This chapter explores the point where the epistemology of science and the epistemology of history meet, namely, the evidence base which allows the historian to attribute knowledge states to a scientific community. A key presupposition of this inquiry is that the central problems in rationalizing our epistemic history do not concern changes in belief, but changes in orthodoxy. It is one thing to ask what each member of a community should believe for himself: an essentially Cartesian question. It is quite another thing to ask what the members should take to be the dominant beliefs of their community. This second question considers the relative burden of proof that their beliefs should bear. Moreover, it is not obvious that the answer to the first question constrains the answer to the second question in any significant way. Even if the vast majority of members of the community hold a certain belief, that is no indication of whether they would allow it to pass in open forum without strenuous argument. On various long-term methodological or ideological grounds, the members may have an interest in keeping certain of their widely held beliefs from gaining general social currency (that is, from acting as licenses or warrants in their language games). Consider this vivid case: for all we know, a majority of scientists may still believe in God, yet the belief must bear an enormous—perhaps insurmountable—burden of proof before it can be used to justify a scientific claim.

Another such case may be the beliefs that scientists (and humanists as well) have of one another's "real reasons" for conducting certain lines of research. Sociologists of science have no doubt accurately captured the "interest-laden" nature of these reasons. But given the scientific community's overriding interest in the long-term maintenance of our knowledge enterprises, it has erected many probative barriers that prevent the sociologists' findings from gaining general credence. Not the least of these barriers are alternative, more orthodox explanations for the seemingly "interest-laden" behavior. Indeed, if Jon Elster (1979, ch. 2) is correct, a key reason why the orthodox view does not necessarily coincide with the majority view is that by distinguishing the orthodoxy from the majority, a community is afforded an indirect means for changing its undesirable yet widespread beliefs, namely, by making them so difficult to express (since they must bear such a large burden of proof) that the members of the community are effectively discouraged from holding the beliefs (Fuller 1985a, ch. 2). Thus, through adequate training, scientists may come to believe that there is a sharp distinction between theoretical (interest-free) and practical (interest-laden) reasoning. And so, we see that although answers to questions of personal belief may not resolve questions of orthodoxy, answers to questions of orthodoxy may be used to resolve (at
least indirectly) questions of personal belief.

However, these last two paragraphs have been something of a teaser, since I do not intend to discuss the interesting normative issues they suggest about epistemic change. My task here is the propaedeutic one of showing how the epistemic historian goes about identifying orthodoxies, or presumed truths, and beliefs that bear varying burdens of proof. This task is set against the backdrop of debates in the philosophy of science, my purpose being to highlight the inadequacies had by all sides.

We start by considering Feyerabend's attacks on positivism and conclude that his own version of realism lacks the key virtue of positivism, namely, a well-developed theory of "verification" or evidence. This problem becomes especially acute in the case of incommensurability, which is defined here in terms of two theories having different verifiability conditions but the same truth conditions (so as not to get into any needless debates with the scientific realist). On our account, two theories have different verifiability conditions when they do not bear the same burden of proof. We claim that our account of verifiability conditions, based on the socio-historical variable of burden of proof, renders the history of science more intelligible than other accounts. Moreover, rendering the history of science intelligible is taken to be an essential condition for any adequate theory of science to meet. We thus adopt a strategy of Minimal Hegelianism, whereby the presence of a sufficient reason is presumed for whatever happens in history. In this chapter, language is treated as the something that "happens" (a performance), which then allows us to draw on Grice's Quantity Maxim and Quentin Skinner's historical methodology to fashion a principle for judging burden of proof: namely, the historical figures who say more (in defense of their claims) needed to say more, since they bore a greater burden of proof for their claims. Minimal Hegelianism is then used to interpret Kuhn's distinction between the tacit knowledge of paradigms and articulated methodological rules. In the end, we draw on resources from the positivist theory of law to bear out Kuhn's claim that paradigms cannot be reduced to rules.

1. Feyerabend and the Problem of "Rival Yet Incommensurable" Theories

In his 1951 doctoral thesis, "An Attempt at a Realistic Interpretation of Experience," Paul Feyerabend (1981a, ch. 2) set out for the first time the metaphysical strategy that, within a decade, would mark him as the most potent philosophical foe that logical positivism has ever had. Feyerabend considered two paradigms: positivism and realism. We shall begin by laying out these paradigms, partly following Feyerabend's argument and partly expanding its scope to incorporate more recent developments in the positivism/realism debate.

According to positivism's proponents, Russell and Carnap, observation
sentences derive their meaning from one-to-one correspondences with atomic sensations or combinations of such sensations. Feyerabend called this the Stability Thesis, presumably after the positivist tenet that meaning was possible only because, at the most basic level of observation, words had some stable attachment—call it "correspondence"—to the world. As subsequent papers showed, Feyerabend took this thesis as the source of the positivist claim that scientific theories may change without the meanings of theoretical terms undergoing a change. This was the notorious Meaning-Invariance Thesis that allowed for the comparison of scientific theories and, thus, for judgments about their relative merits. Moreover, on the positivist view, the set of sentences constituting a theory is nothing more than an economical means for inferring observation sentences.

