The reason I speak of the *fact* of evolution is that this bit of the natural history of the Planet is as well established as any scientific fact *can* be, and has, I think, somewhat the same relation to the Theory of Evolution by Natural Selection that phenomenological thermodynamics bears to ergodic theory. Like phenomenological thermodynamics it can be corrected in detail by the more fundamental theory, but its correctness in broad outline is not dependent upon the confirmation of the more fundamental theory.

When we come to the more fundamental theory —Darwin's Theory of Evolution by Natural Selection— there are again epistemological distinctions to be drawn. At the advancing edge of the theory —and this is where I feel most unsure of myself— it becomes, in places, highly mathematical. Hearing a talk by Dick Lewontin a few years ago, I was struck by the extent to which the kind of modeling he was doing looked, in broad outline if not in mathematical detail, like a kind of stochastic mechanics. At still other points on the advancing edge, we get controversies —Stephen Gould is involved in a number of these— which resemble controversies in history rather than in mathematical physics. And, it goes without saying, there are now rich areas of overlap with genetics and molecular biology. But this is not what I want to talk about. The claim that Darwin made —the one that is responsible for the «cosmological» excitement generated by the theory— was not a mathematical claim, not a claim about Mendelian genetics (which Darwin did not known), not even a claim of the kind that Gould is so concerned to argue, about gradualism and continuity *versus* «leaps, but rather and imediately intelligible and vastly important claim about the Origin of the Species. Darwin said that the species *—all the species except the primordial one—* came into existence by «natural selection».

This is simultaneously a universal statement and a causal explanation. And it is *this* statement that gives Popper trouble.

Why it gives him trouble is easy to see. Even if, under suitable conditions, we could see the mechanisms that Dick Lewontin is modelling operate and verify *(oops!* fail to falsify) that they operate the way Lewontin's model says they should, this would hardly test the causal-cosmological claim just alluded to.

The question that exercises philosophers is, of course, *What could test that causal-cosmological claim?* The simplest position would be that we can just make an induction. If the evidence supports the proposition (1) that natural selection is operating now as evolutionary biologists claim it is, and the proposition (2) that (with insignificant exceptions) no other mechanism capable of producing species is now operating, then we should, barring contrary evidence, just make the induction that natural selection always operated and that no other mechanism ever operated to produce species. But this is not just too simple for an anti-inductionist like Sir Karl; It is too simple for *any* philosopher of science to take seriously.

Of course, even when philosophers agree that something is wrong (or «too simple») they rarely agree on *why* it is wrong, and this case is no exception. Let me, therefore, give my own reasons for thinking that it is «too simple», and not pretend to speak for the mythical «philosophical community». The first thing worth noticing about the «induction» proposed is that it ignores entirely the way in which our willingness to make inductive projections depends on our estimate of the *lawlikeness* of the proposition being projected. We are sure that all life here on earth contains DNA; yet I suspect that science fiction readers among us are not willing on that basis alone to accept «All life everywhere in the Universe contains DNA» as a law; not because we want a larger sample size, or even because our sample is not drawn from a hundred or a thousand different planets rather than from one (although that would surely help), but because we are not able to see why it should be *physically necessary* that all life contain DNA. It may be that experts can think of reasons that it should be physically necessary. If so, I would not be surprised to find that they *were* willing to accept this statement as a law. But that is not surprising: our estimate of the *lawlikeness of a statement* generally depends on knowledge quite remote from the «direct» evidential support for the statement, and can even be non-experimental. Even a person who thought that the first of the two propositions above was sufficiently lawlike to justify extrapolating it back a billion years or two might have the opinion that the corresponding extrapolation of the second proposition («there was never any other mechanism that operated») is not a law but just a contingent historical claim. And such a person would regard the induction I mentioned as fishy —as analogous to, say, an attempt to conclude that people always knew how to read and write from the fact that people we now observe know how to read and write. (Notice tha this latter would be a bad induction even in the absence of direct knowledge of a time at which people did not know how to read and write; we have too much «cross inductive»: evidence that technological devices, methods, etc., are the sorts of things that tend to be discovered at a time and then persist.)

What of «simplicity»? We might argue that the other mechanisms for producing speciation that thave actually been proposed —Lamarckian evolution and Special Creation— involve a genetic theory which has been refuted, in the first case, and the postulate of an Intelligent Being who was in existence prior to all forms of life studied by biology (but is inaccessible to scientific investigation), respectively. This latter postulate is not only unscientific, but would be considered superstitious even by many religious people: in my view, it makes God into a Super-Humanoid in the world, while appealing to the fact taht it is God that is being invoked as an efficient cause, and not, say, *Martians* to insulate the theory of Special Creation from scientific objections.

