Bayes or Bust?
A Critical Examination of Bayesian Confirmation Theory

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8 Normal Science, Scientific Revolutions, and All That: Thomas Bayes versus Thomas Kuhn

1 Kuhn's *Structure of Scientific Revolutions*

Read in the context of the then prevailing orthodoxy of logical empiricism, the first edition (1962) of Kuhn's *Structure of Scientific Revolutions* seemed to offer a novel and indeed radical account of the nature of scientific change. For those who do not have a copy of *Structure* at hand, here is a sample of a few of the purple passages:

Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life.... When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense. (P. 94)

As in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall therefore have to examine not only the impact of nature and logic, but also the techniques of persuasive argumentation within the quite special groups that constitute the community of scientists. (P. 94)

The proponents of competing paradigms practice their trades in different worlds.... Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction. (P. 150)

In these matters neither proof nor error is at issue. The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced. (P. 151)

Before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a shift between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like a gestalt switch, it must occur all at once (though not necessarily at an instant) or not at all. (P. 150)

Many readers saw in these passages an open invitation to arationality, if not outright irrationality. Thus Imre Lakatos took Kuhn to be saying that theory choice is a matter of "mob psychology" (1970, p. 178), while Dudley Shapere read Kuhn as saying that the decision to adopt a new paradigm "cannot be based on good reasons"(1966, p. 67). Kuhn, in turn, was equally shocked by such criticisms. In the Postscript to the second edition (1970) of *Structure* he professed surprise that readers could have imposed such unintended interpretations on the above quoted passages. Let us agree to leave aside the unfruitful question of whether or not Kuhn

ought to have anticipated such interpretations and to concentrate instead on what, upon reflection, he intended to say.

Kuhn's own explanation in the Postscript begins with the commonplace that "debate over theory-choice cannot be cast in a form that resembles logical or mathematical choice" (p. 199). But he hastens to add that this commonplace does not imply that "there are no good reasons for being persuaded or that these reasons are not ultimately decisive for the group."[1] The reasons listed in the Postscript are accuracy, simplicity, and fruitfulness. The later paper "Objectivity, Value Judgments, and Theory Choice" (1977) added two further reasons: consistency and scope. And as Kuhn himself notes, the final list does not differ (with one notable exception to be discussed later) from similar lists drawn from standard philosophy-of-science texts (see also Kuhn 1983).

These soothing sentiments serve to deflate charges of arationality and irrationality, but at the same time they also serve to raise the question of how Kuhn's views are to be distinguished from the orthodoxy that *Structure* was supposed to upset. The answer given in the Postscript contains two themes elaborated in "Objectivity." First, the items on the above list are said to "function as values" that can "be differently applied, individually and collectively, by men who concur in honoring them" (p. 199). Thus, "There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision." Second, it follows (supposedly) that "it is the community of specialists rather than the individual members that makes the effective decision" (p. 200).

I think that Kuhn is correct in locating objectivity in the community of specialists, at least in the uncontroversial sense that intersubjective agreement among the relevant experts is a necessary condition for objectivity.[2] But how the community of experts reaches a decision when the individual members differ on the application of shared values is a mystery that to my mind is not adequately resolved by *Structure* or by subsequent writings. I will have more to say about this and related mysteries below.

My strategy will be to explore these and other issues raised by *Structure* from the perspective of Bayesian methodology. Before launching the exploration, I need to make some remarks about two of the most notable and controversial doctrines of *Structure*: the incommensurability of paradigms and the nonexistence of a theory-neutral observation language.

2 Theory-Ladenness of Observation and the Incommensurability of Theories

Part of what is meant by the theory-ladenness of observation is embodied in the thesis that what we see depends upon what we believe, a thesis open to challenge (see Fodor 1984). I am concerned rather with the related thesis of the nonexistence of a neutral observation language in which different theories can be compared. My response, for present purposes, is tactical. That is to say, without trying to adjudicate the general merits of the thesis, my claim is that things aren't so bad for actual historical examples. Even for cases of major scientific revolutions we can find, without having to go too far downward toward something like foundations for knowledge, an observation base that is *neutral enough for purposes at hand*. A nice example is provided by Alan Franklin (1986, pp. 110–113), who shows how to construct an experiment that is theory-neutral enough between Newtonian and special relativistic mechanics to unambiguously decide between the predictions of these theories for elastic collisions. The two theories agree on the procedure for measuring the angle between the velocity vectors of the scattered particles, and the two theories predict different angles.

More generally, I claim that in the physical sciences there is in principle always available a neutral observation base in spatial coincidences, such as dots on photographic plates, pointer positions on dials, and the like. If intersubjective agreement on such matters were not routine, then physical science as we know it would not be possible. I reject, of course, the positivistic attempt to reduce physics to such coincidences. And I readily acknowledge that such coincidences by themselves are mute witnesses in the tribunal for judging theories. But what is required to make these mute witnesses articulate is not a gestalt experience but a constellation of techniques, hypotheses, and theories: techniques of data analysis, hypotheses about the operation of measuring instruments, and auxiliary theories that support bootstrap calculations of values for the relevant theoretical parameters that test the competing theories. But I again assert that the practice is not science to the extent that this process cannot be explicitly articulated but relies on some *sui generis* form of perception. This is not to say, however, that the vulgar image of science as a blindly impartial enterprise is correct, for the articulation uncovers assumptions to which different scientists may assign very different degrees of confidence. But the

sense in which different scientists can (misleadingly) be said to "see" different things when looking at the same phenomenon is one with which a probabilistic or Bayesian epistemology must cope on a routine basis, even in cases far away from the boundaries of scientific revolutions. How these differences are resolved is part of the Bayesian analogue of Kuhn's problem of community decision on theory choice. Kuhn's problem will be encountered in the following section, and the Bayesian analogue will be discussed below in sections 5 and 6.