The last point about positivism may be taken in two ways (not distinguished by Feyerabend). On the one hand, it may imply that theory adds neither to the empirical content claimed of the world nor to the meaning of the observation sentences. The "nor" follows from the "neither" by the positivist "criterion of cognitive significance," namely, that a sentence's meaningfulness is exhaustively defined in terms of its empirical content. On the other hand, the positivist stance on the role of theory in science may be taken to imply that the empirical content of a theory exceeds that of its observation sentences, if the theoretical sentences are understood as making use of terms denoting, say, middle-sized objects, which are constructed from infinitely many atomic sensations. But in that case, theoretical sentences are, strictly speaking, undecidable, since we can never determine the total evidence for the presence of one such object. On the first reading, positivism is a species of "antirealism" in Bas van Fraassen's (1980) sense, which is informed by scientific practice; on the second reading, positivism is "antirealism" in Michael Dummett's (1976) sense, which is informed by ordinary linguistic practice. Both readings, however, share the view that whatever hidden causal mechanisms seem to be invoked in theoretical sentences can be justifiably regarded as no more than heuristic fictions.

According to realism, the position taken by Feyerabend himself, someone may be caused to utter an observation sentence whenever he has a particular sensation without that sensation conferring meaning on the sentence. The distinction between the causal and semantic sources of utterances is blurred in the positivist use of sensations as its metaphysical starting point, which, in effect, conflates third and first person perspectives on an individual's behavior. The behavioral scientist can correlate the speed with which a subject linguistically responds to the exposure of a physical stimulus. The subject himself immediately identifies his sensation of the stimulus as a meaningful object. Contrary to the connotations of "sensations," "speed" cannot be equated with "immediacy," for the speed of response stems from the conditioning history of the subject, while the immediacy of identification stems for the observation sentence being derivable from a comprehensive theory that defines the sensation as evidence for some higher-
order entity or process. Feyerabend suggests that the positivist conflation may arise from a subject possibly being so well conditioned to respond within one theoretical language that he cannot conceive of the sensation as evidence for some other entity. However, this inconceivability should not be confused with the supposed *infallibility* of the observation sentence, for, once again, since the sentence derives its meaning from a theory which posits entities, if those entities turn out not to exist, then the sentence may turn out to be false.

Feyerabend's arguments were originally aimed at Niels Bohr, who claimed that it was "psychologically impossible" for a physicist to understand the world as clearly through quantum mechanics as through classical mechanics. Classical mechanics is, so to speak, the "ordinary language" of physics. Bohr is interesting because, while he believes that the meanings of sensations are conferred by a subject's entire theoretical language, he also believes that only *one* such language is uniquely isomorphic with the totality of our objects of perception—namely, the world of middle-sized objects of classical mechanics. Feyerabend, thus, sees in Bohr a veiled endorsement of the Stability Thesis. In contrast, the realist treats the entities posited by a theory as the putative causes of a subject's linguistic behavior, such that the behavioral scientist and the subject may be regarded as offering rival theories of the same "phenomena" (which, in turn, will be described in the language of the theory that turns out to be true). This suggests that a realist theory of reference would be identical with the latest physical theory, or at least the best available account of the causes of linguistic behavior, regardless of whether most actual language users know (the best theory of) what their observation sentences are true of. Again in contrast, a positivist theory of reference would group together observation sentences uttered by individuals in different languages that were triggered by sensations of the same type. It would thus depend crucially on the possibility of intersubjective agreement among language users.

Although he has shown the advantages of a realist over a positivist paradigm of experience for doing the kinds of activities (such as searching for hidden causal mechanisms) that normally fall under the rubric of "science," Feyerabend nevertheless realizes that his own account of meaning, which relativizes meaning to particular theories, obscures, if not renders incoherent, the sense in which two theories may be regarded as offering rival interpretations of "the same phenomena." For even in 1951, he had already renounced the possibility of a crucial experiment deciding between them. Yet the cogency of realism as a genuine alternative to positivism rests on making sense of the concept of *rival yet incommensurable* theories. As we shall see, this problem is hardly a stranger to positivism.

One of the more unsavory features of positivism has been precisely its inability to provide sufficient reason for the theory choices that have occurred in the history of science, let alone why theoretical issues should appear more significant than empirical ones. On the sort of antirealism espoused by van Fraassen, there is nothing to choose from, since theoretical
languages are merely notational variants for expressing the same empirical content. At best, then, the prominent disputes in science turn out to be arguments about heuristics: to be a Newtonian or an Aristotelian is simply a matter of how one's scientific research is to be organized. At worst, scientific theory disputes become exercises in the emotive uses of language or, more charitably, "ideological conflicts": to be a Newtonian or an Aristotelian is simply a matter of identifying the interest group of which one is a member. On the sort of antirealism espoused by Dummett, there may be a real choice, but never adequate warrant for making it, since, short of total evidence, legitimate doubt may remain about whether or not the putative object of some theoretical term really exists. This, of course, is one of the many guises under which the problem of induction has traveled. And, as Nelson Goodman's (1955) "grue paradox" has shown, the scientist's predilection for theorizing with one of two terms that save the phenomena equally well will depend on which term happens to be better integrated with other terms in the scientist's language. Ironically, throughout his career, Feyerabend has resorted to these positivist maneuvers to account for both the apparent groundedness of established theories and the real ungroundedness of new theories. Indeed, he may easily be taken to have endorsed the following argument:

(P1) If theory choice can be explained by positivist maneuvers,

(P2) And theory choice (as opposed to, say, data gathering) is the most significant feature of science,

(C) Then science does not live up to its rational self-image.

