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Overcoming the Problem of Induction: Science and Religion as Ways of Knowing

ALAN PADGETT

Many public intellectuals today who are science-minded are critical of religion as being irrational. This chapter begins by examining the rationality of science itself, especially as presented in the well-known “problem of induction,” a problem going back to the philosophy of David Hume. The author defends the rationality of inductive, or better *informal* reasoning, which is seen to be at work in both scientific and theological rationality. Four theses concerning informal reasoning are defended: fallibility, tradition, diversity, and contextuality. The author assumes the demise of classical foundationalism, and develops some of the notions found in Thomas Reid’s response to Hume. This exposition and defense of informal reasoning opens up the possibility for a quite different, but equally legitimate, rationality for religious faith seeking understanding.

The dialog between science and religion is a fascinating and important one. In chapter 57 I introduced the history of the complex interactions between Christianity and modern science in the West. In this chapter I will defend the rationality of religion as a way of knowing, comparing it to the rationality of the natural and human sciences. I will argue for the importance of both scientific knowledge and religious understanding in our world today. Whether in China or North America, intellectuals should be wary of rejecting either the teachings of science or the ways of religious wisdom. Modern cultures do not progress by rejecting religion outright, but rather by embracing the proper place of religious wisdom in a sophisticated and contemporary worldview. Despite the longstanding argument of science-minded atheists, which we briefly touched on in our previous chapter, a scientific approach to thinking about reality does not mean we reject all religious truth-claims as mere superstition.

Fortunately for us, we are living in exciting times for those interested in the interplay between religion and science. Gone is the time when religion could be simply dismissed as “anti-science” with any kind of intellectual integrity. The rise of the so-called “new atheism” of Richard Dawkins, Sam Harris, and Christopher Hitchens is surprisingly ignorant of work in philosophy of religion and academic religious studies. Authors like Dawkins still argue

against religion on the basis of science, but with many scholars today I find such "scientism" to be naïve philosophically.¹ My own work these past 15 years in the dialog between religion and science is based upon the conviction that philosophy has a role to play in the religion and science dialogue, and in the world of philosophy and religious studies Dawkins' work is particularly weak. Philosophy helps both scientists and religious believers to develop sophisticated understanding of their assumptions, approaches to understanding, and methodologies. Bringing philosophy into the conversation between religion and science can help create congenial atmosphere in which theology and science can go beyond dialogue, to collegiality. Therefore in this second chapter we move from history to philosophy.

There can be a real mutuality between science and religion as ways of knowing, when their differences are respected. Each discipline, science and theology, has a distinct approach to the quest for truth, and these differences need to be better understood. Only in this way does it make rational sense to bring them together again, when we can respect each mode of inquiry as its own legitimate quest for truth. I also call this perspective the "mutuality model" because I argue that specific sciences and theology, even though they have their own domain as disciplines, can rationally influence each other in special situations without giving up the important distinctive and methods of each. I came to these conclusions within the academic discipline of philosophy, in particular in considering the rationality of modern science. Only after investigating the rationality of science and discovering its communal and personal dimensions did I then realize that much the same can be said of theological knowledge. In particular the study of the problem of induction and the nature of informal logic was very helpful in moving me toward a mutuality model for theology and science. In this chapter I will introduce my solution to the so-called problem of induction, and then explore its implications for both science and theology as legitimate ways of knowing. My reply to David Hume's famous problem of induction is therefore the epistemological centerpiece to that harmony and mutuality which I promote between religion and science.

So in this chapter we have three goals. First we will review the so-called problem of induction. Second, we will defend the rationality of science against those who worry about inductive reasoning, or what I will prefer to call informal reasoning. Third, the results of our investigation of informal reasoning will be applied to the rationality of theology. This will help us respond to a common claim that religions do not provide us with truth because the great world religions conflict with each other. To anticipate our conclusion, these contradictions are to be expected because of the traditional, communal, and participant-observer character of religious quests for truth about sacred reality.

The Problem of Induction

The rise of modern science provided the gold standard of Enlightenment rationality for most philosophers of the West in the seventeenth and eighteenth centuries. In broad terms, the problem is this: how can specific examples, even many of them, justify as true a universal law of nature? Isn't it always possible that the universal law has some evidence that would falsify it? One instance of such counter-evidence would logically have to falsify the law. So how are scientists justified in believing them to be universal?

