DO UNBORN HYPOTHESES HAVE RIGHTS?*

BY

LAWRENCE SKLAR

OUR intuitive satisfaction with philosophical models of rational belief in science is, of course, highly influenced by our experience of the historical development of science as practiced. While one can go overboard in this direction, emphasizing conformity to a history infected with "accident" and neglecting critical examination of the usual standards of rationality on their own merits, surely it is reasonable to let one's views of how science ought to be influenced by how best scientific practice has historically proceeded.

Clearly the long existence of geometry as the single clearly systematized and secure science was largely responsible for the persuasiveness of models of science which posited that (at least ideally) all beliefs ought to be founded upon indubitable self-evidence. The lengthy sway of Newtonian mechanics as a (possibly) all encompassing and final theory founded upon inference from the data of the senses surely was instrumental in the long domination of philosophy of science by inductivist models. And the scientific revolutions of the twentieth century, especially those aspects of the revolution which showed us that the wealth of data previously taken to support Newtonian theory was, when taken in conjunction with the new data incompatible with the older theory, equally supportive of novel theories incompatible with the Newtonian, was straight-forwardly influential on a range of philosophies of science ranging from Popper's skepticism of the notion of inductive support through those who take the quasi-Kantian line that our very understanding of what the data is is an imposition on the world by us of our theoretical preconceptions.

The question I want to examine here is this: Quantum mechanics and relativity show us that the data which seemed to support very well one theory can be seen, in the light of scientific change, to support as well quite different theories, theories incompatible with the older theory in deep theoretical and conceptual ways. Now consider the attempt to decide, at any moment, whether or not a given set of data supports a hypothesis. Having in mind the potential existence of a vast array of theories, genuine alternatives to the hypotheses we have brought to mind, but alternatives which we haven't yet even imagined, should we not wonder if amongst that vast array of alternatives there might not be some which, were we to know what they were like, we would consider even better supported

Pacific Philosophical Quarterly 62 (1981) 17–29. 0031-5621/81/0100-0017$1.30

by the present data than any of the alternatives we have brought to awareness?

This is, of course, a kind of skepticism about induction, but it is worthwhile to distinguish it from skepticism founded upon distinct, although closely related, grounds. We can look upon Humean skepticism, perhaps, as skepticism founded upon the fact that all the possible data is never in. I ought not to believe all crows are black on the basis of the observed sample of crows, for nothing in the nature of the sample assures me that the very next crow won't break the pattern.

We could be led to a skepticism closer to the one I have in mind by Goodmanian arguments, although Goodman, of course, doesn't draw skeptical conclusions from his observations. "Grueish" hypotheses are, like the ones I have in mind, alternatives to our ordinary hypotheses all equally compatible with the empirical evidence to date. But, presumably, what we are to do as philosophers is to figure out what is wrong with these outlandish hypotheses. Whether the line one takes is Goodman's notion of the entrenchment, or some version of the thesis that our selection of projectible predicates is itself rationalized by our current best available background theory, we are, in any case, led away from skepticism by some argument designed to show us that as things stand now we do have some reason for preferring as genuine scientific alternatives members of the set of hypotheses from which we do choose, to alternatives constructed in the Goodmanian manner which can somehow be rejected as spurious candidates.

While the issues here are close to those with which I am concerned, they are not identical. What I have in mind is, rather, the skepticism engendered by reflection upon historical scientific experience; skepticism based not upon the existence of outlandish pseudo-hypotheses cooked up by the philosophical manipulation of predicates, but, rather, on the reasonable assumption, warranted by past experience, that there are vast numbers of perfectly respectable scientific hypotheses, hypotheses which, were we aware of them, would receive our most serious scientific consideration, but which, due to the limitations of our scientific imagination, we just haven't yet brought to mind. What we are concerned with is a Newton dubious of the inverse square law, not because objects might obey it up to 1700 and cease to do so thereafter, but because he imagines the possibility of an array of genuine alternatives to his theory even though, of course, he can't imagine just what such alternative theories would be like.