This line of thought is one I myself find convincing. But let us examine it a little. This line of thought depends on the principle (which, I have argued, underlies even our mundane belief in the existence of a material world), that an important reason —and a **good** reason— for accepting a theory is the absence of any serious alternative in the field, end also in the maxim, «Don't postulate in-principle-unobservables». Positivists (and I count Sir Karl as a Neopositivist) apply the first principle tacitly when they accept certain theoretical statements which have never withstood any Popperian test because there was never any «rival hypothesis» (e.g., «Space has three dimensions»). Buy they are uncomfortable with this sort of argument when the question is a historical one. But, in its present form, the theory depends heavily —the causal-cosmological claim which is the heart of the theory depends heavily— on one's acceptance of the methodological maxims just mentioned.

But doesn't the acceptance of physical theories depend on the same methodological maxims? Of course it does. But it would be wrong to dismiss the intuition that there is a significant difference between physical theories and the Darwinian theory of speciation. I suspect that the heart of the issue has to do with the —admittedly difficult or impossible to explicate, but still vitally important— notion of «lawlikeness». As is often mentioned, «lawlikeness» is connected with «physical necessity». A lawlike statement states a putative physical necessity, or at least something which is physically necessary under idealized circumstances. (Dudley Shapere has done a good deal of work on the different sorts of «laws» and «lawlike statements» we find it science, emphasizing in particular the different kinds of descriptive and idealizing functions such statements play.) Now, one of our ways of deciding on the lawlikeness of a statement is simply to ask ourselves the question: «Is this the sort of statement that i would expect, if true anywhere, to be true *everywhere?* Newton's inverse square law for gravitation is not *exactly* true anywhere. But it is the sort of statement that, if true in one region of (flat) spacetime we would expect to be true in every region. So, as long as we did not even consider the possibility of curved spacetime, let alone spacetime of variable curvature, this was a paradigm case of a lawlike statement. Of course, *how we know* that this statement couldn't be true for, say, a trillion miles from Terra, and false everywhere else, is an interesting philosophical question. It certainly isn't analytic that this couldn't be the case: one could imagine a science fiction story (say by Robert Sheckley) in which it turned out that we were in a little bubble of spacetime which obeyed peculiar laws made up by some extraterrestial. But in that case there would be a *reason* why those laws obtained only in this little («little» cosmologically speaking) region. It may be that it is part of the way our minds work that we demand for certain sorts of statements that they should hold everywhere or nowhere, not in the sense of being a Kantian synthetic apriori, but in the sense of being what Saul Kripke calls a basic «prejudice». «Prejudices» in Saul Kripke's sense, are not irrational beliefs; rather they are epistemological ultimates which we conform to unless the price is too high. (Some such idea —the idea of a defeasible synthetic a priori— is suggested in some of Reichenbach's early writings.)

An example of a *non*-lawlike statement may help. We are willing to believe that it may be true in some places and not others that people speak English. If asked why, we can give a reason —say, that English evolved from Anglosaxon, and Anglosaxon was not spoken everywhere, but this, of course, just pushes the question back to the similar question «Why was Anglosaxon not spoken everywhere?» We just aren't bothered by the fact that such things are true in some places and not others in the way we would be if (in a flat spacetime) the Law of Universal Gravitation held in a particular region (shaped, let us suppose, like a chiliagon, in its spacial cross section), and not anywhere else. We don't expect *certain* sorts of regularities to just obtain by accident. That's one of our ultimate Kripkean «prejudices».

What about the existence of mechanisms capable of producing speciation other than natural selection? I don't think we have any «defeasible apriori» *prejudice* about that either being the case at every time or at no time. If there *were* mechanisms other than natural selection capable of producing speciation, why shouldn't they depend on conditions which obtained in the past (or might obtain in the future) but which do not obtain now?

In short, I am suggesting that the reason we don't think that the generalization «Only natural selection produces species» *must* be true at every time or at no time (even apart from the problem of what produced the first living material) is that it doesn't quite look «lawlike», or, at any rate, it isn't one of our basic «prejudices» that this statement is lawlike.