As a philosopher I have long been interested in the traditional problem of induction, especially in the light of recent moves in epistemology after the collapse of classical

foundationalism. I believe both theology as an academic discipline, and all the special sciences, depend upon informal reasoning for their epistemic warrant. In this account of my solution to the problem of induction, I present four theses on informal reasoning for discussion and critique, without pretending to prove or discuss each one fully. My purpose is to provide some further philosophical grounding for the mutuality model for theology and science.

Ever since the work of Hume philosophers have wondered just how it is that induction really works. No one, not even Hume, has really questioned the fact that induction is a normal part of our everyday reasoning processes.² It is in many ways a part of our common sense. I assume water, which I needed to live yesterday, does not become poison today. I look for a penny in the general area where I heard it drop. Based upon theory and careful experiment, I believe that matter is made up of tiny particles that cannot be directly observed. Not one of these argument forms (real or implied) is deductive, and they all go beyond direct experience. Examples could be multiplied a thousand fold. We do live by inductive reasoning. Hume challenges us to find a justification for such reasoning. In this overview, I take up Hume's challenge.³ Thomas Reid, Hume's fellow countryman and contemporary, gave the best response to Hume's challenge during his own lifetime. My own reply will follow, in broad outline, Reid's reply to Hume.⁴ In particular, I follow Reid when it comes to our everyday experiences. We depend upon our common sense, and our prior, pre-philosophical assumptions, to justify our everyday inductions concerning the world around us. But does that also apply to the special case of academic disciplines? Can our common sense, everyday reasoning sustain the special sciences?

This further question is the topic of the current chapter. I hope to make some improvements upon Reid's "common sense" approach. I will argue that the collapse of classical foundationalism in recent epistemology provides the philosophical horizon for improving upon his solution to Hume's problem.

Henry Kyburg usefully distinguished between two types of problems for induction: the problem of description and the problem of justification.⁵ The descriptive problem is one of spelling out the general principles we use in good inductive arguments. For the most part, I shall ignore the specifics of this problem in this chapter. The problem of justification is one of giving reasons or grounds for the rationality of inductive principles. This is Hume's challenge, and the central focus of this chapter.

Reid's Common-Sense Realism

Reid's response to the challenge of Hume was to appeal to our "common sense."⁶ Reid recognized that the problem of induction is a philosopher's puzzle. The fact that logicians have not, so far, been able to solve this puzzle does not imply that induction is unreliable.⁷ We are justified in relying upon induction because of its pervasive role in our everyday life, and because it plays a part in the common sense of humanity. As he wrote in his 1764 treatise, *An Inquiry into the Human Mind*:⁸

If there are certain principles, as I think there are, which the constitution of our nature leads us to believe, and which we are under a necessity to take for granted in the common concerns of life, without being able to give a reason for them – these are what we call the principles of common sense; and what is manifestly contrary to them, is what we call absurd.

Reid recognized, along with Kant, that there are certain principles of inductive argument, which provide justification for our assessment of particular informal arguments.⁹ From now on, I shall use the term “informal” instead of “inductive,” for reasons that will soon become clear. Some of the principles of informal inference may be propositions, while others are criteria for theory choice, which are perhaps best understood as epistemic value judgments. These latter can be spelled out as propositions, but are typically discussed in simpler format, often a single word (e.g., fruitfulness or parsimony).

Reid's point that the problem of induction is a philosopher's puzzle, which does not in fact undermine our reliance upon it, has been clarified in the recent debate between externalism and internalism in epistemology. We should distinguish between a person being justified in believing that *p* is true, and a person consciously seeking warrant (or justification) for one's belief that *p* is true. A person may in fact be warranted (or justified) for accepting some proposition, without being able to articulate that warrant.¹⁰ In other words, we need to distinguish between justification as an activity (internal) from the state of being justified (external). I take it that not only Reid, but most externalist or reliabilist epistemologists would accept this point. And there is surely something correct here.