Nor am I concerned with the problem of theoretical under-determination in any of its forms. The alternatives I have in mind are not those variants of the original theory which a positivist would declare trivial semantic alternatives. I shall not be concerned with alternatives constructed by manipulation of the theoretical apparatus which leaves observational consequences invariant, nor with those quaint alleged alternatives which one gets by switching from talk of objects to talk of time slices, from things to modes of spacetime points, and the like. General relativity is not, in any way, an "empirically equivalent" variant of the Newtonian theory of gravitation. But shouldn't a prudent Newton have realized that such a theory could exist, even if he couldn't say what it was? And might not some such unimagined theory be more plausible, even relative to present data, than Newton's own? And shouldn't we now admit the existence, Platonically, of innumerable alternatives to our best present theories, alternatives all of which would save the current data equally well, but none of which are equiv-

alent, *vis-à-vis* all possible empirical experience, to the currently accepted hypotheses? Shouldn't we realize that these alternatives exist, even if we can't say what they are? And shouldn't we accept the fact that among these unimagined alternatives some may very well be more plausible than our own theories relative to present observational facts?

The depth of the problem we face here can be emphasized by just a brief consideration of a number of models of theory choice we have been offered by methodologists. In each case we see that we can make sense of the adoption of one hypothesis only by viewing the decision process as the selection of the preferred hypothesis from an antecedently given set of possible alternatives. In each case we must wonder if we are left with any coherent theory at all when we face up to the fact that the set of hypotheses we have yet brought to mind constitutes only an infinitesimal finite fragment of the full range of alternatives.

Consider, for example, the attempt to resolve the familiar problems encountered in trying to define a notion of qualitative confirmation which proceeds by arguing that an instance is confirming of one hypothesis only relative to an alternative choice or set of such alternative choices. If the set of alternatives we should really have in mind is the indeterminate class of all possible hypotheses, including ones we have not yet thought up, can we understand what it is for an observational instance to confirm a given hypothesis at all?

Consider Bayesian strategies for confirmation theory. Here we must distribute a priori probabilities over all the alternative hypotheses to be considered. If there is only a finite set of hypotheses we have in mind, this is easy to do, even if it isn't easy to find any source beyond subjective whim for rationalizing any particular chosen distribution. Even when the set of alternatives is infinite, if it can be characterized by some orderly parameterization we have the means (say by moving from point to interval estimation) of plausibly assigning a priori probabilities and then grinding through conditionalization with respect to the evidence in the usual way. But if we must keep in mind the infinite and *indeterminate* class of all possible hypotheses, known and unknown, how can we even begin to assign a priori probabilities to those few hypotheses (or parameterized sets of them) we do have in mind (unless, perhaps, to give them all a priori probability zero on the basis of their very small place in the space of all possible hypotheses)?

Consider inference to the best explanation. Should we adopt that hypothesis relative to which the evidence has the highest likelihood of all the likelihoods generated by the hypotheses we have in mind as alternatives? Rather, shouldn't we realize that in the vast sea of alternatives we have not yet considered it is all too probable that there is some, as yet inconceived, hypothesis relative to which the evidential warrant is even better explained than it is by our current best candidate? On this basis shouldn't we agree that being the best of an arbitrarily selected and narrow class simply isn't being good enough to be believed, and once again skeptically withold our judgment?

Finally we ought to consider those attempts at reconstructing scientific inference which rely less upon formal models of hypothesis choice and more upon models of choice allegedly founded upon abstraction from historical scientific practice. Familiarity with this literature once again shows us a universal predeliction for the competitive model: belief is to be credited to that hypothesis

which does best in competition for survival with its rivals, be they older, previously accepted, hypotheses or novel alternatives recently contrived. But what credibility can accrue to the victor in a battle for survival which, by historical accident and paucity of imagination, simply keeps nearly all of the competitors out of the arena?

2

An initial response to the alleged skeptical consequences of the existence of inconceived hypotheses would be to affirm that since we cannot possibly deal with the unavailable, its existence or potential existence cannot be relevant to questions of justification. For example it might be argued that to be skeptical of induction because all of the data on which we might make a judgment are not yet in is to misconstrue what justification in the inductive context means. The whole point of induction is to allow us to draw inferences on less than an exhaustive observation of all the facts in the world, and so, to declare an inference unjustified simply because it proceeds on the basis of a small sample of observational facts is to misconstrue what would count as a justified inference in the inductive context.