What of the argument that *The other mechanisms which have actually been suggested —Special Creation and Lamarckian Evolution— are either hopelessly unscientific or refuted?* Isn't this an argument for the lawlikeness of «Only natural selection is *capable of* producing species? Well yes. But notice; the idea that Special Creation is «hopelessly unscientific» turns on the acceptance of a huge cultural and ideological change that took place in the West starting around 1.500 and reaching unstoppable momentum in the seventeenth century. The acceptance of this argument —let us admit it— does have something «metaphysical» about it (though not, I think, in a bad sense).

Am I then agreeing with Popper? Is natural selection (including the causal-cosmological claim) a «methaphysical research program» and not science? Well, why can't it be *both* a metaphysical research program *and* science?

Here we come to the nub of the issue. If we limit «science» to the formulation of «laws», and we take these to be statements which, like the familiar differential equations of fundamental physics, we expect to hold true in every region or in no region (at least assuming the fundamental idealizations of the branch of physics in question); if we think the epistemology of science can be reduced to pretending that all scientists do is write down systems of such «laws» and derive «basic statements» from these systems, or these systems plus «coordinating definitions» (Reichenbach), «initial conditions» (Popper), or whatever; then we should not be suprised if practically nothing but physics turns out to be «science». This model doesn't fit the epistemology of evolutionary theory, and Popper is honest, I think, to admit that it doesn't. But in a less tendentious sense of the term «science», *of course* evolutionary theory is science. At the risk of boring everyone with Wittgenstein's term «family resemblance» once again, I have to ask why on earth we should expect the sciences to have more than a family resemblance to one another? They all share a common heritage, in that they owe allegiance to a minimum of empiricism (they «put questions to Nature», they are conducted in a fallibilistic spirit, etc.), they frequently depend on very careful observation and/or experimentation (think of the amount of data that evolutionary biologists have collected!), and they interact strongly with other disciplines recognized to be «sciences». But there is no set of «essential» properties that all the sciences have in common.

If Evolutionary theory does not, taken as a whole, fit Popperian (or more broadly positivistic) accounts of science, there are other models it does fit. «Inference to the best explanation» accounts would not be disturbed by the way in which facts quite remote from the «subject at hand» enter into our acceptance of evolutionary theory, or even by the way in which general methodological convictions enter. It was the aim of these accounts to allow such things to enter into theory acceptance. I shall shortly discuss cases which are «extreme» for «inference to the best explanation» accounts. But first I want to discuss yet another way of dealing with evolutionary theory.

Lakatosian «research programs»

The very important work of Imre Lakatos is, perhaps, *the way* in which neo-Popperian ideas have become part of mainstream philosophy of science (as, for all their *popular* appeal, Popper's never did).

It is when he gets to epistemology that Lakatos shows his Popperian heritage. Like Popper he believes falsifiability —which he construes more reasonably than Popper, in that he allows auxiliary hypotheses to play an important role— is vitally important. But he does not believe that good theories are always falsifiable. (He and I independently made very similar criticisms of Popper on this score in our papers in the Popper volume.) He resolves the tension between his desire to keep «making risky predictions», *the* one-and-only *sine qua non* of science, and his recognition that we must allow research program to have periods in which it is *not* making such risky predictions, by saying that the unit of epistemological assesment is the whole reseach program and not the theory or statement. (Like all Popperians, he is totally contemptuous of what we actually *say* about theories and statements —pointing out that we speak of theories and statements as «well confirmed», «implausible», «well established», etc., is «ordinary language philosophy», an intellectual sin beneath contempt). A theory that leads to risky predictions over the long run is called «progressive», in his jargon.

Now no knowledgeable person would deny that evolutionary biology taken as a whole is a progressive reseach program in this sense. Many successful predictions have been made in a number of different areas which share the Darwinian-Mendelian paradigm. It is when we ask for the *epistemological* significance of this that our problems begin. Does the progressiveness of a research program really account for, explicate, the *rationality* of accepting the current incarnation of that research program? Here Lakatos is surprisingly coy. He wants, it seems to me, to save the idea that science is a uniquely rational activity *without* giving the working scientist any substantive advice at all —and while one can understand both desires, they are obviously hard to reconcile.