There is a problem with the justification of informal inference, however, in those infrequent cases when I may have cause to doubt some common-sense principle (or perhaps, the manner in which I have brought common sense into language). A young boy raised in a very honest small community will come to rely rationally upon the principle of testimony. All other things being equal, we should accept as true the sincere report of people regarding some matter they are in good position to know. Without such a principle, or something like it, we would know almost nothing in fact. However, the boy grows up and moves to Metropolis, where liars and con artists soon teach him to modify the principle. He may now wonder: is this principle in fact a reliable one, epistemically? In seeking warrant for the principles of informal arguments, we are in fact consciously seeking warrant for our beliefs. I do not recommend this as a systematic program, *à la* Descartes, but there are situations (such as incoherence among principles, or the case of our young friend) that call for the investigation of our warrant for accepting these principles. In such situations, even if one is rational to rely upon the deliverances of some informal inference, one will want to investigate further to see why it is rational to rely upon them. I believe this is particularly true in the academic disciplines. The development of special interests and explanatory focus may call us to question our “common sense” assumptions. The history of quantum mechanics in the twentieth century is a well-known example of what I have in mind.

Hume challenged us to discover why it is we believe these “common sense” principles, that is, to find the grounds for our rational reliance upon informal arguments to provide us with knowledge. The fact that informal inferences are reliable for an individual in our everyday settings is not at issue. Informal inferences can be reliable for a particular person in a “normal” setting, even though logicians may not be able to discover why they are reliable. The logician's puzzle is: what warrant (if any) can we give for our reliance upon patterns of informal inference *when they are called into question*? I am particularly interested, in this essay, in the justification of informal reasoning within academic disciplines like theology or specific sciences. I believe this is an important problem even if we normally are rational to rely upon our “common sense” informal inferences. Notice that this question should come in a particular, rather than in a general, form. To make any headway in this area, however, we will need to clarify what an informal argument is.

The nature of informal inference

Some good arguments are formal or deductive in character. These arguments are such that, if their premisses are true, then their conclusions must (logically must) be true. Arguments like this in ordinary language can, in principle at least, be expressed in a formal and symbolic manner that demonstrates their deductive character. I say "in principle" because there is no universal, agreed upon logic accepted by all cultures. The logics we do have, moreover, have not always been known. I will call all deductive arguments "formal" even if they are not so expressed, because of this in-principle deductive structure. All other good arguments are informal (that is, not formal or non-deductive).

In order to clarify this distinction, we need a different notion other than the standards of validity and soundness, which are well-formed notions in formal logic.¹¹ I should like to add the idea of the *assessment* of an argument, to discover the epistemic support that the premisses (if true) give to the conclusion. The sign of a good formal argument, then, will be that the epistemic support is the strongest possible one: logically necessary. On the other hand, a good informal argument will give epistemic support to its conclusion, without logically determining it. Let us call this "cogency."¹² Cogent informal arguments increase the likelihood that their conclusions are true, but do not make them logically necessary.¹³ By the "assessment" of an argument, then, in this technical sense, I will mean the investigation of the strength of epistemic support that the premisses give to the conclusion.

In the sweep of Western cultural history, philosophers from Plato and Pythagoras to Russell and Husserl have given priority to demonstrative, formal reasoning. The model for such reasoning has been mathematics. In our day, as part of the rejection of classical or radical foundationalism, we should learn to appreciate degrees of knowledge that are less than logically necessary. By *classical foundationalism* we mean that epistemological program, at the heart of so much Western philosophy, which sought to ground our knowledge-claims upon "basic beliefs" (beliefs not founded upon other beliefs) that are "self-evident or incorrigible or evident to the senses."¹⁴ This is the program that Richard Rorty attacks under the name "epistemology" in his *Philosophy and the Mirror of Nature*.¹⁵ Of course, there are more types of epistemology, even in classical Western philosophy, than classical foundationalism. This is a good thing, since classical foundationalism is in trouble. I will simply assume, rather than argue, that classical foundationalism should be abandoned in epistemology.

But if classical foundationalism should be rejected, so should the privileging of demonstrative reasoning over probable reasoning. After all, certain knowledge is notoriously difficult to attain. In areas of philosophy important for human flourishing, such as the philosophies of ethics, religion, science, politics, and beauty, almost all of our knowledge is probable and fallible, rather than deductive. One could claim that probable knowledge is far more important for human life than deductive knowledge. So why can we not begin to value such knowledge, *alongside* deductive truths?¹⁶