Similarly, the argument might go, by its very nature the process of hypothesis selection in science requires us to make our decision on the basis of the consideration of only that limited selection of hypotheses which have come to mind at a given time. It is as impossible to consider as potential candidates for belief all possible hypotheses as it is to have in view all possible observational facts about the world. Accusations of "mere contingency" or of its being "accidental" that we have considered those hypotheses we have brought to mind and not the others are not more supportive of skepticism here than would be the claim that it is accidental that we have the observational data we do have and not some other sample in the inductive case. To be justified in accepting a hypothesis, it will be argued, is to have selected the best among the available candidates. The invocation of other possible hypotheses not yet brought to mind as a grounds for skepticism is just a misconstrual of what counts as justification in the context in question. It is no argument that one isn't justified in doing what one does on the basis one has by pointing out that the basis for inference is weaker than another which is, in fact, impossible to obtain.

Up to a point I think that this argument has merit. Whenever a procedure in which we have pre-analytic confidence is criticized by invidious comparison with some ideal which is, by its very nature, unobtainable, we are justified in invoking as our first defense of the procedure in question the argument that the only justification for giving up a procedure, imperfect as it may be, is the move to a better one. It is never justified, it will be claimed, to give up the best procedure one has simply on the basis that it doesn't meet standards which are in principle unfulfillable in any case.

But, of course, the skeptic can reply that just as the best hypothesis may not be good enough, leading us to withhold judgment (and this not simply as a skeptical general withholding of judgment, but as the right thing to do, at least temporarily, in actual cases—even under very unskeptical decision-making schemes), so the best methodology may simply not be good enough. If this

leaves us without any rational grounds for decision making, says the skeptic, well, that is just too bad. Of course we may continue to decide with the vulgar while chastely withholding judgment in the privacy of our learned study.

Rather than debate this general issue, though, I think it more profitable to note that there are some general moves one can make in this case, moves which if not fully resolving the issues between the skeptic and his opponent, at least throw some light on the issues between them. Let me turn to these.

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First let us consider a couple of related arguments which attempt to mitigate our skepticism by urging on us a subtler understanding of the *content* of our hypotheses than might have initially occurred to us.

(a) One such move is to urge on us a distinction between our fundamental theory of the world (or "cosmology") and those more limited theories characterizing narrower features of reality. Into the former class go such things as our general spacetime theory, our general theory of objects and their states (at the moment relativistic quantum field theory—such as it is), etc. Into the latter class would go theories of the chemical realization of the genetic code, of covalent chemical bonding, of superconductivity, of plate tectonics, etc.

Do we really harbor the suspicion that, in the fullness of time, we will either come to believe, aware as we will be of novel hypotheses we hadn't previously considered, that our earlier acceptance of theories was then unwarranted? Of our general, fundamental, cosmological theories I think this might be so. And if we do harbor the suspicion of this possibility, how can we be assured of the permanence of the warrant for the narrower, less fundamental theories all of which presuppose the correctness of our cosmological views?

I suggest it is just this presuppositional nature of the narrower theories, properly understood, which can immunize them from the kind of skeptical doubt which infects our belief in our grand cosmology. For, I suggest, we may understand the content of the narrower theory as being relativized to the background cosmology. So that our understanding of, say, the claim that genes are DNA molecules can be thought of as invariant (and hence its warrant invariant) under even a radical revision of our understanding of just what molecules (or material things in general) are.

Suppose Wheelerian fantasies are one day realized, and we find that DNA molecules, like all other matter, are just tightly knotted up bits of spacetime. Even wilder, suppose that our future scientists finally see the wisdom of the Leibnitzian view that all the world is a construct of spiritual monads. Would any of this change our opinion that genes are DNA molecules? Or that we were so warranted in believing in 1975? Would it change our estimate of our earlier warrant that electron sharing holds some molecules together? Or that superconductivity is the result of long-range pairing which bonds fermion electrons into the suitable constituents of a degenerate boson gas? I think not.