The matter of incommensurability is much more difficult to discuss for two reasons. First, it is tied to controversial issues about meaning and reference, which I cannot broach here. Second, the topic of incommensurability presents an amorphous and shifting target. In *Structure*, for example, incommensurability was a label for the entire constellation of factors that lead proponents of different paradigms to talk past one another. In recent years Kuhn has come around to a more Carnapian or linguistic formulation in which incommensurability is equated with untranslatability. More specifically, the focus has shifted from paradigms to theories, and two theories are said to be incommensurable just in case "there is no common language into which both can be fully translated" (Kuhn 1989, p. 10). I have no doubts about Kuhn's claim that theories on different sides of a scientific revolution often use different "lexicons," that differences in lexicons can make for a kind of untranslatability, and that in turn this explains why scientists reading out-of-date texts often encounter passages that "make no sense" (1989, p. 9). But I deny that incommensurability or untranslatability in a form that makes for insuperable difficulties for confirmation or theory choice (a phrase I don't like for reasons to be given below) applies to the standardly cited cases of scientific revolutions, such as the transition from Newtonian to special-relativistic mechanics and the subsequent transition to general relativity. Newtonian, special-relativistic, general-relativistic, and theories of many other types can all be formulated in a common language, the language of differential geometry on a four-dimensional manifold, and the crucial differences in the theories lie in the differences in the geometric object fields postulated and the manner in which these fields relate to such things as particle orbits.[3] This language is anachronistic and therefore may not be the best device to use when trying to decide various historical disputes.[4] But it does seem to me to be an appropriate vehicle for framing and answering the sorts of questions of most concern to working physicists and philosophers of science. For exam-

ple, on the basis of the available evidence, what is it reasonable to believe about the structure of space and time and the nature of gravitation? This is not to say that the common language makes for an easy answer. It is indeed a difficult business, but it is a business that involves the same sorts of difficulties already present when testing theories that lie on the same side of a scientific revolution. Finally, so that there can be no misunderstanding, let me repeat that I am not claiming that what I call a common language provides what Kuhn means by that term. It does not, for example, show that the Newtonian and the Einsteinian can be brought into agreement about what is and is not a "meaningful" question about simultaneity. What I do claim is that these residual elements of incommensurability do not undermine standard accounts of theory testing and confirmation.

My response to worries about the applicability of the notion of truth to whole theories is similarly local and tactical. In the Postscript to the second edition of *Structure* Kuhn writes, "There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle" (1970, p. 206). I need not demur if 'theory' is understood in a *very* broad sense to mean something like a conceptual framework so minimal that without it "the world" would be undifferentiated Kantian ooze.[5] But I do demur if 'theory' is taken in the ordinary sense, i.e., as Newton's theory or special relativity or general relativity. For scientists are currently working in a frame in which they can say, correctly I think, that the match between the ontology of the theory and its real counterpart in nature is better for the special theory of relativity and even better for the general theory than it is for Newton's theory. Of course, to get to this position required two major conceptual revolutions. How such revolutions affect theory choice—or as I would prefer to say theory testing and confirmation—remains to be discussed.[6]

3   Tom Bayes and Tom Kuhn: Incommensurability?

Kuhn's list of criteria for theory choice is conspicuous for its omission of any reference to the degree of confirmation or probability of the theories. This is no oversight, of course, but derives both from explicit doctrines, such as the nonexistence of a theory-neutral observation language, and the largely tacit but nevertheless pervasive anti-inductivism in *Structure*. Just

as striking from the Bayesian perspective is Kuhn's emphasis on theory choice or acceptance, since for the Bayesian, theories are not chosen or accepted but merely probabilified. Only in the exceptional cases where the probability is 0 or 1, or so close to one of these values as makes no odds, would there seem to be a natural commensurability between Tom Kuhn and Tom Bayes.

For the Bayesian, various problems and puzzles raised in *Structure* disappear. For example, consider the illustration Kuhn uses to reveal the difficulty of applying the criterion of theory choice most closely related to degree of confirmation: accuracy. Kuhn notes that although accuracy is the most decisive of his five criteria, it cannot uniformly discriminate among theories.

Copernicus's system ... was not more accurate than Ptolemy's until drastically revised by Kepler.... If Kepler or someone else had not found other reasons to choose heliocentric astronomy, those improvements in accuracy would never have been made, and Copernicus's work might have been forgotten. More typically, ... accuracy does permit discriminations, but not of the sort that lead regularly to unequivocal choice. The oxygen theory, for example, was universally acknowledged to account for observed weight relations in chemical reactions, something the phlogiston theory had previously scarcely attempted to do. But the phlogiston theory, unlike its rival, could account for metals' being much more alike than the ores from which they were formed. One theory thus matched experience better in one area, the other in another. (1977, p. 323)

To give a brief Bayesian commentary to this passage, we are dealing here with different times and thus with different bodies of evidence and different versions of a theory. There is no puzzle in the fact that if either of the corresponding members of the pairs (T, E) and (T', E') are different, $Pr(T/E)$ and $Pr(T/E')$ may be different. Nor as a Bayesian should I be worried by the fact that while on the basis of current evidence I regard the current form of T as highly probable, I regarded previous forms of T on the basis of the evidence then available as having a low (but nonzero) probability, for I was never in danger of rejecting T (or of accepting or choosing its negation), since acceptance and rejection of theories is not my game.

A shotgun marriage of the two Toms could be arranged in either of two ways. We could take Bayes to supply the probabilities, Kuhn to supply the values or utilities, and then we could apply the rule of maximizing expected utility to render a decision on theory choice. Or we could take the Kuhnian virtues as helping to determine the probabilities—simplicity presumably

affects the prior probabilities of the theories, while accuracy affects their posteriors—and then we could choose the most probable theory. But like most shotgun marriages, these would be mistakes. For Bayes, they would be mistakes because they would involve the pretense that the accepted theory T is true even though one's degree of belief in T is less than 1, perhaps substantially so. For Kuhn, they would be mistakes because the efficacy of his values in no way depends upon the truth of the theories, so estimates of the probable truth of theories are irrelevant to Kuhnian theory choice.

Part of the wrangle here derives from the unfortunate phrase 'theory choice'. Scientists do choose theories, but on behalf of the Bayesians, I would claim that they choose them only in the innocuous sense that they choose to devote their time and energy to them: to articulating them, to improving them, to drawing out their consequences, to confronting them with the results of observation and experiment.[7] Choice in this sense allows for a reconciliation of Bayes and Kuhn, since this choice is informed by both Bayesian and Kuhnian factors: probability and the values of accuracy, consistency, scope, simplicity, and fruitfulness.