If a theory or a research program is *supported* by evidence to the extent that the research program is currently «progressive»; if *coherence, simplicity,* and other traditional «inductivist» parameters do not have to be considered at all, then we are going to get very strange results. It may be that, for all I know, *Velikovsky's* theories are *well-supported* by such a criterion; he certainly has been lucky with some risky predictions. Even if we cannot find an actual contemporary example, it is certainly posible that a theory some of whose assumptions are wildly off the mark leads to a «progresive» research program: just imagine that those particular assumptions don't get tested (perhaps because they are difficult to test), and it is only *part* of the «theory core» that is responsible for the successful predictions. (Historical examples are easy to find; Ptolmaic astronomy was, for a long time, a «progressive research program», but the assumption that, in addition to the heavenly bodies and their orbits, there are invisible «spheres» in which the bodies are embedded had nothing to do with the success of the predictions Ptolmaic astronomers made —indeed their own theory of «epicycles» required planets to pass *through* the spheres what were supposed to «carry» them.)

One way to meet this problem would be to require that a «progressive» research program make predictions which test *all* of the *core assumptions* of the theory. This would require some way of telling what *are* the «core» assumptions without relying on hindsight. I believe, with Hardin and Rohrlich, that we can only tell what is «core» and what is «protective belt» after a theory has been superseded, or rather, after it has become a «limiting case» of a superseding theory which belongs to a later «research program». It would also require a way of telling when the «protective belt» has been «adjusted» too often. It seems to me that this leads back to precisely the kind of traditional epistemological problem that Lakatos wants to abandon.

At any rate, the question whether the predictions evolutionary biologists have made really test the causal-cosmological claim which fundamentalist and other opponents of the theory of evolution attack, is, as we have seen, a difficult one for a Popperian, and I do not see how it is any easier for a neo-Popperian like Lakatos. If the only criterion for being part of a research program is a sociological one, if the *sociological* unity of the community of evolutionary biologists is enough to justify considering all of evolutionary biology one research program, and the «progressive» character of this program justifies accepting *all* of it, then we are, perhaps, justified accepting evolutionary theory on Lakatosian grounds. But then, by parity of reasoning, we were once justified in accepting the idea of «spheres», even though that idea had nothing to do with the success of science in any period. Such a view gives too much to the idea that only predictive success matters in science, in my opinion. If, on the other hand, we have to show that a «crucial experiment» directly testing the causal-cosmological claim has been performed, or that a «risky prediction» whose success *directly* supports that claim has been made, then, if we become Lakatosians, we shall have as much trouble fitting the acceptance of the real «core» of evolutionary theory, the causal cosmological claim which is its very heart and soul, into our picture of «scientific rationality» as Popper does. The idea that all there is to rational procedure is making novel predictions still haunts philosophy of science.

«INFERENCE TO THE BEST EXPLANATION»

The view that Bayes himself held seems to have been revived recently, at least in a qualitative form, by Richard Boyd. The idea of a basically Bayesian picture of theory acceptance (with the «prior» coming from background knowledge about «likely sorts of mechanism») fits a good deal of what Kuhn has called «normal science»; but I would argue that it does not fit some of the major scientific revolutions (the quantum mechanical revolution in particular) without undue «schema stretching».

Boyd's approach is an instance of what has been called Inference to the Best Explanation epistemology. This approach has not been very much elaborated (in spite of the fact that many philosophers refer to it approvingly). I want to examine it briefly with a special eye to the case of quantum mechanics.

Someone who learned about quantum mechanics only by reading recent articles in **Philosophy of Science** might well think that all the conceptual problems were connected with Bell's Theorem. This is, perhaps, a comment on the peculiarity of the relationship between philosophy of any science and the science. To be sure, Bell's theorem is important because it puts «hidden variable» theories at a severe disadvantage (it shows, so to speak, how high the price is for taking the hidden variable approach); but *the physical community had almost no intererest at all in hidden variable theories even before Bell proved his theorem.* The discussion in the literature of philosophy of quantum mechanics has for a long time been remarkably indifferent to the way physics is going.