Of course how we then "understand" those narrower theories might change. The meaning of plate tectonic theory is, I suppose, rather different when we come to suppose that crustal plates are, like everything else, thoughts in the mind

of the great world-spirit (or whatever), than is the meaning of that theory under our present cosmological scheme. But be that as it may I still think there is plausibility to the claim that while contemplation of inconceived hypotheses might make us skeptical of our warrant for our grandest, most fundamental theories, it really doesn't influence our confidence in the warrant we hold for inductively well-grounded narrower theories. Do we really think that any future increment in scientific imaginativeness could lead us to doubt that yeasts cause fermentation, or that vision is mediated by the impact of light on the retina of the eye? If the future does do in these theories it will do so only by a dissolution of the cosmological background they presuppose. And if we understand the meaning of these narrower theories as being relativized to the background cosmology ("Genes are DNA molecules, whatever molecules are"), we block skeptical concern about the narrower theories based on the dubiousness of their cosmological backgrounds.

Nothing here is meant to prove that we ought not to be as skeptical of the warrant for the narrower theories as of the grander. Rather, it is meant to point out that insofar as there is *real* doubt, based on the contemplation of hypotheses yet unborn, it is doubt localized in general in the domain of fundamental theories. Such real doubt infects our confidence in the warrant of narrower theories only to the extent that they are not yet firmly established in the ordinary, inductive, sense. No doubt we are more confident in the theory of polymers than we are, say, in the theory of superconductivity or of plate tectonics. To that extent we remain more open to the possibility that there is some real alternative better explanation of the phenomena treated by the latter than we do to the possibility that some alternative to long-chain molecules will someday explain plastics better than current molecular chemistry does. But, in general, while both narrow and cosmological theories can be replete with empirical support, for the latter, but not for the former, we are genuinely assailed by doubts which originate in our awareness that there are more theories in Plato's heaven than have, as yet, been dreamt up by our theoreticians.

(b) There is another way of attempting to vitiate skepticism by re-understanding the content of theories which is generally applicable to both narrower and cosmological theories. The degree to which it can assuage our skeptical doubts is, however, quite problematic. Here I refer to the proposal that we take hypotheses not as asserting their manifest content but only as putting forth that content as approximately true or partially true.

Perhaps this approach can be motivated by referring, once again, to the downfall of the Newtonian system. As everyone knows, wrong as Newtonian dynamics and Newtonian gravitational theory may be, the theory remains a superb approximation to the truth in a vast variety of instances. Should we not, then, in anticipation of future scientific revolutions overthrowing our present cosmological theory, take our present fundamental theory to be, like Newton's, at best a body of partial and approximate truth? And if we do this can we not protect ourselves against the attack of the skeptic? For while his allusion to unborn hypotheses may very well make us dubious of our claim to hold the final truth, even the prospect of unimagined alternatives to the ones we have considered cannot lead

us to doubt that our present best cosmological theories are warranted as at least a good approximation to the truth or at least in part true.

One objection to this "way out" might be that although we can, indeed, have some assurance that our present best theories will always retain some value as approximate truths, we cannot now know, in the light of future hypotheses we might come up with much less in the light of possible new data, just where the present theory is going to fail to tbe reliable (or warranted as reliable) and where it will retain its approximative value. Thus, it might be argued, even weakening our attribution of content to one of mere approximate truth does not really help us in the face of skepticism of the kind we have been considering, since we can't now know, in the absence of knowledge of just what hypotheses are as yet unborn, just which parts of our current theory we are presently warranted as taking as even approximately true.

But, realistically, things aren't as bad as all that. Can't we be assured now, that whatever new hypotheses come along in the future, we will never give up our present estimation of the reliability of relativistic quantum field theory to predict the energy levels in the hydrogen atom? Couldn't Newton, ignorant of quantum mechanics and of relativity but knowledgeable of the fact that some theories he hadn't yet thought of would fit the data as well as the best theory he had come across, still be assured that whatever the future brought the inverse square law of gravity and Newtonian dynamics would remain inviolate as a good approximate account of planetary motion?