Alas, this reconciliation is rather shallow. Once we are clear that the sort of choice involved in "theory choice" is a practical one, then there is nothing sacred about the list of items on Kuhn's list of values. Other values, such as getting an NSF grant or winning the Nobel Prize, can and do play a role. Further, the kind of choice in question may be bigamous, since a scientist can choose to work on two or more theories at once, and it is fickle, since it can oscillate back and forth. The kind of choice *Structure* envisioned was much more permanent; indeed, the impression given there is that normal science is not possible without tying Catholic bonds to a theory, bonds that may only be broken by leaving the Church, i.e., by creating a revolution.

Is there no way to bridge the gap between the two Toms on this issue? To explain how baffling the Bayesian finds the notion of theory acceptance, consider the case of Einstein's GTR, arguably the leading theory of gravitation and thus the top candidate for "acceptance." Marie, a research worker in the field familiar with all of the relevant experimental findings, does some introspection and finds that her degree of belief in GTR is p.

Case 1. Marie's degree of belief p is 1, or so near 1 as makes no odds. Then, as already remarked, there is a natural sense in which the Bayesian can say

that Marie accepts GTR. Such cases, however, are so rare as to constitute anomalies. Of course, one can cite any number of cases from the history of science where scientists seem to be saying that for their pet theory they set $p = 1$. Here I would reissue the warning of chapter 4 that we must distinguish carefully between scientists qua advocates of theories and scientists qua judges of theories. It is the latter role that concerns us here, and in that role, scientists know, or should know, that only in very exceptional cases does the evidence rationally support a full belief in a theory.

Case 2. Marie's degree of belief p is, say, .75. Subsequently Marie decides, on the basis of her probability assignments and the values she attaches to GTR and its competitors, to "accept" GTR. What could this mean?

Subcase 2.a. When she accepts GTR, Marie changes her degree of belief from .75 to 1. This is nothing short of madness, since she has already made a considered judgment about evidential support and no new relevant evidence occasioning a rejudgment has come in.

Subcase 2.b. When she accepts GTR, Marie does not change her degree of belief from .75 to 1, but she acts *as if* all doubt were swept away in that she devotes every waking hour to showing that various puzzling astronomical observations can be explained by the theory, she assigns her graduate students research projects that presuppose the correctness of the theory, she writes a textbook on gravitational research devoted almost exclusively to GTR, etc. But at this point we have come full circle back to a sense of theory acceptance that is really a misnomer, for what is involved is a practical decision about the allocation of personal and institutional resources and not a decision about the epistemic status of the theory.[8]

This rather pedantic diatribe on theory acceptance would best be forgotten were it not for its implications for the picture of normal science. As we have seen, theory "choice" or "acceptance" can refer either to adopting an epistemic attitude or to making a practical choice. As for the former, there is no natural Bayesian explication of theory acceptance, save in the case where the probability of the theory is 1. Since scientists qua judges of theories are almost never in a position to justify such an acceptance, the Bayesian prediction is that rarely is a theory accepted in the epistemic sense. Similarly, when theory choice is a matter of deciding what theory to devote one's time and energy to, the Bayesian prediction is that in typical situations where members of the community assign different utilities to such devotions, they will make different choices. Thus, from either the

epistemic or practical perspective, the Bayesian prediction is for diversity. This prediction is, I think, borne out by actual scientific practice. Thus in section 6, I will argue that insofar as normal science implies a shared paradigm, the paradigm need not, and in fact often is not, so specific as to include a particular ("accepted") theory. I will also hazard a proposal for a minimal sense of 'shared paradigm' that yields a less straitjacketed image of normal science and that also diminishes, without obliterating, the difference between normal and revolutionary science.

By way of closing this section and introducing the next, let me propose a final way of reconciling the Kuhnian and Bayesian pictures when scientific revolutions are in the offing. Radically new theories (so the story goes) carry with them different linguistic or conceptual frameworks. Thus, to even seriously entertain a new theory involves the decision to adopt, if only tentatively, the new framework. And this decision is in large part a pragmatic one, involving the factors emphasized in Kuhn's account of paradigm replacement. These considerations certainly impact on Bayesianism, since, as discussed in chapter 3, probability assignments depend on the linguistic and conceptual framework adopted. (So while it is not true, as C. I. Lewis claimed, that if anything is to be probable, then something must be certain, it is true that if anything is to be probable, something must be accepted. But that something is not a statement, whether of evidence or theory, but a framework that specifies the possibilities to be considered.) In response, let me begin by repeating my cautionary claim that major scientific revolutions need not be seen as forcing a choice between incommensurable linguistic or conceptual frameworks, since it is often possible to fit the possibilities into a larger conceptual scheme that makes the theories commensurable to the extent that there is an observation base that is neutral enough for purposes of assessing the relative confirmation of the theories. But I agree that the recognition of the larger possibility set can produce changes in the probability values and that those changes are often best described in Kuhnian terms.

4   Revolutions and New Theories

For Bayesians, a scientific revolution is not to be identified with the replacement of a paradigm in the sense of an accepted theory, since, as argued in the preceding section, Bayesians eschew theory acceptance. I suggest rather that revolutions be identified with the introductions of new

theories. Such revolutions can come in one of two forms. The mildest form occurs when the new theory articulates a possibility that lay within the boundaries of the space of theories to be taken seriously but that, because of the failure of logical omniscience ((LO2) in the language of chapter 5), had previously been unrecognized as an explicit possibility. The more radical form occurs when the space of possibilities is itself significantly altered. In practice, the distinction between the two forms may be blurred, perhaps even hopelessly so, but I will begin discussion by pretending that we can separate cases.[9]

Even the mild form of revolution induces a non-Bayesian shift in belief functions. By 'non-Bayesian' I mean that no form of conditionalization, whether strict or Jeffrey or some natural extension of these, will suffice to explain the change. Conditionalizing (in any recognizable sense of the term) on the information that just now a heretofore unarticulated theory T has been introduced is literally nonsensical, for such a conditionalization presupposes that prior to this time there was a well-defined probability for this information and thus for T, which is exactly what the failure of logical omniscience rules out.