However, nothing said so far forces us to accept (P1).

Larry Laudan (1977), for one, has tried to get around the "rival yet incommensurable" problem by denying that two theories must be commensurable in order to be rivals. The general strategy is to decide in favor of the research tradition that has solved more of the problems it has set for itself. However, this strategy is less helpful than it first seems, since it is not clear how such decision would be made. Assuming that the decider can individuate problems within each research tradition, he must then weigh and tally them, not unlike a utilitarian calculus. But it is difficult to imagine the mechanisms of such a weighting without supposing that the research traditions are much more commensurable than Laudan would have them be. Indeed, Bentham's own calculus was motivated by the idea that qualitatively different goods were reducible to the net magnitudes of pleasure and pain that they gave the same result to either the decider or those for whom the decision was binding. Thus, we can conclude that at least Laudan's attempt to sever rivalry from commensurability will not do. Notice that we have so far supposed, as have most philosophers of science, that incommensurability is an obstacle to any possible rivalry between two
theories. Laudan's strategy was to circumvent the obstacle. But perhaps incommensurability is *integral* to understanding the sense in which two theories may be rivals. However, there are several senses of incommensurability. We shall first look at a radical sense of incommensurability, to which we would normally be attracted, but which is too radical for our current purposes.

One strain in the positivist tradition traces all seemingly significant disagreements—namely, those over higher-order concepts like truth, goodness, and beauty—to mutual misunderstandings by the disputants. Let us call this the Babel Thesis. The Babel Thesis appears in the view that two theories of equal empirical adequacy are merely notational variants, as well as in the Quine-Davidson position that optimal translation makes most of the sentences in the translated language come out true (Follesdal 1975). The idea is that given the same evidence, and the same background knowledge, all rational individuals would license the same inferences. And so, if two disputants agree on the premises but not the conclusion, then this disagreement may be attributed to some misunderstanding: perhaps one party erroneously presumed that the other party had the same background knowledge, or perhaps the other party's conclusions have simply been mistranslated. Applied to science, the Babel Thesis implies that the only epistemic reason why it appears that choices between theories must be made is that the proponents of the two theories do not quite understand what each other is claiming and/or about what they are claiming it; hence, rivalry implies incommensurability in a rather strong sense. Our only real epistemic duties, then, are to effect a translation that maximizes consensus where the two proponents are making claims about the same domain of objects and to specify where the two proponents are making claims about different domains of objects.

Notice that the Babel Thesis does not deny that there are good social reasons necessitating theory choice: perhaps resources are limited, and the language of one theory (especially its metaphors) suggests applications that the other does not, even though there is no difference in empirical content. The implications of the Babel Thesis are far-reaching. For if the very need for theory choice is "social" rather than "epistemic" (in the way that a classical philosopher is likely to draw the distinction), then there is something to the thesis, advanced by both Feyerabend and Foucault, that to believe that established scientific theories are entities "commanding" our assent and "constraining" our assertions about the world is to mix the epistemic and social import of those theories. However tantalizing it would be to explore this implication here, the reader must turn to the next chapters. For present purposes, the Babel Thesis does not offer the right account of "rival yet incommensurable," since the reading of the history of science that it presupposes raises too many foundational questions.
2. The Missing Link: Burden of Proof

Instead, our account of "rival yet incommensurable" turns on the idea that two theories are incommensurable because they do not bear the same burden of proof. Admittedly, considerable normative force is attached to the idea that two theories bear the burden of proof equally, as in the case of a crucial experiment, whereby the same outcome is said to verify one theory and falsify another. This approach to burden of proof suited the positivists, who generally conceived of theories as closed logical structures. As a result, they have been unable to make sense of the intuition that evidence bears only on parts of theories, and on different sized parts of different theories (Glymour 1980). However, in order to give burden of proof a more "realistic" flavor, followers of Karl Popper have refined the idea of crucial experiment so that the outcome of such an experiment need not bear on each of the theories equally: one theory may have a high-level principle falsified, while the other may, at the same time, have only a low-level theorem corroborated. Imre Lakatos (1970) introduced the idea of "negative heuristic" in order to determine just what aspects of a theory may be tested by what pieces of evidence. And Doppelt (1982) has even attempted a partial explication of Thomas Kuhn's concept of incommensurability in terms of two theories having widely divergent senses of the relative significance had by particular pieces of evidence.

Nevertheless, to say that the evidence does not bear on two theories equally is not to say that the two theories do not bear the burden of proof equally. In order to identify the condition necessary for the two theories not bearing the same burden of proof, imagine that one predicts that some event will happen (0) and the other predicts that the event will not happen (-0). The burden of proof would be different, then, only if the kind of evidence that is adequate for showing the truth of 0 is not also adequate for showing that -0 is false. A typical case would involve proponents of one theory needing only to assert "0" as evidence for 0 because the truth of 0 is so well entrenched that mere assertion commands assent, while advocates of an opposing theory need to go through a great many arguments and experiments in order to persuade the scientific community that -0 is really the case: mere assertion of "-0" may command little more than incredulous looks.