There is a far deeper objection, however, to the way out of skepticism which tells us to take the content attributed to presently believed hypotheses as only approximate and partial. Once again the real pressure of skepticism only arises when we consider our most fundamental, cosmological theories. It can be argued (and has frequently been) that the transition from Newton to relativity (not to speak of the transition to quantum mechanics) constituted a total shift to a new world picture completely incommensurable (as the cliché goes) with the picture of the world on the older theory. From this, very familiar, point of view there is no sense in which the older theory can be viewed as even approximately correct, misconstruing as it does (from the relativistic point of view) the most fundamental facts about spacetime, and, worse yet, (from the quantum mechanical point of view) being mired in the fallacious classical conception of an objective world independent of measuring apparatus, etc. Even as an approximation if we could make sense of such a notion, Newton's theory is a failure.

Seeing this, how could we not imagine the vast realm of hypotheses we have not yet thought up which differ as much in their ultimate picture of the world as our present physical picture differs from the Newtonian? And yet each of these hypotheses is to be imagined as fitting the data by which we support our present account as well as does our own best theory and better supported by it. Imagining such a realm of not yet conceived hypotheses, how can we be anything but skeptical of the warrant by which we hold to belief in our present theories—even viewing them merely as approximations to the truth?

Perhaps there is an anti-skeptical reply to this. It would require first making sense of the notion of conceptual approximation, showing us, against the familiar

arguments of the incommensurabilists, that one can make sense of one theory approximating the conceptual apparatus of another. Next the full reply to skepticism would have to indicate some way in which we could obtain some assurance that we need not now fear the existence, in the realm of as yet inconceived hypotheses, of numbers of sufficiently plausible hypotheses so radically different conceptually from our present best theory that we could not speak of our present best choice as even conceptually approximating these new alternatives. For were such unborn hypotheses to be now believed in by us, would we not be skeptical of the warrant for our current best theory? I have no idea how either a notion of conceptual approximation could be obtained, nor any idea how we could assure ourselves of the non-existence of plausible but radical still inconceived novelties.

4

There is an alternative method of attempting to evade skepticism which allows us to credit to our hypotheses the full content they seem manifestly to maintain. This option suggests, rather, that we modify our epistemic attitude toward the "winning" hypotheses in some direction away from the intuitive and naive notion of belief. That is, instead of saying that we believe the most successful of contending hypotheses (withholding belief, presumably, because of our awareness of the rich body of not-yet-conceived alternatives), we say, rather, that we hold some weaker, subtler epistemic attitude toward the victorious contender.

One such approach, of course, is to move from the dichotomy of belief and disbelief toward some version of epistemic probability—subjective, logical, or otherwise. But I don't think that this will be a satisfactory move at this point. For as we have seen earlier it would be too easy for the skeptic to push us to the point of admitting only zero probability for the hypotheses we have thought up. There is an anti-skeptical reply to this to which we will return later, however.

The approach I have in mind here, though, is to adopt a locution similar to those familiar to us from Popper. Since the totality of data is never in, and since Popper is skeptical of inductivist claims to warrant from samples, he argues that we ought not to believe even our best conjectures. Rather, we ought to speak of "tentatively holding" them or "momentarily adopting" them. Should we not, similarly, in the face off unborn hypotheses, forever withhold belief from our best current contender, instead adopting a fallibilistic attitude framed in some such jargon?

While there may be some value to this approach, in the end I think it the least useful way of confronting the skeptical challenge. The trouble is, of course, that just as "all the data" is never in, so we never exhaust in our consideration the totality of the Platonic universe of possible hypotheses. On the view under consideration, then, we ought to forever withhold belief, however partial. But if our stance is, forever, merely to adopt or maintain (or whatever) a current hypothesis, but at the same time to use it to explain the phenomena, predict the outcome of future experiments and control the world, then what useful distinction have we made between this allegedly weakened epistemic attitude and genuine

belief? Surely, moving in this direction smacks all too much of wanting to have one's skeptical cake while getting the nutritional advantages of consuming the meal, dessert and all.