Once we give up the opinion that only formal, deductive arguments are *real* arguments, many of the so-called "problems" associated with induction disappear. A formal argument is one whose validity depends upon the (in principle) logical structure of the argument, on purely formal grounds. These arguments can be given symbolic structure. Informal arguments, however, are *not formal*. A full assessment of the argument cannot be carried out merely by attention to their structure or symbolic form. The conclusion of an informal argument trades upon the content of the premisses the way that a deductive argument

trades upon the formal logical structure. To assess informal arguments, therefore, we must pay attention to the meaning of the premisses and our background knowledge concerning their content, in order to determine whether the argument is cogent. The structure or form of an argument is important, of course. I am not denying this. But structure alone is not sufficient in the case of informal arguments. This follows from the very fact that such arguments are informal. And yet, so many "problems with induction" have arisen because philosophers trained in symbolic logic have complained that the formal (symbolic) structure of informal arguments are not formally valid.¹⁷ Of course they are not! All good informal arguments must (logically must) be formally invalid. If they were formally valid, they would be formal (deductive) arguments. To remind us of these facts, I call inductive reasoning "informal," hoping the name will press home the point that informal reasoning cannot be assimilated to thinking along formal lines.

Let us take an example of this problem. Carl Hempel often discussed a paradox of induction, called the "black raven paradox" or just the Hempel Paradox.¹⁸ J. L. Mackie once called this the paradox of confirmation. This paradox arises from assuming that (in an informal argument) "[i]f observations confirm one formulation of a hypothesis they confirm any logically equivalent formulation."¹⁹ Add to this the further assumption that "There is a black raven" is to be understood as a material conditional (If x is a raven then x is black). It then follows that the supposed general law that "All ravens are black" is supported not only by "There is a black raven," but also by, "There is a non-black non-raven", for example, "There is a blue car." But our common sense tells us that "There is a blue car" does not help confirm the law in question about black ravens.

The problem here lies in importing an idea from symbolic logic, namely the *material conditional*, and imposing it upon the informal logic of simple induction. Since evaluating an informal inference depends upon knowing the content of the statements, and an often complex background of related knowledge, the way the hypothesis is stated is important to our assessment of the argument. Hempel's paradox rests upon a common problem. He assimilates rules from formal logic into his analysis of what is best understood as an informal argument. And this is just to misjudge the differences between formal and informal logics. I am not arguing that cogent informal arguments may contain logical contradictions. We are talking instead of standards for assessing the epistemological weight that premisses give to conclusions (*evaluation*). The rules for inference from one premiss to the next (i.e., *derivation*) are often quite different in formal and informal arguments.

It also follows logically from a proper definition of informal inference that, for all good informal inferences, the premisses will only make their conclusion likely to be true. All of the premisses may be true, and the conclusion could still be false. Notice that this fact follows from the very definition of "informal argument." So this fact is not, and cannot be, a "problem of induction." On the contrary, it is a logical truth. In the philosophy of science, for example, theories are supported by evidence through informal arguments.²⁰ This means that theories will *always* be "underdetermined" by the data. It is logically necessary that, if the sciences are based upon informal patterns of inference, their theories will be underdetermined by the evidence. If the evidence (premisses) *determined* the theory (conclusion) by logical necessity, then we would have a formal (deductive) argument.

Good informal arguments, then, cannot be fully assessed on the basis of their structure alone. Two informal arguments can have the same structure, and one can be cogent while the other is not. Assessing informal arguments often depends crucially upon the semantics of the argument, not its syntax. Of course clarifying the syntax or form of any argument is

important, in the analysis of any argument. But to assess an informal argument, I need to know the meaning of the premisses, and know something of what the argument is about. Perhaps it is time for an example. Consider the following argument.

- (1) Over the years, I have observed that when I fail to water my grass lawn in the summer, the grass dies.
- (2) While on summer vacation out of state, I learn that the lawn was not watered the whole summer.
- (3) I infer that, more likely than not, the grass on my lawn is dead, even though I have not observed it (yet).

To assess the argument, I need to know some things about grass, water, lawns, and summers. Abstract reasoning about "All Fs are G" is not really of any use. For consider the symbolic version of this argument:

- (4) Some Gs (observed unwatered lawns during the summers) are F (dead).
- (5) Therefore, all Gs are F.
- (6) M (my lawn back home) is G.
- (7) Therefore M is F.

But this is not a formally valid argument, because of the step from (4) to (5). Although (7) does follow from (5) and (6), (5) itself does not follow deductively from (4). Yet this is *not* a "problem of induction." Good informal arguments should not be assessed as one would a formal argument.