The role played by the kind of evidence has been underscored because in discussions of the possibility of crucial experiment, and how evidence bears on theories in general, "evidence" is usually taken to be sentences describing the outcome of some empirical inquiry. Until Hacking's (1983) recent work on experiment, little attention had been paid to the kinds of procedures used for generating this evidence. In fact, it may even be suspected that much of the importance that Lakatos and Laudan attach to empirical adequacy and theoretical promise as principles of rational theory choice would be diminished if a clear distinction were made between the sheer quantity of evidence (that is, the number of verified claims) possessed by two theories
and the quality of evidence (that is, the kinds of procedures for verifying claims) demanded of the two theories. In that case, the Lakatosian dictum "All theories are born refuted" may be replaced by all theories are born plaintiffs as the problem for which an account of theory change in science would be the solution.

No doubt, the impoverished sense of evidence normally found in the philosophy of science can be traced to the lingering effects of positivism, especially its historical role in explicating "the conditions of verifiability" for an observation sentence, the unit of evidence. In brief, the strategy has been as follows. The truth conditions of an observation sentence are defined by the Tarski Convention; hence, "0" is true if and only if O. O's verifiability conditions are, in turn, defined as a reduction of O to the set of atomic sensations warranting the assertion "0" (for more, see Hacking 1975a, ch. 12). Notice that verifiability conditions are defined not as a procedure for generating O but merely as the appearance of O that licenses the assertion "O." This explication of the conditions of verifiability goes little beyond the Stability Thesis. Indeed, by identifying verifiability with a simple phenomenal analysis of truth conditions, the positivists did not distinguish the two conditions sharply enough for thinking about one independently of the other. This is a major point for us, since, in saying that two theories bear unequal burdens of proof, we want to draw the following distinction that would be difficult for the positivist to express:

(a) Given two theories, one of which entails O and the other -O, if O is true, then ipso facto -O is false.

(b) Given the same two theories, if E is the kind of evidence that would verify O, then E is not necessarily the kind of evidence that would verify -O.

Whereas (a) claims that the two theories have truth conditions for their respective observation sentences, (b) claims that the two sentences do not necessarily have common verifiability conditions, since one theory may bear the burden of proof and thereby require more elaborate procedures for establishing the truth of its claims than the other, whose claims are (for the time being at least) presumed true. The positivist would deny (b) because the kinds of evidence required to verify O and -O are the same, namely, the observation of whichever happens to be the case. Notice also that (a) and (b) contribute to saving the appearances of Feyerabend's realism, in that the independence of (a) from (b) secures the minimal condition of realism: the truth-value of a sentence obtains irrespective of the means by which the value is determined. Furthermore, (b) simply states that the two theories bear unequal burdens of proof, our gloss on the concept of incommensurability. Finally, (a) shows that at least partial translatability is allowed between the two theories, so that we do not depend on the Babel Thesis.
As soon as it became clear that no theory could be verified in the positivist sense of verifiability, the possibility was opened for the concept of burden of proof to be articulated in one of two ways. On the one hand, the concept could be articulated in terms of the degree to which a given claim has been confirmed or corroborated, such that, if the observational consequences of one theory are more confirmed than another, then the more confirmed theory is presumed true and the burden of proof is placed on the less confirmed theory to make its case. On the other hand, the articulation could be in terms of the number of problems solved or phenomena saved, such that the burden of proof is placed on the theory that accounts for less. However, by focusing on what we previously called "the quantity of evidence," positivists have had notorious difficulties in converting their definitions of verifiability (suitably amended) into procedures for reading the history of science. If verifiability is defined as degree of corroboration, how are such degrees to be identified for particular claims made in history, which, at least in the more interesting cases, do not advertise their statistical status on their surface structures?

But is this a fair criticism of positivism? That depends on whether we can provide an account of verifiability—especially that aspect of the concept concerning how two theories bear the burden of proof—from which "instructions" can be derived for reading the history of science. As successive editions of A.J. Ayer's (1952, ch. 8) *Language, Truth, and Logic* amply illustrate, verifiability has been developed more with an eye toward satisfying certain logical demands than historical ones. Thus, we should not be surprised if an adequate definition of verifiability turns out to have no obvious bearing on how history is read. And perhaps this is how it should be, especially if one takes the Popperian line that the philosophy of science stands in relation to science just as ethics does to human action in general (Popper 1981). In that case, since the philosophy of science is an exclusively normative discipline, it is under no obligation to develop concepts that make the phenomena of the history of science easier to save; for it might well be that science, like human action in general, has been exemplary only during sporadic "revolutionary" episodes.