As previously noted, we can try to acknowledge the failure of logical omniscience (LO2) by means of Abner Shimony's (1970) device of a catch-all hypothesis H_C, which asserts in effect that something, we know not what, beyond the previously formulated theories $T_1, T_2, \ldots, T_q$ is true. Now suppose that a new theory T is introduced and that as a result the old degree-of-belief function Pr is changed to Pr'. The most conservative way the shift from Pr to Pr' could take place is by the process I will call *shaving off*; namely, $\Pr(T_i) = \Pr'(T_i)$ for $i = 1, 2, \ldots, q$, $\Pr'(T) = r > 0$, and $\Pr'(H_C) = \Pr(H_C) - r$. That is, under shaving off, H_C serves as a well for initial probabilities for as yet unborn theories, and the actual introduction of new theories results only in drawing upon this well without disturbing the probabilities of previously formulated theories. Unfortunately, such a conservatism eventually leads to the assignment of ever smaller initial probabilities to successive waves of new theories until a point is reached where the new theory has such a low initial probability as to stand not much of a fighting chance.

Certainly shaving off is a factually inadequate description of what happens in many scientific revolutions, especially of the more radical type. Think of what happened following the introduction of Einstein's STR in 1905. Between 1905 and 1915 little new empirical evidence in favor of STR

was recorded; and yet the probability of competing theories, such as those of Lorentz and Abraham, set in classical space and time, fell in the estimates of most of the members of the European physics community, and the probability subtracted from these electron theories was transferred to Einstein's STR. The probabilities of auxiliary hypotheses may also be affected, as illustrated by the introduction of GTR. When Einstein showed that GTR accounted for the exact amount of the anomalous advance of Mercury's perihelion, the hypothesis of an amount of zodiacal matter sufficient to affect Mercury's perihelion dropped dramatically in the estimates of most of the physics community.[10]

In using the term 'non-Bayesian' to describe such nonconditionalization belief changes, whether of the conservative shaving-off type or some more radical form, I do not mean to imply that the changes are not informed by Bayesian considerations. Indeed, the problem of the transition from Pr to Pr' can be thought of as no more and no less than the familiar Bayesian problem of assigning initial probabilities, only now with a new initial situation involving a new set of possibilities and a new information basis. But the problem we are now facing is quite unlike those allegedly solved by classical principles of indifference or modern variants thereof, such as E. T. Jaynes's maximum entropy principle, where it is assumed that we know nothing or very little about the possibilities in question. In typical cases the scientific community will possess a vast store of relevant experimental and theoretical information. Using that information to inform the redistribution of probabilities over the competing theories on the occasion of the introduction of the new theory or theories is a process that is, in the strict sense of the term, *a*rational: it cannot be accomplished by some neat formal rules or, to use Kuhn's term, by an algorithm. On the other hand, the process is far from being *ir*rational, since it is informed by reasons. But the reasons, as Kuhn has emphasized, come in the form of persuasions rather than proof. In Bayesian terms, the reasons are marshalled in the guise of plausibility arguments. The deployment of plausibility arguments is an art form for which there currently exists no taxonomy. And in view of the limitless variety of such arguments, is it unlikely that anything more than a superficial taxonomy can be developed. Einstein, the consummate master of this art form, appealed to analogies, symmetry considerations, thought experiments, heuristic principles (such as the principle of equivalence), etc. All of these considerations, I am suggesting on behalf of the Bayesians, were deployed to nudge assignments of initial probabilities in

favor of the theories Einstein was introducing in the early decades of this century. Einstein's success in this regard is no less important than experimental evidence in explaining the reception of his theories.

There is little to be salvaged from the Bayesian model of learning as conditionalization by claiming that, although the model fails in periods of scientific revolutions, it nevertheless holds for periods of normal science. For normal science defined as the absence of even a weak revolution shrinks to near the vanishing point. New observations, even of familiar scenes; conversations with friends; idle speculations; dreams—all of these and more are constantly introducing heretofore unarticulated possibilities and with them resultant nonconditionalization shifts in our degrees of belief, often of a non-shaving-off variety. All that remains of Bayesianism in its present form is the demand that new degrees of belief be distributed in conformity with the probability axioms. This is a nontrivial constraint, but by itself it induces only the uninteresting de Finetti form of subjectivism.

This suggests that the term 'scientific revolution' be reserved for the second and more radical form of revolution I distinguished above. For a revolution in this sense, Kuhn's purple passages do not seem overblown. The persuasions that lead to the adoption of the new shape for the possibility space cannot amount to proofs. Certainly for the Bayesian, they cannot consist of inductive proofs, since the very assignment of degrees of belief presupposes the adoption of such a space. After a revolution has taken place, the new and old theories can often be fitted into a common frame that belies any vicious form of incommensurability (as illustrated in section 2 for Newtonian and relativistic theories). But this retrospective view tends to disguise the shake-up in our system of beliefs occasioned by the adoption of the new shape for the possibility space. Bayesianism brings the shake-up to light, albeit in a way that undercuts the standard form of the doctrine.

5 Objectivity, Rationality, and the Problem of Consensus

I have endorsed a Bayesianized version of Kuhn's claim that in scientific revolutions persuasion rather than proof is the order of the day: revolutions involve the introduction of new possibilities, this introduction causes the redistribution of probabilities, the redistribution is guided by plausibility arguments, and such arguments belong to the art of persuasion.

This endorsement is confined to the first stage of the revolution, when the initial probabilities are established for the expanded possibility set. The Bayesian folklore would have it that after this first stage, something more akin to proof than persuasion operates. The idea is that an evidence-driven consensus emerges as a result of the Bayesian learning model: degrees of belief change by conditionalization on the accumulating evidence of observation and experiment, and the long-run result is that a merger of posterior opinion must take place for those Bayesian agents who initially assign zeros to the same hypotheses. In this chapter I have raised doubts about the conditionalization model. And in chapter 6, I showed why the mathematically impressive merger of opinion theorems are of dubious applicability to the sorts of cases discussed in *Structure*.

If honest theorem proving won't suffice, perhaps we can define our way to a solution. That is, why not define 'scientific community' in terms of de facto convergence of opinion over a relevant range of hypotheses? The answer is the same as that given by Kuhn in his Postscript to the threatened circularity of taking a paradigm to be what members of the community share, while also taking a scientific community to consist of just those scientists who share the paradigm. Just as scientific communities "can and should be isolated without prior recourse to paradigms" (p. 176), so they can and should be isolated without recourse to convergence-of-opinion behavior. The European physics community in the opening decades of this century can be identified by well-established historical and sociological techniques, and one wants to know how and why, for example, this community so identified reached a consensus about Einstein's STR. Nevertheless, there does seem to be at least this much truth to the definitional move: repeated failures to achieve merger of opinion on key hypotheses will most likely lead to a split in, or a disintegration of, the community.