An interesting corollary to the right understanding of the nature of informal inference has to do with probability calculus. Some philosophers, following Thomas Bayes, respond to Hume by turning to the mathematics of probability.²¹ But the probability calculus, when used properly, is a kind of mathematical argument whose conclusions must (logically must) be true. So the probability calculus is a kind of formal argument. The conclusion of a valid, symbolic argument in the mathematics of probability must be true if the premisses are true. So such arguments are formal, not informal, patterns of inference. If I know that 10 percent of all Methodists are Canadians, and if about all I know about Debbie is that she is a Methodist, then I know with logical certainty that the probability she is a Canadian is 0.1. This is a formal (not an informal) argument, even though it is about probability. Perhaps we should use the term "likelihood" for informal support for conclusions, rather than probability, to avoid confusing them. In any case, in Bayesian terms, a full assessment of an informal argument must include the *specific reasons* why evidence *E* makes one hypothesis (*H1*) rather than another (*H2*) probable. Merely knowing in the abstract that $P(H1/E\&K) > P(H2/E\&K)$ does not allow me to assess the argument fully. The starting probabilities in a Bayesian argument are usually known on informal grounds. So while the Bayesian calculus may well be helpful in some areas, we cannot rely upon it alone to assess informal arguments.²²

I have stated that in order to assess an informal argument, like the one given about my unfortunate lawn, we need to know something about the subject of the argument. That is, in informal arguments we need background knowledge about the premisses and the objects being discussed, in order to assess the likelihood that the conclusion is true.²³ How likely is it, for example, that there might have been enough rain over summer to keep my lawn alive?

But that is not all we need to know. We also need to have a good grasp of the principles of informal inference. These principles, I will argue, differ slightly from discipline to discipline. So chemists, for example, need to be trained in the practice of natural scientific reasoning, just as detectives need to be trained in the science of good detective work. This brings us, then, to the principles of "common sense" which Reid believed included the principles of sound reasoning.

Tradition and Reason in the Principles of Informal Inference

Postfoundational theses

There are four theses that I will propose concerning the "principles of induction," which go beyond Reid's position on the "self-evident" character of the principles of reason.²⁴ The positions which I present for discussion presuppose the collapse of classical foundationalism.²⁵ At the same time, I do seek some kind of ground or basis for our acceptance of these principles and propositions, even if not classical foundations. Is that not a contradiction?

I The fallibilist thesis and noetic warrant

In the presence of a reason for doubt, rational people will seek justification or warrant for our beliefs. The Cartesian program of radical doubt should be avoided, but in the presence of reasonable doubt, some further analysis is reasonable. This seeking of grounds or reasons is not classical foundationalism, just because I agree that these grounds may be fallible. On the other hand, given some reason for doubting a belief, it is reasonable to ask the question, "Why should I believe it?" I should like to present two fallible grounds upon which we might seek justification for informal principles of reasoning within academic disciplines. Broadly speaking, these grounds will be reason and tradition. Let us begin with reason.

In looking at the grounds for our faith in informal reasoning, Hume overlooked a possible ground for the principles of informal inference. This is a problem with Hume's criticism of informal reasoning. Hume presents us with only two options, when in fact a third possibility exists. Thus, his argument commits the fallacy of a false dilemma. Hume's argument relies upon the claim that knowledge is based either on deduction *à la* mathematics, or on impressions (sensations). But it is surely possible that inductive arguments are based on principles that are known by rational intuition, but are not necessary truths like mathematics. This was the view of both Kant and Reid, in their response to Hume. This option is one that I believe is fruitful for understanding the bases of inductive reasoning.

I suggest we develop a fallibilist notion of rational insight as warrant or justification,²⁶ which I will call "noetic warrant." Leibniz, Hume, Kant, and other philosophers assimilate principles of reason to the example of mathematics (or in more modern cases, symbolic logic). In this they surely do philosophy a disservice. Our claim to know something by reason does not imply that the idea is necessary and universally applicable, at least not in every case. Some principles may not be logically necessary, while others may be limited in the kind of circumstances in which they apply. I propose that we find another category of rational judgment at the foundation of our acceptance of informal argument.²⁷ Let us call a "noetic concept" any idea which, it is proposed, we accept on the basis of so-called *a priori*

or *noetic warrant*. Noetic concepts may be propositions, epistemic values, definitions, or criteria. Now the warrant for a proposed noetic concept comes (at least in part) from rational intuition. This implies a *process* of seeking warrant for such propositions or principles. Noetic warrant makes a claim to knowledge that:

- 1) may arise in the context of experience, but
- 2) goes beyond the deliverances of experience, strictly understood, to assert an insight or intuition into the subject based upon reason.
- 3) what is known in this way does not have to be necessary and universal.²⁸
- 4) the claims we make to noetic warrant may be wrong.