If philosophy of science is not to become indistinguishable from mere descriptive history, the only strategy other than Popper's would seem to involve showing that the history of science is inherently norm-governed. He would try to vindicate Hegel's maxim: "the real is rational and the rational is real." More to the point: the more history that comes out rational by one's philosophical account, the closer one has come to discovering the identity of the governing norm (or set of norms). Thus, in principle, greater knowledge of history begets better normative judgments. We have introduced these global concerns in order to point out that, insofar as we are interested in defining one aspect of verifiability—burden of proof—as a philosophical concept that implies a method for reading the history of science (rather than as a philosophical concept against which the history of science is then judged), we are implicitly endorsing a Hegelian rather than a
Popperian approach to the normative status, or "rationality," of history. This is, to say the least, a controversial move (one, however, which echoes the move toward "panglossian epistemology" in ch. 1). And while I do not intend to show that Hegel should be endorsed over Popper, I do intend to show that Hegel is at least as worthy of endorsement. Interestingly, given the generally ecumenical spirit of our times, it has become necessary to make my point by showing that Hegel is something other than Popper dressed up for a nineteenth-century audience. In other words, we must address the question: Is there a genuine difference between the Popperian who argues that most of history does not conform to his norms and the Hegelian who argues that all of history conforms to his?

One reason why we might at first suspect that there is no real difference is that the Hegelian sense of history, the "world-historic," excludes most of what would otherwise be considered historical phenomena—much in the manner of a normative account of historical rationality. Among contemporary philosophers of science, Ian Hacking (1981b) has most forcefully pressed for this sort of similarity between Popperian and Hegelian sense of historical rationality. At the outset, Hacking admits that the differences between Hegel and the Popperian Lakatos stand out. On the one hand, Lakatos held that judgments of an agent's or an act's rationality were essentially retrospective, which implies that history is rational only in the sense that a modern can claim that he would have done what some historical agent did, had he been in the same situation, given the agent's set of beliefs and interests. On the other hand, Hegel believed that, in some objective sense, history itself was governed by sufficient reason. Hacking argues, however, that Lakatos was forced to adopt a retrospectivist approach to rationality because his paradigm of rationality, the hypothetico-deductivist methodology of the positivists, constituted a style of reasoning alien to the styles in which most scientific activity has been conducted. Consequently, Lakatos found it difficult to identify instances of rationality in history and had to resort to "rationally reconstructing" past episodes. Hacking, nevertheless, endorses these historiographical maneuvers because they readily support his pet thesis that any attempt to show the rationality of history will issue either in massive omissions of what actually happened (so as to preserve the rationality of the account) or in many judgments to the effect that various historical agents did not act in an optimally rational fashion (simply because we cannot easily "make sense" of what they did). Hacking believes that Hegel took the way of the "either," while Lakatos took the way of the "or." Admittedly, Hacking's argument is grounded in a Foucauldian premise, namely, that any account of historical rationality constitutes a "rationalization" (in Freud's sense) and hence a falsification of history. But whatever one makes of the cogency of this premise, the argument still poses a challenge that deserves to be met.

Let us start by returning to the Hegelian claim that there is sufficient reason for whatever happens in history, such that the philosopher does not so much impose norms as discover them. If we treat the utterances of historical
agents as situated actions (as, say, a speech-act theorist or a Marxist would) rather than as verbal icons of the external world (as a positivist and most others would), then the Hegelian claim can be translated into Paul Grice's (1975) Maxim of Quantity for Conversational Implicature. Since this conclusion must appear to be drawing a rabbit out of a hat, the link should be made more explicit.

Grice's project is to articulate what "speakers" (understood most generally to include any language users) must presuppose in order for communication to be a rationally grounded activity. His strategy is to identify heuristics that a speaker uses to supply the background against which his interlocutor's utterances are to be understood. One of Grice's four main heuristics, or "maxims," is Quantity: *everything that is said needs to be said.* In conversation, this maxim works by each speaker presuming of the other that everything they say is necessary for mutual understanding to occur. One says neither too much nor too little. The same can be applied to the historical record, so that whenever it seems that argumentative points are being belabored by an historical agent, the historian is instructed to presume that the figure did not take for granted that his intended audience would understand what the historian now finds so obvious. Notice, therefore, that the Quantity Maxim is presumed to operate between the author and his intended audience, but not necessarily between the author and the historian. It also follows that there may be points that the author need never articulate, since he and his audience take them for granted, even though without having them made explicit the historian runs the risk of seriously misunderstanding the author.

To draw out the consequences of this line of reasoning would lead us back to the Babel Thesis. But in order to apply the Gricean account to the history of science, we shall limit our horizons. Our model will be Quentin Skinner's (1969, 1970) appropriation of J.L. Austin's (1962) speech-act theory, which was first introduced in chapter three. In particular, we shall restrict the concept of "understanding" to the audience being sufficiently informed by the speaker that it can decide whether his claim is true or false; that is, we are accepting a verifiabilist account of meaning. In that case, we can speak of the *threshold of decidability* for a claim as the extent to which a speaker must inform his audience before it can make a decision: How explicit must he be? What follows is a tentative ordering of thresholds from claims that are either presumed true or presumed false until otherwise shown (A) to those that place the entire burden of proof on the speaker to reverse the presumed truth-value of the claim (E):

**Question**—What must an author/speaker do so that his intended audience can determine the truth-value of his claim?

(A) Mere assertion of the claim is sufficient for immediate acceptance or rejection. Indeed, if the audience hesitated over accepting or rejecting the claim for no stated reason, then the audience's competence would be justifiably thrown into doubt. This may be regarded as the
speech-act version of so-called analytic truths and synthetic truisms. Furthermore, (A)-type claims may be so "self-evident" that they remain unsaid as the speaker addresses his audience; hence, they are the ones most likely to elude the historian and distort his understanding.