At this juncture, let us recall Kuhn's idea that since there is "no neutral algorithm for theory-choice, no systematic procedure which, properly applied, must lead each individual in the group to the same decision ..., it is the community of specialists rather than its individual members that makes the effective decision." Even in Kuhn's own terms, I find this idea puzzling, since I do not find in *Structure* a clear account of how the group decision is to be effected. But since I have argued that there is no need to choose a theory in the choose-as-true sense and since there is no need to achieve consensus on theory choice in the choose-to-investigate-and-articulate sense, this puzzle is moot. However, the Bayesian analogue of this

puzzle remains; namely, how is the community to operate so as to produce a Bayesian consensus when its members have divergent degrees of belief?

One mechanism discussed by Lehrer and Wagner in *Rational Consensus in Science and Society* (1981) requires that members of the community change their degrees of belief in accordance with a weighted-aggregation rule. Suppose that at the initial moment, person i has a degree of belief p_i^0 in the theory in question. Each person i is assumed to assign a weight $w_{ij} \geqslant 0$ to every person j, which can be taken as an index of i's opinion as to the reliability of j's opinions. According to Lehrer and Wagner's rule, i then "improves" her initial opinion p_i^0 by changing it to $p_i^1 = \sum_j w_{ij} p_j^0$. If there are still differences of opinion, the aggregation process is repeated with the p_i^1 to obtain further "improved" probabilities p_i^2, etc., until eventually the probabilities for all the members fall into line.[11]

Lehrer and Wagner offer a consistency argument for their aggregation rule: "If a person refuses to aggregate, though he does assign a positive weight to other members, he is acting as though he assigned a weight of one to himself and a weight of zero to every other member of the group. If, in fact, he assigns positive weight to other members of the group, then he should not behave as if he assigned zero weight to them" (1981, p. 22). This argument has the flavor of 'When are you going to stop beating your wife?' I do assign a positive weight to the opinions of others, but as a Bayesian I do this not by means of weighted aggregation but by conditionalization. I conditionalize on information about the opinions of my peers, and I notice that the result is a shift in my degrees of belief toward the degrees of belief of those I respect. As a young student these shifts brought my opinions closely in line with those belonging to people whom I regarded as the experts, but as a mature member of the community, I find that such shifts, while still nonnegligible, do not conform my opinions to those of others, at least not on matters where I now regard myself as an expert. And I resist any attempt to bend my carefully considered opinions.

Furthermore, there is a direct clash between Bayesianism and Lehrer and Wagner's type of rule for producing consensus. Let the 'improved' or consensus probability Pr be a weighted average $\sum_k \beta_k \text{Pr}_k(\cdot)$, $0 \leqslant \beta_k \leqslant 1$ and $\sum_k \beta_k = 1$, of the individual probabilities Pr_k. Such a rule commutes with strict conditionalization only if there is dictatorship in the sense that one of the β_k's is 1 (see Berenstein, Kanal, and Lavine 1986).

Independently of Bayesianism, there are two reasons to be unhappy with the Lehrer and Wagner proposal and ones like it. The first is that it

is descriptively false, as shown by the very example they use to motivate their proposal. In the 1970s Robert Dicke claimed that optical measurements of the solar disk revealed an oblateness large enough to account for $3''$ to $5''$ of arc in Mercury's centenary perihelion advance and hence to throw into doubt Einstein's explanation of the advance. When other astrophysicists disagreed with Dicke's conclusions, the differences were not smoothed over by producing a consensual probability by means of a weighted-aggregation process. The weight of opinion is now against Dicke's interpretation, but this agreement is in fact not due to aggregation but to the acquisition of additional evidence.

Of course, Lehrer and Wagner are perfectly aware of these facts, and the descriptive inadequacies of their proposal do not bother them, since they take themselves to be offering a normative proposal. But even in these terms, the proposal is to be faulted. It is fundamental to science that opinions be evidence-driven. Differences of opinion need not constitute an embarrassment that needs to be quashed, for these differences can serve as a spur to further theoretical and experimental research, and the new information produced may drive a genuine scientific consensus. If not, the attempt to manufacture a consensus by a weighted-aggregation procedure smacks of the "mob psychology" for which Kuhn was criticized.

This last point generalizes. Bayesianism, and other approaches to scientific inference as well, suggest that unless there is some evidence-driven process that operates on the level of individual scientists to produce a group consensus, the consensus will amount to something that, if not mob psychology, is nevertheless a social artifact that does not deserve either of the labels 'rational' or 'scientific'. Thus, contrary to Kuhn's idea, the group cannot decide; at least it cannot rationally decide to agree if the individuals disagree. I do not see how this conclusion can be escaped, unless some yet-to-be articulated collectivist methodology is shown to be viable.

6   A Partial Resolution of the Problem of Consensus

Part of the answer to the Bayesian version of the problem of consensus is that quite often a consensus does not exist and does not need to exist for normal scientific research to take place.[12] *Structure* warned of the danger of taking textbook science as our image of how real science actually operates, and in particular, it showed how textbook science tends to make scientific revolutions invisible by painting an overly rosy picture of a

smoothly accumulating stockpile of scientific knowledge. But I think that *Structure* failed to emphasize how textbook science also disguises the diversity of opinions and approaches that flourish in nonrevolutionary science.

Consider again the case study on relativistic gravitational theory developed in chapter 7. Textbooks in this area have tended to be books on Einstein's GTR, thus fostering the illusion that GTR has achieved the status of paradigm hegemony. In addition, early textbooks not only downplayed the existence of rival theories but disguised serious difficulties with two of the principal experimental tests of GTR, the red shift and the bending of light. Normal scientific research in this field continued in the face of both a challenge (deriving from Dicke's solar-oblateness measurements) to the third experimental leg of GTR and also an ever growing number of rival theories of gravitation. This and similar examples suggest that for normal science to take place, the community of experts need only share a paradigm in the weak sense of agreement on the explanatory domain of the field, on the circumscription of the space of possible theories to be considered as serious candidates for covering the explanatory domain, on exemplars of explanatory success, and on key auxiliary hypotheses. (I am tempted to say that this is the minimal sense of paradigm needed to underwrite normal science, but historians of science probably have counterexamples waiting in the wings.)