5

The most interesting reply to the kind of skepticism we have been considering is that which attempts to meet it head on. Rather than evading skepticism by weakening the putative content of hypotheses which are accepted from their face value, or by reducing acceptance to something less than belief or partial belief, this approach argues that we might very well have good reasons for standing by our choice of the best of the contenders for belief, even acknowledging the vast array of as yet unexpressed alternatives we have ignored in the decision-making process. These reasons might be "quasi a priori" or they might, themselves, be a sort of induction from our empirical experience. Let us look at a few arguments of this sort.

(a) What could assure us of the reasonableness of our belief in the hypothesis selected as best from the set of available contenders even though we know there is an infinite plentitude of alternative hypotheses we simply haven't considered? Well, suppose we had some reason to believe that the hypotheses we have considered are all, in at least one crucial respect, and relative to present data of course, superior in warrant to all those not yet thought up. But how could we know or have reason to believe this when we don't even know what the unborn alternatives are?

Consider the *simplicity* of hypotheses. Whatever that is, and however we are to assign degrees of it to hypotheses, it is frequently said that, all other things being equal, we ought to believe the simplest of alternative hypotheses. Why we should prefer the simple hypothesis to the more complex is very hard to say. Without some plausible a priorism it is hard to connect simplicity with any plausible "mark of truth," and "pragmatic" rationales are notoriously difficult. to sustain in the epistemic context. Be that as it may, let us assume that it is right to believe the simpler rather than the more complex alternative (all other things, of course, being equal).

But if simpler hypotheses are preferable to less simple, do we not have at least some reason for dismissing the claims of skepticism founded upon the existence of as yet inconceived hypotheses? For do we have not, now, some fair warrant for believing that the hypotheses not yet thought up will be less simple than those which have come to mind?

By what right might we believe this? One reason could be the "empirical generalization" that, as a matter of psychological fact, scientists generally do think up simple hypotheses before the more complex alternatives occur to them. More interesting, if more speculative, is a "quasi a priori" argument: Perhaps the very meaning of simplicity is given by the order of imagination. Perhaps we simply call simpler those hypotheses we are (generally) likely to think up first. From this point of view simplicity, and hence plausibility, are granted to those hypotheses we have considered over those we have not by the very fact that the former have come to mind!

Here resort to historical cases may be informative, but the matter would have to be explored with some care. To be sure the data which Newton relied on in support of his theories of dynamics and gravitation can be seen to be compatible also with quantum mechanics and with special and general relativity. But, surely, even were Newton to be aware of those theories he would have been justified in sticking to his original hypotheses as the simplest, hence most worthy of belief, of the alternatives then in mind relative to the data available to him.

On the other hand there are some well known alternatives to the Newtonian theory, unavailable to Newton, which are such that we might very well be inclined to say that had Newton been aware of them he would have been obligated, by his very own standards of rational belief in science, to have opted for these alternatives and dropped his original theory, even relative to the data which he was in possession of at the time. I have here in mind so-called neo-Newtonian spacetimes both in their flat and curved version. The former provides an alternative to the Newtonian account which allows one to retain the empirical consequences of Newtonian dynamics without postulating a spacetime structure rich enough to allow the definability of absolute velocity, a notoriously "unobservable" quantity in Newton's original theory. The latter assimilates gravity to spacetime curvature, as in general relativity, but in such a way as to reproduce the empirical consequences of Newtonian gravitational theory. Again it has conceptual advantages over the original Newtonian theory, eliminating the distinction, present in the original theory, but empirically undeterminable, between the absence of a gravitational field and the presence of one whose uniformity makes it undetectable by fooling us into thinking that free-fall is inertial motion.

It is at least plausible to argue that had Newton been aware of these alternatives, considerations of simplicity (in some sense of that elusive concept) would have obligated him to prefer them as explanatory of the very data he used to back up his belief in his own theory. But here the alternatives to the original theory are those a positivist would declare mere trivial semantic variants of the older theory, for they are alternatives specifically designed to duplicate the totality of observational consequences of the original Newtonian theory.

It would be well worth a historical investigation to ask if there are any cases where the following pattern of development actually occurred: An original theory was well accepted by the scientific community to account for a range of empirical data. Later a new hypothesis was thought up which, while having some predictive consequences which have distinguished it as a genuine alternative theory from the original, was equally compatible with the original vis-à-vis the range of data which was taken to support the earlier theory. Further, the alternative was such that, even neglecting the possibility of testing it against the original theory by empirically exploring those regions where they give differing empirical predictions, a rational person would argue that had this theory been thought up at the time, it would have been more worthy of belief than the older theory at the time the original theory was adopted and in the light of the data which at that time supported the theory actually adopted.