(B) An assertion of the claim must be accompanied by explication or verbal argument, for while the claim may be inferred from those already accepted by the audience, it is an inference not often (or perhaps ever) drawn.

(C) An assertion of the claim must be accompanied by a "loose" statement of evidence, that is, associated considerations (for example, circumstantial evidence, analogous decidable claims) which by themselves do not demonstrate the truth-value of the claim, but nevertheless provide enough information so that the audience, given the claims it already takes as decidable, will be able to decide this one as well.

(D) An assertion of the claim must be accompanied by a "strict" statement of evidence, that is, a procedure for demonstrating the truth-value of the claim that uses "techniques" (in the broad sense, to include unaided perception and mental computation, as well as the more obvious cases) accepted as reliably representing reality (if empirical) and/or preserving information across operations (if logico-mathematical).

(E) An assertion of the claim must be accompanied not only by a strict statement of evidence, as in (D), but also by an account of why the audience, without having undergone the stipulated procedure, might think that the claim has the opposite truth-value from the one demonstrated.

The methodological upshot of what may be seen as our Minimal Hegelianism is that the history of science is a tale of burdens of proof shifting from a claim to one of its many possible denials, with incommensurability arising when the threshold of decidability for some claim O is at (A) and the threshold for -O is at (E). Roughly, incommensurability lessens as the thresholds for O and -O draw closer together, with the two claims being commensurable only if both have (C) as their threshold and thus bear the burden of proof equally. Furthermore, the thresholds may be adjacent, as in the case of (A) for O and (B) for -O, which would arise if someone who speaks for -O could show that what the audience takes as a natural consequence of its body of accepted claims, O, in fact does not logically follow those claims. This is the sort of position in which the ordinary language philosopher finds himself whenever he claims to have "discovered" a mistake in usage. No claim is ever definitively shown
to be false, but rather, the burden of proof it must bear becomes so overwhelming (an extreme case of [E]) that no one is inclined to take up the challenge in its defense. And, in accordance with the Quantity Maxim, the claim falls into the oblivion of tacit rejection (that is, [A]). Not surprisingly, then, a "revolution" occurs when someone successfully bears the burden of proof of a claim whose threshold is (E), and so, a "paradigm switch" would amount to a reversal of the burden of proof for a large body of claims.

3. Burden of Proof as Tacit Knowledge: Rule-Governedness

Although we have just borrowed liberally from the Kuhnian lexicon, points of contact have yet to be made with the account of scientific change given in Kuhn (1970a). First, a "paradigm" may be identified with those claims whose threshold of decidability is (A). However, they do not constitute all the claims made by the members of a scientific community, since many of their claims may have decidability thresholds at (C). And in the case of "anomalies," the two incompatible claims accounting for the anomaly would have their thresholds at (C), making it impossible to decide between them. Notice that even though two such claims would be "commensurable" in our sense because they bear an equal burden of proof, they could also be symptomatic of another sense of Kuhn's "incommensurability," namely, the one normally described as Quine's (1960) "underdetermination of theory by data." On our account, Kuhn's thesis about scientific revolutions states that once enough claims and their denials have (C) as their threshold of decidability, claims whose threshold is (A) will be successfully denied by claims whose threshold is (E). For our purposes, the historical validity of this thesis is not so much of interest as the reading of the history of science that would be required for testing the thesis. This is not an idle consideration. For if Kuhn's (1977b) own replies to his critics are to be believed, a great deal of the misunderstanding that philosophers have had of his thesis stems from a profound misunderstanding of how the history of science works. Admittedly, Kuhn has never plumbed these depths to anyone's satisfaction, and it may be thought that his remarks are merely self-serving. However, read charitably, the issue seems to center on chapter five of The Structure of Scientific Revolutions, where Kuhn maintains "the priority of paradigms" over methodological rules in reading the history of science. An interpretation of this assertion will now be offered that turns it into an extension of our own Minimal Hegelianism.

In chapter five, Kuhn most closely associates "paradigm" with "tacit knowledge," the key features of which are captured by a scientific community's claims whose threshold of decidability is (A). Since such knowledge is "self-evident" or "natural" to the scientists, it will probably go unarticulated, as the Quantity Maxim would have it, and thereby prove elusive to the historian. This much has already been admitted. But Kuhn
seems to make a stronger claim about tacit knowledge that compounds the historian's difficulties: tacit knowledge is never articulated, that is to say, not even to novice scientists. A novice is said to learn the trade exclusively through the negative feedback of instructors once a textbook example has been misapplied to a new case. Kuhn is rather careful not to say that the novice learns a certain bodies of beliefs; instead, he learns a rough-and-ready way of talking about what he is doing which consists of appeals to the similarity between his actions and the ones prescribed, again, by textbook examples. Kuhn even seems to suggest that a scientific community has no way of maintaining that its members hold similar beliefs about what they are doing, only that they "pass" as competent performers—whether it be in the lab or at a conference. And so, Kuhn concludes that while it should be possible to determine the paradigm of a scientific community from examining the historical record, there is no reason to think that any assertion concerning a communal commitment (say, to a particular methodology) made by a member of that community will capture it. In fact, such assertions tend to be made only once the self-evident nature of the paradigm has been called into question.