One could argue that not having a paradigm in the stricter sense of a shared theory of gravitation has lowered the puzzle-solving efficiency of normal science. In this regard, one can recall Thorne and Will's 1971 statement that, faced with a zoo of alternative theories of gravitation, astrophysicists were hamstrung in their model-building activity. While I think that this is a fair observation, I also think that there is more to progress in normal science than puzzle solving. Chapter 7 emphasized the conceptual advances derived from the exploration of the space of possible theories, a point that brings me to the second part of the answer to the problem of consensus.

Insofar as a consensus is established, it is often due to a process akin to the eliminative induction described in chapter 7. This process is typically accompanied by a proliferation of theories not as an exercise in Feyerabendian anarchy or Dadaism but as a means of probing the possibilities and as a preliminary to developing a classification scheme that makes systematic elimination a tractable exercise. Since such elimination

is not of the simpleminded Sherlock Holmes variety and involves Bayesian elements, it is well to remind ourselves of the prospects and problems of achieving a rational consensus in this way. According to the results of chapter 6, a merger of opinion for a maximal class of equally dogmatic belief functions will not be achievable, not even in the infinitely long run, if the possible theories are underdetermined by the data. Despite all of the philosophers' talk about underdetermination, its actual extent in real cases is unclear. Underdetermination aside, a consensus would be achieved by a convergence of opinion on the (possibly multiply quantified) observational predictions that separate the competing theories. Whether such a convergence takes place in the short or medium runs depends on the class of belief functions. Typically, it cannot set in rapidly for a maximal class of mutually equally dogmatic belief functions. Thus, where the consensus does obtain, one may assume that the class is less than maximal, and the more it falls below maximality, the lower the degree of rationality and objectivity the consensus will carry with it.[13]

But neither the lack of a consensus nor the less-than-solid character of the consensus where it does obtain need concern the Bayesian epistemologist. The lack of a consensus may adversely affect the social cohesion of the scientific community. But I believe that with agreement on what I termed a minimal paradigm, scientific communities are capable of much more tolerance of diversity of opinions regarding particular theories than recent philosophies of science have imagined.

7 Conclusion

The philosophy of science is littered with methodologies of science, the best known of which are associated with the names of Popper, Kuhn, Lakatos, and Laudan. In this chapter I have offered a critique of the Kuhnian version, and given the space, I would offer specific complaints about the other versions. But aside from the specifics, I have two common complaints. The first stems from the fact that each of these methodologies seizes upon one or another feature of scientific activity and tries to promote it as the centerpiece of an account of what is distinctive about the scientific enterprise. The result in each case is a picture that accurately mirrors some important facets of science but only at the expense of an overall distortion. The second common complaint is that these philosophers, as well as many of their critics, are engaged in a snark hunt in trying

to find The Methodology of Science. The hunt is fueled by a conflation of three aspects of science and/or by a wrongheaded perspective on one or more of these aspects.

The first and, to my mind, the most interesting aspect is the epistemic one. I insist (in my Bayesian mode) that this aspect be explained in Bayesian terms. This implies that all valid rules of scientific inference must be derived from the probability axioms and the rule of conditionalization. It follows that there is nothing left for the methodologists to do in this area. Another implication is that the methodologists are wasting their time in searching for a demarcation criterion that will draw a bright red line between science and nonscience in terms of the methodology of belief formation and validation, for it is just all Bayesianism through and through, whether the setting is the laboratory or the street. What does demarcate science as it is now practiced is the professionalized character of its quest for well-founded belief.

This brings me to the social/institutional aspect of science, which is responsible for many of the characteristic features of scientific activity. Why, for example, do scientists display the Mertonian virtue of communalism, openly sharing information? Not because they also possess the other Mertonian virtue of disinterestedness and strive selflessly to advance scientific knowledge rather than their own agendas. On the contrary, communalism is explained by coupling the selfish desire for recognition, which obviously does motivate most scientists, with the current institutional arrangement that gives credit for a discovery to the person who first publishes it in a professional journal.[14] Such arrangements are clearly contingent, since the course of history might well have evolved a different set of protocols. And if it had evolved a very different set, science as currently practiced would not exist. Whether the practice that did evolve would deserve to be called science is a nice question that in general will not have a definite answer unless one believes, as I do not, that there are identifiable essences attached to the concept of science. I most certainly do not draw from this line of reasoning the conclusion that because they are contingent, the current social/institutional arrangements of science and the characteristics they foster are not worthy objects of study. But I do caution against trying to use the results of such a study to build an account of The Methodology of Science.

Finally, there are decisions about the tactics and strategies of scientific research, an aspect of science that the methodologists have taken as their main theater of operations. A typical issue here might (with only mild caricature) be posed thus: "My old paradigm has an impressive record of predictive and explanatory success. But lately it has been unable to generate any novel predictions that stand up to experimental test, and it has been unable to resolve several long-standing anomalies. Should I continue to tinker with it in the hope that its fortunes can be revived, or should I switch allegiance to a rival paradigm?" I suggest that this and similar issues should be seen as practical decisions about the allocation of intellectual and economic resources. From this perspective, there is nothing left for the methodologists to do except to repeat, perhaps in disguised form, the advice to choose the action that maximizes expected utility.

In sum, I agree with Feyerabend that there is no Methodology. But my reasons do not stem from an ideology of anarchism or Dadaism; nor do they rely on incommensurability and fellow travelers. A little Bayesianism and a lot of calm reflection are all that is needed.

It might be complained that the picture I have sketched leaves out the interactions among the three aspects of science I have identified. I agree that these interactions generate a number of unresolved problems. I have, for example, tried to highlight in this chapter and the preceding one the curious relationship between the epistemic and social aspects as regards the notion of scientific objectivity. A key component of scientific objectivity is agreement among members of the relevant scientific community. But an objectivity worth having requires an individualism: the consensus must emerge not from social pressures but from an evidence-driven merger of individual opinions operating under Bayesian strictures. The account I have given of the matter is far from complete, and I am unsure about what else is needed to complete the story. But I do not think that Methodology is the answer.

4. If antirealists are to be believed, it can never be had, because theory is underdetermined by observational evidence precisely in the sense that there exist theories not observationally distinguishable from their rivals. For some remarks on this matter, see chapter 6 and section 4 below.