I have not been able to think of a clear-cut example of this kind. Perhaps there are some. But they are surely few such cases. Isn't that alone somewhat persuasive in favor of the argument we have been considering: Either as a matter of empirical fact or of a priori truth, we just do think of the more believable hypotheses first.

Yet the example of neo-Newtonian spacetime, although, as explained, not quite the kind of example needed to counter the claim, will make one hesitate to accept this refutation of skepticism too glibly.

A variant of this account might go as follows, bringing us once again to the problem of a priori probabilities considered earlier: In any Bayesian theory of inference we need to assign to hypotheses intrinsic a priori probabilities. Could we not have some grounds, again either empirical or a priori, for assuming that hypotheses we have thought up have higher a priori probabilities than any in the vast array not yet brought to mind? Once again this belief could be founded on a psychological (historical?) generalization that people generally do think up intrinsically plausible hypotheses first. Or it could be founded on an attempt to show that our very assignment of a priori probabilities, on many accounts a "subjective" matter anyway, is just a numerical representation of the order in which hypotheses occur to us. Even the infinitude of hypotheses not yet brought to mind need not be devastating to this argument, since we could supplement it with some view about the way in which a priori probability clumps up among a few initial alternatives leaving little to be distributed among the vast totality remaining. Once again, a defense in depth of this position would not be easy to provide, but is there not some plausibility to the claim that although there are, indeed, many, many hypotheses we haven't yet considered, surely there is sometimes good reason to think that the ones we have brought to mind grab the lion's share of intrinsic believability amongst them?

(b) A related but interestingly different argument against the skeptic would have us rely not upon the general likelihood of simpler or more a priori plausible hypotheses coming to mind first, but, instead, upon a higher order inductive rationale to the view that, as things actually stand now in the present state of science, we need not be reduced by the spectre of unborn hypotheses to skeptical dismay.

The argument I have in mind goes something like this: At an early or "immature" state of science we should, to be sure, be extremely tentative in adopting any hypothesis merely because it is best among the competitors brought to consciousness. But as science enters its mature stage we need not be so diffident. On the basis of a kind of meta-level inductive reasoning we may have good reason to believe that the hypothesis on which we have fixed our belief is not only the best possible choice from among the alternatives we have considered, but is also superior to any hypothesis in the remaining body of those as yet unimagined. Indeed, we may have reason to think that of all those as yet not brought to mind, none is sufficiently viable that were it to occur to us it would result in any serious weakening of our confidence in the choice we have made.

Actually, the usual version is a little more modest than that. More often it would be claimed that while awareness of a novel hypothesis would make us lose some confidence in our currently accepted beliefs as good approximations (conceptual and otherwise) to the truth. That is, while few would argue that such higher order inductions to maturity of science would lead us to conclude that our science is finished, many would argue, I believe, that such inductions do justify us in considering our present beliefs well along the road to final maturity.

On what basis could such an inductive assurance of the unimportance of the inconceived be founded? Well, just as skepticism, real skepticism, about induc-

tion which refers to the vast array of evidential facts not yet considered could be at least partially muted by a consideration of the vast range and diversity of facts taken into account by our present theory, and the paucity of known phenomena as yet unexplained, skepticism of the kind we have been considering might be met by reference ot the enormous richness and diversity of hypothesis types that have been considered, the extent and intensity of imagination employed in thinking up all imaginable relevant alternatives, etc. Surely there is something in the reply of the scientist to the philosopher which is often heard: "You try to induce skepticism in me by referring to the vast array of hypotheses, concordant with the data, which we haven't yet considered. But we have thought long and hard looking for such novel alternatives and have come up with none—other than the silly philosophical variants of current theory which, in the present context, just don't count."