Kuhn's claim may be initially understood as a reassertion of the Hegelian over the Popperian line on historical rationality. If the methodological rules uttered by a member of the scientific community could capture the subtle nature of the paradigm, then much of the historical record would be rendered superfluous. Philosophers of science could simply take what their favorite scientist had to say about scientific practice as an adequate synopsis of that practice, without studying how the scientific community actually did their work. Hacking showed that philosophers in fact do just that. However, in so doing, they do not carefully distinguish the intended application of methodological rules (a theory of optimal scientific activity) from their illocutionary force (the activity performed in uttering the rules). Unless the historical record overtly contradicts the methodological pronouncements, the philosopher is inclined to take them as intended—that is, not as actions on par with other aspects of scientific practice, but as privileged representations of that practice which allow the philosopher to safely ignore those other aspects. (Recall our critique of "transcendentalist" approaches to representation in ch. 2.) Kuhn's point would then be that the illocutionary force of asserting communal commitments is to indicate that the scientific community is entering a period of dissensus over such commitments—a point that would be easily missed on the Popperian approach to historical rationality. In contrast, such a point would vindicate the Hegelian approach, since it would go to show that methodology does not merely recapitulate practice but has a distinct function of its own—that is, there is a sufficient reason for its being uttered.

At this point, it would be instructive to integrate the discussion of Kuhn with the issues raised about Feyerabend at the beginning of the paper. At that time, we pointed out that if realism is to be regarded as a genuine alternative to positivism, it must overcome positivism's inability to provide
sufficient reason for theory choices that have occurred in the history of science. We then saw that Feyerabend failed to meet the challenge. However, our reading of Kuhn may offer aid to the realist. First, as someone interested more in the performative than in the representational nature of language, Kuhn would claim that all theory disputes amount to methodological disputes over how and/or whether certain concepts apply. Next, as a Minimal Hegelian who discovers "reason in history" (Hegel 1964), he would presume that there is a deep paradigmatic structure to the history of a scientific community that cannot be simply inferred from the surface utterance of methodological rules. Furthermore, just as the positivist maintained that theories are heuristic fictions, Popperians, we have seen, make similar claims about accounts of historical rationality—which Kuhn, of course, implicitly denies. Indeed, Kuhn's (again implicit) critique of the Popperian approach to historical rationality parallels the one originally made by Feyerabend against the positivist concept of experience. Whereas the positivist conflated the situation that gave rise to an individual uttering an observation sentence (which could be determined by some third party, such as a behavioral scientist) with the meaning he conferred on that sentence (which would be determined by the theoretical framework of the observer himself), the Popperian is taken to have conflated the historical situation that gave rise to the utterance of methodological rules (namely, communal disensus) with the interpretation that the utterer would have his audience attach to those rules (namely, that this is how science is and should be practiced). However, having said this much, we have yet to indicate wherein lies the difference between tacit knowledge and articulated methodology.

Given our previous analysis of the Quantity Maxim into thresholds of decidability, it follows that methodological rules do not merely state what was obvious to the scientific community of the time. Were the rules so obvious, they would not need to be stated; they would simply be shown in practice. On the contrary, the assertion of methodological rules is warranted only when there is sufficient doubt about the regulation of scientific practice. From these considerations it might be concluded that methodology is nothing but ideology. In other words, scientists start talking about their practice only as a means of deferring attention from what they are really doing, which is much more complicated than a neat set of rules would indicate. Furthermore, it would be practically impossible to enforce such rules, even if they constituted an adequate description of the best science, since scientists practice their trade in diverse locales. How would one then be able to control the judgment calls on what counts as a correct application of a particular concept or a correct extension of a particular theory? However, by uttering methodological rules, scientists aim to persuade their colleagues to read the history of their discipline in a way that allows them to evaluate previous practice, supposedly (given the unlikelihood that such methods could be rigorously enforced) with an eye toward subsequent practice. In effect, the perlocutionary force (that is, the practical import) of methodological utterances is to make scientists Popperian historical
rationalists. Not surprisingly, then, Popper favors episodes of "crisis" in the history of science precisely because only then are scientists licensed to make the kinds of judgments—one always being made by philosophers of science—that he thinks are most appropriate for bringing out the rationality of science.

However compelling this conclusion may be, it still would not satisfy someone who believes that there is a point at which ideology ends and knowledge begins. Even though Kuhn may in fact be right that scientists have given inadequate accounts of the principles governing their practice because they are really trying to serve some other purpose by asserting such principles, that fact in itself does not preclude the possibility of a reflective scientist someday articulating an adequate account of the principles governing his practice. Kuhn offers no reasons for thinking that tacit knowledge must remain tacit forever, or else be distorted in articulation. However, Kuhn does seem to want to make this strong claim. One way to make good on it would be to show that the tacit knowledge of paradigms is different in kind from the methodological rules articulated by scientists. That is to say, the two do not "govern" scientific activity in the same sense, and so one cannot be reduced to the other. This somewhat cryptic strategy may be illuminated by discussing the two kinds of norms we have in mind.