I obviously do not have the time for a technical discussion at this point in this lecture, but I have to say a few words about the history of the problem of «interpreting» quantum mechanics. The problem is as old as the discovery of the phenomenon of quantum mechanical interference (of which the most famous illustration is the celebrated two-slit experiment). That phenomenon already convinced physicists that the squared norm of the state vector is not a «classical probability»; or to put the same point in a different way, it convinced them that the peculiar transition from probability to actuality that we witness in quantum mechanical experiments is not a mere conditionalization of a previously valid probability to additional evidence. While a few physicists (Bohm, De Broglie) attempted to find classical models for the «reduction of the wave packet» (models involving «pilot waves» in the case of De Broglie and a special force in the case of Bohm's theory), the overwhelming majority of all physicists —and not, by any means, only the operationalists— followed Bohr's advice to simply stop trying to think of quantum mechanical reality in classical terms. That is to say, they acepted the advice to accept the theory (which has, remember, a higher level of accuracy and a greater quantity of direct experimental evidence in its favor than any previous theory in the history of physical science), *even though there is no «likely sort of mechanism» which we know of and which could account for the phenomena the theory postulates.* The problem isn't just that the «prior probability» of the kind of phenomena that quantum mechanics postulates would seem to be as close to zero as empirical probabilities ever get, if prior probabilities are really calculated from background knowledge about «likely sorts of mechanism»; the problem is that we don't even know that a «mechanism» *is* what accounts for those phenomena. Certainly no one has suggested any «mechanism» that could account for those phenomena and that would not violate constraints that working physicists are not prepared to give up. This does not appear to be a case in which we have to look for an *unlikely* mechanism because we have discovered unlikely phenomena; it looks more like a case in which *thinking in terms of «mechanism» is not what is called for.*

What I have just said is not the opinion of very many philosophers of physics. Almost every philosopher of physics has his own «realistic» interpretation of quantum mechanics; an interpretation which he defends and which at most one other philosopher (and no physicist, unless he co-authored it with a physicist or is a physicist him/her-self) accepts. This is a game which I know very well —I played it myself for years! What I want to urge is not that we stop playing this game— it *is* a fun game, and some interesting theorems get proved —but that we attach greater importance to the fact that physics goes on quite well without any visible success in this game.

Indeed, even to say that what we are dealing with in quantum mechanics is «unlikely sorts of phenomena» may be making the situation in quantum mechanics look too «Bayesian». A Bayesian might say, after all, that what is relevant is not the prior probability of the phenomena we witness in quantum mechanics (provided it isn't actually zero), but the result of conditionalizing that prior probability to the very unlikely observations that we have made —although how that conditional probability is to be determined if our priors really come from previous experience, Bayesianism does not tell us. What I have in mind is this: if we suppose that there *are* «phenomena» in the standard sense in quantum mechanics, events describable in the language of «pre-theories», to use the terminology employed by a number of authors, which quantum mechanics must simply *explain,* then we are assuming that quantum mechanics is compatible with the existence of a global truth about all «phenomena» (that all «phenomena» statements «commute», in other words), and not simply with the existence of a true description of the phenomena that *each* observer can conceivably measure. But no way of rendering this assumption compatible with quantum mechanics is known except «brute force», e.g., just assuming that all macroobservables commute, whether simultaneously measurable or not. The radical nature of quantum mechanics is shown by the fact that at least one famous thought experiment (Schrödinger's Cat) challenges even *this* assumption.

In any case, the Bayesian story cannot account for the fact that Einstein's Special Theory of Relativity was accepted by a number of famous physicists (Planck, Lorenz) *before* the realistic «spacetime» interpretation of that theory had been proposed by Minkowski. What these physicist —let me add Einstein himself to the list— accepted was *not* just the existence of some «phenomena», some events «describable in the language of pretheories», which had a low «prior probability»; they accepted the falsity of the statement (metastatement) that. *There is a two-place bivalent relation of simultaneity between distant events.* If the notion of «prior probability» applies to such principles at all, principles which have been treated as apriori since the beginning of conceptual thought about these matters, it would seem that this metastatement had prior probability *one.* Even if one somehow argues (perhaps on Quinian grounds) that no statement (or no statement outside of first order logic) ever has probability exactly *one,* why the principles Einstein appealed to (principles of formal symmetry and of not postulating in-principle-unobservables) —should have had more weight than the metastatement about simultaneity, when *a priori* they would seem more likely to have exceptions, is not explained by «inference to the best explanation» epistemology at all.

EXTREME CASES FOR PHILOSOPHY OF SCIENCE

Briefly (and too simply) put, my position is that quantum mechanics and 1905 Relativity fit Positivist stories fairly well, but don't fit «inference to the best explanation accounts (at least not without more clarification than we have yet received of what «best» means when a theory doesn't even have a non-controversial interpretation, clashes with statements which are «a priori relative to background knowledge», etc.). Darwinian Evolution seems to fit Inference to the Best Explanation accounts, but doesn't fit Positivist or Popperian accounts. In this sense, certain theories are extreme cases for certain philosophies of science. I will close by proposing that one should give up hope for a single pattern which fits all theories. Perhaps one can still say some things about scientific procedure in general, but the time has come to recognize that scientific theories come in different «types» and that informative philosophizing has to descend to a more «local» and less «global» level.