Of course the skeptic has the obvious reply. One's certainty of the maturity of present day science, in the sense we have been discussing, is really no more than one's certainty in the correctness (or approximate correctness) of current science. But this certainty of the irrelevance of unimagined alternatives is itself founded upon an ignorance of just what those alternatives might be. Just as we can induce skepticism with regard to current, inductively grounded, belief in science by referring to hypotheses not yet imagined, so such reference should be equally persuasive in inducing skepticism in the inductively grounded belief that, as a matter of fact, no such unimagined hypothesis would reduce confidence in our current beliefs were they to come to light.

6

In the light of the considerations above, it is worth while asking what our epistemic attitude ought to be, at the present time, to our best available "cosmological" theory. Can we be assured that it is not the case that our paucity of imagination has caused us to overlook alternatives to our present fundamental physical theory which, were we to be aware of them, we would think far more plausible candidates as correct explainers of present data?

It certainly is the case that confidence in the immutability of our present theory, at least as good conceptual approximation to the ultimate accepted hypothesis, is not universal. While many of the conceptual difficulties of relativistic quantum field theory, the closest thing we have to an overall fundamental theory at the present time, appear to be the kind of difficulties which further refinement will eliminate (I refer here to problems of divergence, renormalization, etc.), the conceptual peculiarities of the underlying general quantum formalism leave many very skeptical indeed that it, or anything quite like it, will be the ultimate way in which we will view the world. The difficulties involved in characterizing a measurement, in understanding the curious non-causal non-locality of correlations, etc., are well known.

But at least three distinct scenarios can be offered by someone who predicts the ultimate demise of our present conceptual scheme in physics:

(1) Someone sufficiently clever will eventually see how to construct an alternative scheme which, having exactly the same experimental consequences as present day quantum theory, lacks the conceptual elements in it which lead to difficulties in understanding. This would parallel the discovery of neo-Newtonian

spacetimes as conceptually superior reconstructions of the original Newtonian theories of dynamics and gravitation.

(2) Someone sufficiently clever will eventually discover an alternative to quantum mechanics. This new theory will differ from quantum mechanics in some of its observational predictions. But the data which presently supports quantum mechanics will support this theory equally well. On the other hand, since the novel alternative is so much conceptually superior to quantum mechanics, even prior to a test of the observational areas where the new theory differs from the present one, many will affirm that the new theory is a more plausible account of the old data than is quantum mechanics, and that had we been clever enough to think it up in the first place we would have seen from the very beginning how implausible quantum mechanics was relative to this, as it happened, not then thoughtup competitor.

(3) Prior to the discovery of new data not compatible with quantum mechanics, no one will think up anything better. New observational results will, however, eventually lead to a new fundamental theory. Of course this new theory, incompatible with quantum mechanics as it may be, will account for all the observational results which made quantum theory seem so plausible before the new observational results were in. Even accepting this new theory, though, we will say that those who accepted quantum mechanics on the basis of the old data would have been justified in doing so even if this new theory had occurred to them. Just as Newton would have had a right to reject general relativity, on the basis of the claim that relative to the data then available, Newtonian theory was the simpler, and hence more plausible, explanatory account; in the light of this new theory we will still feel that, relative to the old data, quantum theory was indeed still the most plausible explanation.

Once again it is possibility (2) which interests us most here. Someone skeptical of the current theory on the basis of possibility (1) can be countered as usual with the positivist claim of equivalence of the alternatives. Someone skeptical on the basis of possibility (3) will be told that, of course, new data can always make us reject present theory in terms of a novel alternative, but that is no reason to be skeptical of the support present data gives present theory. Possibility (2) is the one which simultaneously tells us to be skeptical of even the present support quantum mechanics receives from present data, but, at the same time, gives us hope that even relative to present experimental results a diligent pursuit of imagined alternatives may very well lead us to an account of present experimental facts better than the one we presently, for want of a better alternative, accept.

Given the conceptual "nastiness" of quantum theory, one might well wish that the hopeful skeptic is right.

The University of Michigan
Ann Arbor, Michigan

NOTES

*An earlier version of this paper was presented as an invited address at the March, 1980, meetings of the Pacific Division of the American Philosophical Association. Work on this paper was partially supported by a grant from the National Science Foundation, grant number SOC-7905696.