5. In *A Treatise on Probability* Keynes wrote, "The conception of having *some* reason, though not a conclusive one, for certain beliefs, arising out of direct inspection, may prove important to the theory of epistemology. The old metaphysics has been greatly hindered by reason of its having always demanded demonstrative certainty.... When we allow that probable knowledge is, nevertheless, real, a new method of argument can be introduced into metaphysical discussions" (1962, pp. 239–240). It is this "new method of argument" that Russell attempted to clarify in *Human Knowledge: Its Scope and Limits* (1948).

6. I have changed Giere's notation to conform to mine and have included the "initial conditions" in *K*. Again, my purpose here is not to pick on Giere but to criticize what I take to be a vain hope about theory testing. The hope goes back to Keynes and Russell and is still widely shared.

7. I have changed Jeffreys's notation to conform to mine.

8. There are still problems aplenty here (see chapter 6).

9. In fairness to my revered colleague Wes Salmon, I should point out that his proposal was made in the context of trying to understand Kuhn's account of theory choice. However, I would note that Bayesians should not get sucked into playing the game of choosing among theories (see chapter 8).

10. See Earman and Glymour 1980b for an account of the British eclipse expeditions and their role in the early reception of GTR.

11. See Norton 1989 for a detailed account of how Einstein discovered and justified GTR to himself. According to Norton, Einstein's reasoning follows eliminativist lines.

12. For an analysis of Einstein's derivation of the perihelion shift, see Earman and Glymour 1991.

13. See Earman and Glymour 1980a for a review of the early red-shift tests.

14. For more details of Eddington's analysis, see Earman and Glymour 1980b.

15. For a comprehensive review of these and other members of the zoo of gravitational theories, see Will 1981.

16. Other relevant references describing the program include Will 1971a, 1971b, 1972, 1974a, 1974b, 1981, 1984; Ni 1972, 1973; Thorne, Lee, and Lightman 1973.

17. For a definition of this and other terms relevant to Thorne and Will's program, see Thorne, Lee, and Lightman 1973.

18. For example, to explain the influence of gravity on light, either Maxwell's theory or the photon theory may be used.

19. Will defines a test body as one "that has negligible self-gravitational energy (as estimated in Newtonian theory) and is small enough in size so that its coupling to the inhomogeneities of external fields can be ignored" (1984, p. 351).

20. This is a reference frame that falls along one of the geodesics of **g** and is small enough that the inhomogeneities of the gravitational field can be ignored.

21. In recent years there has been some speculation about a "fifth force." The existence of such a force would undermine the weak principle of equivalence, since it is supposed to moderate the gravitational attraction of bodies in a manner that depends upon the chemical composition of the bodies. The evidence is now deemed to be overwhelmingly against such a force. It

would be interesting to see how this judgment can be rationalized in terms of the Bayesian-eliminative scheme proposed here.

22. This figure is adapted from Will 1972, 1974b.

23. Apologies may be due to Sherlock Holmes for using him as a foil. Although many passages suggest that he believed in a crude form of eliminative induction, other passages indicate a more sophisticated view. (Here I am indebted to Prof. Soschichi Uchii.) In *The Sign of Four* Holmes speaks of the "balance of probability," indicating that his method involves probabilistic elements. And even more intriguing, in *The Hound of the Baskervilles* he speaks of "the scientific use of the imagination." With a little charity we can read this as the ability to articulate all of the relevant possibilities. Thus Holmes's success may be seen as the result of carrying out the version of eliminative induction discussed above.

Chapter 8

1. *Structure*, p. 199. This already serves to separate Kuhn from Quine's epistemology and all of its disastrous consequences.

2. For Bayesianism, the emergence of a sharp consensus (in the degree-of-belief sense) from widely differing prior probabilities is at the heart of scientific objectivity (see chapter 6 for a discussion of objectivity and rationality within the Bayesian framework).

3. See Friedman 1983 and Earman 1989 for details.

4. While the use of this language may not be the appropriate device for understanding all of the historical disputes, it does illuminate those having to do with the long-running debate between the absolute versus relational conceptions of space and time (see my 1989 book). There may be some sense in which Newton's theory, translated into space-time language, is no longer Newton's theory. But I deny that there is any interesting sense in which this translation fails to accurately capture the physical content of Newton's theory. Nevertheless, I acknowledge that the adoption of the new language influences probability assignments. I will return to this point in sections 3 and 6.

5. Since I have never been able to understand exactly what is at issue here, I don't know whether I should demur or not.

6. My response to incommensurability and relativism has been tactical. A more radical response is given by Kelly and Glymour (1990), who argue that there are inductive methodologies powerful enough to take into account shifting frameworks, paradigms, and whatnot.

7. At some junctures Giere (1988) distinguishes between choosing to pursue a theory and accepting the theory as true; at other places, however, it is hard to tell what he has in mind. The issues discussed here have played themselves out before in philosophy of science (see, for example, Bar-Hillel 1968, Carnap 1968, and Kyburg 1968).

8. It might be complained that successful puzzle solving in normal science requires a commitment to or an embrace of a theory that goes beyond a practical decision to allocate a portion of one's research time to the theory. This complaint may or may not be correct as a psychological account of the motivation of individual scientists. I think that it is true for some scientists but false for others.

I should also note that there is a sentiment in favor of theory acceptance on the grounds that it is needed to produce scientific explanations (see, for example, Salmon 1968). I must confess that I find this sentiment nearly incomprehensible. Without attending to the truth of a theory *T*, we can check the various conditions of Hempel and Oppenheim's account (or your favorite alternative account) of scientific explanation to make sure that *T* is a potential explanation of the phenomenon under scrutiny, that is, that *T*, if true, would be an explana-

tion. The notion that we should then accept T, or some other rival potential explainer, to turn the potential into an actuality strikes me as being akin to thinking that wishing it were so makes it so. If on the basis of the available evidence the probability of T and each of its rivals is less than 1, as it almost always is, then that's the way things are, and wishing it were otherwise is futile.

Finally, I want to comment on the notion of accepting a theory in the sense of "acting on it." Thus scientists may be said to act on Newton's theory when they use it to calculate the orbit of a lunar rocket probe. But the very same scientists, without any change of cognitive attitudes, may act on Einstein's GTR rather than Newton's theory in calculating planetary perihelia. What we again have are practical decisions whose different outcomes turn on the different utilities assigned to accuracy of prediction and ease of calculation in the two contexts.