On this fallibilist understanding of noetic warrant, we have moved from *all* to *some*: some appeal to rational insight makes the warrant for our concept noetic, even if it is mixed with an appeal to experience. Of course, it may be that all of the warrant is purely rational, but it need not be that way. Given this mixed idea of noetic warrant, it makes sense to test, examine, and explore supposed noetic concepts. This is the sort of reasoning that is typical concerning moral first principles. I believe that Putnam points us in the right direction when he argued that (so-called) *a priori* truths – which I call noetic concepts –

have the character of *maxims* – general principles that are not, or at least may not be, exceptionless, and they involve “generic,” or somewhat pre-theoretical, notions rather than the (supposedly) perfectly precise notions of an ideal theory in the exact sciences.²⁹

It is points (1), (3), and (4) that make this notion of noetic warrant different from Hume’s “relations of ideas” and Kant’s “*a priori* judgment.” Examples of such noetic concepts would be logical truths like *modus ponens*, the principle that only real things have causal efficacy, rational criteria for theory choice, or basic moral truths. On the other hand, I agree that sometimes epistemic warrant is based fully upon experience. So another category would be “empirical warrant”, that is, a *posteriori* warrant for some proposition.

The typical claim, since Leibniz, is that the truths of reason must be necessary. But Kripke has, I believe, given us good reasons to affirm that some noetic concepts may be about contingent things. Richard Swinburne, in a careful reply to Kripke, gives several possible definitions of “necessity” and “*a priori*,” and this seems right.³⁰ There is indeed more than one meaning to these words. I am willing to accept the proposal that all noetic warrant is based upon “necessity,” given the large and flexible definitions of necessity that Swinburne develops. Reid certainly had, for example, a much larger conception of necessity than logical necessity.³¹ Perhaps we will develop our notions of necessity even more over time. But I am not committed to this thesis. I suggest that we simply look and see. What I wish to deny, however, is that all noetic concepts are “necessary” in the sense of *logically necessary*.

A noetic concept, then, does not have to be an analytic proposition. The fact that “every colored object takes up space” is something known to be true on the basis of noetic warrant, and not simply from experience alone. We can state this fact in different words (“All colored things are spatial things,” etc.), but any argument for it assumes its truth in some way. Yet it is not an analytic truth, in the strict sense of “contained in the meaning of the words.” The meaning of “colored” does not logically include the notion of “spatial.” It is reasonable, on the other hand, to assume that this noetic concept is a metaphysical truth. In any possible world story, whenever we instantiate the property of being colored, we likewise instantiate

(by implication) “being spatial.” But this noetic concept is not an analytic truth, because “being colored” does not include in its meaning “being spatial.” This conclusion is hardly surprising, however, since being analytic is a logical property, while “known to be true on the basis of noetic warrant” is an epistemological property.³²

Noetic warrant is not based upon logical arguments, nor simply the accumulation of experiences (although they may arise from and be stimulated by our encounter with the world). The warrant by which we agree that proposed noetic concepts are true comes from rational reflection upon the proposition (criterion, etc.). This rational reflection is what I call noetic warrant. How do we know when some proposition is genuinely noetic, rather than empirical? Two signs or symptoms point the way. First, any attempt to explain our concept ends up assuming it (or something like it). To draw another example from ethics, it is very hard to explain why doing and being ethically good is to be pursued without assuming this to be true already. In other words, careful analysis of why a person should believe this proposed noetic concept shows that it is not based simply upon cumulative experience, nor upon some argument. Second, our grounds for knowing the concept have the air of rational insight, intuition, or appeal to reason. To take an example from logic, when we reflect upon why we believe the law of non-contradiction we find ourselves appealing to rational insight, and not to cumulative experience. Another way of putting this same point is, the quality that makes this true belief qualify as *knowledge* is rational-plus-empirical, and not merely empirical alone.

The claim to know some truth noetically is not an arbitrary one. Since noetic concepts are not always logically necessary, it makes sense to test and explore our noetic warrant. For example, some statements we hold to be true through noetic warrant will be analytic, and others will be synthetic. Only by exploring the nature of our noetic concepts can we know if one is synthetic or analytic. Once this is determined, then we can also explore the truth of our proposed concept. This exploration or assessment is not a kind of verification or proof. Herbert Feigl made the important distinction, concerning the rational exploration of the first principles of reason, between *verification* and *vindication*.³³ While basic principles cannot be verified, since they are at the foundations of rationality, they can be vindicated. They can be explained by being clarified, and set within a system of ideas, which expands upon them and demonstrates their use.³⁴ I am proposing, then, that we seek vindication for the principles of informal inference.