If we take at face value Kuhn's account of how novices learn the scientific trade, largely through negative feedback (which is also how colleagues keep checks on one another's work), then the application of concepts to new data and the extension of theories to new domains would seem to be definable only in terms of what the philosopher of law Herbert Hart (1948; Baker 1977) has called defeasibility conditions. Hart's main example is a situation in which a judge must decide whether a contract has really existed between the two litigants. As in Anglo-American legal matters generally, the burden of proof is on the party who wants to deny the presumed state of affairs, namely, the one wants to claim that what initially passed as a contract between him and the other party should be invalidated as having never existed. Hart points out that the judge decides the matter not by seeing whether the transaction under dispute falls under a well-defined concept of contract (that is, a concept with specifiable necessary and sufficient conditions for application), but by seeing whether the complaint conforms to one of a more or less well-defined set of defeating conditions, as in the misrepresentation of the terms of the contract or incompetence of one of the parties. This practice may be traced to the case law tradition itself, where no codes are present that actually give a legal definition of a term like "contract." Hart then makes greater, Wittgenstein-inspired claims for defeasibility, especially that all mental concepts are defeasible in nature. Thus, there are no defining characteristics for intentional activity; rather, specific intentions are presumed of human beings unless otherwise shown in behavior.

For our purposes, Hart's defeasibility conditions are interesting because they provide an alternative way of thinking about what it means "to apply a
concept. On this view, misapplications may have more in common than correct applications, in that the former cases are defined by specific defeasibility conditions, while the latter cases simply share the fact that they have not been defeated as applications of the concept. Defeasibility conditions may thus be the key to understanding how a scientific paradigm can be pedagogically constraining yet liberal enough to permit innovative scientific work. The historian of science searching for the defeasibility conditions for applying a paradigmatic concept would ideally proceed to find two apparently similar situations where a concept is applied, but in one case the use is taken as unproblematic, while in the other it is taken as "problematic," either in that the scientist is taken to have erred or that he is taken to have made a radical move which challenges how other concepts are subsequently applied. Articulating the difference that allowed the first application to pass unnoticed but not the second would amount to a codification of tacit knowledge. The precise nature of this codification awaits further inquiry. But if negative feedback is indeed essential to scientific practice, then some insight may be gleaned from philosophies of law, especially Hans Kelsen's (1949), that attempt to define a legal system as a pattern for imposing sanctions on law-breakers. And while Kelsen's approach has been criticized for, in effect, reducing legal systems to decision procedures that judges follow upon recognizing an illegality (Moore 1978), it may be exactly the right starting point for modeling the norms governing a scientific paradigm.

We have been proposing that the historian codify tacit knowledge through defeasibility conditions, on the assumption that the negative cases are easier to characterize than the positive ones. Yet scientists themselves tend to utter methodological rules that take the form of a hypothetical imperative, flanked by a *ceteris paribus* clause. Thus, one is instructed to apply concepts or extend theories in certain ways, granting that the appropriate background conditions obtain. In the archetypal case of a scientific law, the force of inserting the *ceteris paribus* clause is to indicate that the background conditions presupposed in a fair, or ideal, test of the law are so numerous and varied that one is better off simply asserting the outcome that would be expected in the fair test—however improbable its occurrence outside an experimental context—than specifying the expected outcome of testing the law under less than ideal conditions (Suppes 1962). In other words, the presence of the *ceteris paribus* clause presumes that the positive case is easier to characterize than the many possible negative ones, thereby *inverting* the strategy behind specifying defeasibility conditions.

When transferred from the statement of scientific laws to that of methodological rules, the ideal test case becomes the optimally rational science that occurs only during selected periods in the history of science, with the rest of science deviating from it in ways too numerous and varied to be codified. Of course, on this account, much of the content of the rules will be determined by the episodes collected together as exemplary by the methodologist. Whatever resemblance statements of methodology have
borne to one another in the past would then stem from the same cases being collected together. Hart (1961) also noted this kind of reasoning in the legal sphere; for even though judges decide cases on the basis of whether a challenged presumption withstands defeasibility claims, the reasons they offer for their decision will be stated in the form of a rule said to be exemplified in selected instances from the body of case law. And as in the case of scientific methodological rules, the judge is more interested in his decision being taken as a precedent for subsequent decisions than as a faithful record of how decisions were rendered in the past, even though it is only by appealing to history that his decision can have the desired impact. Consequently, Hart espouses a principle of judicial discretion, whereby the judge is under no obligation, other than logical consistency, to collect cases together in a particular manner in the course of justifying his decision.

Although methodological pronouncements seem to be subject to a much narrower sense of "discretion," insofar as the exemplary cases of scientific activity are more readily agreed upon than that of judicial activity, the upshot nevertheless remains the same. Because tacit knowledge effectively defines the threshold of scientific competence (below which defeasibility conditions obtain), while methodological rules define the ideal cases of scientific competence (whose rarity is offset by ceteris paribus clauses), tacit knowledge underdetermines methodology. As a result, methodological disputes would seem to be both inevitable and insoluble by appeals to the nature of tacit knowledge.