9. Which form of revolution was occasioned by the introduction of Einstein's GTR? Arguments can be made in favor of both the weak and strong forms.

10. For details, see Earman and Glymour 1991.

11. Lehrer and Wagner prove various results about the conditions under which iterated weighted aggregation produces a consensual probability; the details are not relevant here.

12. Donald Gillies (1990) has argued for what he calls "intersubjective probabilities" on the grounds that Dutch book can be made against a community whose members have differing degrees of belief. This argument proves too much, since it would entail the unhealthy consequence that the members of a scientific community are not allowed to disagree. The way to escape the conclusion is to deny that a scientific community acts as a corporate or economic body in discharging its basic role of discovering and testing scientific hypotheses. In addition, Gillies's communal Dutch-book construction is not amenable to the more pristine interpretation (recommended in chapter 2) that relegates Dutch bookies to the role of window dressing.

13. That is, unless it could be shown that the class of belief functions has been narrowed for evolutionary reasons that enhance the reliability of the degrees of belief, an option I found unattractive in chapter 6.

14. As I learned from David Hull's lecture "Why Scientists Behave Scientifically," delivered in the Pittsburgh Series in the Philosophy of Science, September 1990. Philip Kitcher pointed out to me that Merton himself offered an explanation along these lines (see Merton 1973).

Chapter 9

1. This is a slight modification of the setup used in chapter 6.

2. See chapter 14 of Rogers 1987.

3. A "positive instance" $I(n)$ of H_D will have the form $Pn \ \& \ \neg Ra_n$ or $\neg Pn \ \& \ Ra_n$. Since evidence statements are assumed to be true and since if Pn is true (false) in any $w \in \text{Mod}_{\mathscr{L}}$, it is true (false) in every $w \in \text{Mod}_{\mathscr{L}}$, it follows that for positive instances we can set $\Pr(Pn \ \& \ \neg Ra_n) = \Pr(\neg Ra_n)$ and $\Pr(\neg Pn \ \& \ Ra_n) = \Pr(Ra_n)$. Thus $\Pr(H_D/\&_{i \leqslant n} I(n)) = \Pr(H_D/\&_{i \leqslant n} I'(n))$, where $I'(n)$ is $\neg Ra_n$ or Ra_n according as $I(n)$ is $Pn \ \& \ \neg Ra_n$ or $\neg Pn \ \& \ Ra_n$. As a result, we can continue to take evidence sequences to be of the form $Ra_1 \ \& \ \neg Ra_2 \ \& \dots$.

4. From note 3 we know that

$$\Pr(H_D/\underset{i \leqslant n}{\&} I(n)) = \Pr(H_D/\underset{i \leqslant n}{\&} I'(n))$$

$$= \frac{\Pr(H_D)}{\Pr(I'(1)) \times \Pr(I'(2)/I'(1)) \times \cdots \times \Pr(I'(n)/\&_{i \leqslant n-1} I'(i))}$$

The results of chapter 4 can now be applied to conclude that if H_D is true and $\Pr(H_D) > 0$, the instance confirmation goes to 1 as the number of positive instances goes to infinity and that if countable additivity holds, the probability of H_D itself goes to 1 in the limit. Recall that in this context, countable additivity means that

$$\Pr((\forall i)\eta(i)) = \lim_{n \to \infty} \Pr(\underset{j \leqslant n}{\&} \eta(j)).$$

5. For results relevant to the PAC (or probable approximate convergence) model of learning, see Blumer, Ehrenfeucht, Haussler, and Warmuth 1987 and Rivest et al. 1989.

6. For more careful and detailed formulations of formal-learning problems, see Osherson, Stob, and Weinstein 1986; Osherson and Weinstein 1989a, 1989b; and Kelly and Glymour 1989. The paradigm adopted here of truth detection in the limit is from Kelly and Glymour 1989.

7. The domain of any $w \in \text{Mod}_{\mathscr{L}}$ is the disjoint union of two parts D_1 and D_2, each of which is countably infinite. Each element of D_1 is named by a numeral, each element of D_2 is named by an a_i, and n-ary arithmetic and empirical predicates are interpreted subsets of the n-fold Cartesian products $D_1 \times D_1 \times \cdots \times D_1$ and $D_2 \times D_2 \times \cdots \times D_2$ respectively.

8. In a different setting, Osherson, Stob, and Weinstein (1988) show that effective Bayesian learners must pay a price.

9. The term 'verifutable' introduced above is due to Kelly and Glymour (1989). The example used here is due to Kelly (1990).

10. To my knowledge, Kevin Kelly was the first to use formal-learning theory to show how Bayesianism contains a concealed dogmatism. I am grateful to him for sharing his work with me prior to its publication.

11. If, say, $K = \text{mod}((\exists i) \neg Pa_i)$, then K is not compact in the evidence, since every finite subset of $\{Pa_1, Pa_2, \dots\}$ is satisfiable in K, even though the entire set is not.

12. Compare this to the usual notion of not finitely verifiable/falsifiable obtained by negating the condition that ψ is *finitely verifiable* (respectively, *falsifiable*) relative to K just in case for any $w \in K$ such that $w \in \text{mod}(\psi)$ (respectively, $w \in \text{mod}(\neg \psi)$), there is a finite E such that $w \in \text{mod}(E)$ and such that for any $w' \in K$, if $w' \in \text{mod}(E)$, then $w' \in \text{mod}(\psi)$ (respectively, $w' \in \text{mod}(\neg \psi)$).

13. In the general case where $K = \bigcup_i \text{mod}(\Delta_i)$, the Bayesian agent is probabilistically certain at the outset that the actual world models one of the Δ_i, and that relative to any such Δ_i, ψ is equivalent to a verifutable sentence.

14. For a sampling of recent opinions on the reliabilist conception of knowledge, see Alston 1986, Bonjour 1985, Foley 1985, and Goldman 1986.

15. The formal learner may be unmoved by this line of argument in view of the fact that, short of the infinite limit, the Bayesians have not been able to give any objective significance to the little numbers they assign (see chapter 6).

16. See Earman 1986 and Penrose 1989 for suggestions along this line.

17. This seems to be what Putnam has in mind when he says that somebody proposes the hypothesis.

18. See Carnap's 1963b response to Putnam.

Chapter 10

1. Here Sue seems to be referring to van Fraassen's objection, discussed in section 3 of chapter 5 of *Bayes or Bust?*