In one place, Reid seems to have understood this point. He wrote,

The most simple operations of the mind, admit not of a logical definition: all we can do is to describe them, so as to lead those who are conscious of them in themselves, to attend to them, and reflect upon them; and it is often very difficult to describe them so as to answer this intention.³⁵

This notion of needing to further clarify and define the “simple operations of the mind” is part of what a good vindication does. We may also, along the way, need to correct our noetic concepts.

There are at least three ways in which our acceptance of a noetic concept can be made more sure. One is by further empirical exploration. Are there other areas of knowledge and experience, not originally considered, that help us to believe (or disbelieve) in the principle under consideration? Such empirical considerations help our sense of surety about our warrant, but should not mean that the noetic warrant is based upon experience alone. If we

find that, after all, the warrant is based upon experience alone, then we discover that it is not noetic, but empirical warrant.

Second, the principle can be placed within a larger system of which it is a part, and which helps make sense of it. To the extent that the larger system is credible, so too will our principle be. Sometimes basic principles of a worldview can only be tested in experience, over a long history, and in dialogue – even conflict – with rival traditions and worldviews.³⁶ Third, we can explore the implications of our principle, given our other beliefs, and examine these implications. So, for example, *modus ponens* is as secure a noetic concept as any. One can, of course, test it to see if it makes sense in terms of ordinary reasoning processes. We can place it in a larger system of formal logic, and see its implication for other forms of reasoning, examining these implications as well. So too the moral principle that “it is good to help those in need” or something like it (say the principle of benevolence) can be tested by comparison with standard examples of goodness. It can be placed in a larger frame of ethical theory, and its implications can be questioned by borderline cases. Claims to noetic truth, then, may fail the test: what we thought was based upon reason may not have been.

Hard-nosed skeptics may be thinking something like along these lines. “Very well. You have defined a type of reasoning that, if there is any such, improves upon Reid’s solution to the problem of induction. But are there any of these ‘noetic concepts’ which are synthetic? If so, show me!” Let us take, then, an example from scientific reasoning. One type of informal argument is the inference to the best explanation (or better, inference that an explanation is best), and one criterion for a good explanation is that of comprehensiveness. The best explanation covers all the relevant data. Is this criterion empirical? Have we looked at lots and lots of good explanations to come to this conclusion? Of course not. It is a rational requirement, not an empirical conclusion. So the criterion of comprehensiveness is noetic. Perhaps, then, it is analytic. Yet its denial is coherent and possible, and its truth does not come from the meaning of the words in the sentence. Only an arbitrary, *ad hoc* definition of “good explanation” as “covering all the data” can possibly validate an argument that this criterion is analytic. I can prove any statement is analytic with such logic! “The moon is made of green cheese,” for example, is analytic when we define the moon as “a verdant heavenly body composed of moldy milk products.” One will have to argue that we use “good explanation” to always mean “covers all the data” – but that is not true to our use of these terms in science and philosophy.³⁷ Sometimes a “good explanation” is one that satisfies the questioner, for example. The criterion, then, is not analytic. So it is synthetic. The criterion of comprehensiveness, then, is a synthetic noetic concept.

The principles that underlie induction, therefore, may be justified (in part) by noetic warrants. Some of these noetic concepts may be necessary propositions; others may not. In this way we can advance Reid’s solution to the problem of induction, and also avoid the objections to “apriorism” voiced by many philosophers of science today who are heirs to Hume.

II The tradition thesis

From the definition of informal inference, to recap, I have argued that we need to know the content of the argument (its formal and informal character – its semantics and its syntax) in order to assess the strength of the argument. This further requires both a background knowledge concerning the subject(s) of the argument, and some grasp of the standards of

informal inference relevant for that subject. These standards must be learned, just like the background knowledge. To become an excellent chemist, for example, is not only to learn the facts and theories of chemistry. It is to be inducted into a particular history and tradition of standards of argument.

Pierre Duhem, early in the last century, gave an oft-quoted example of a visitor to a physics lab. A scientist is engaged in an experiment to measure electric resistance, but the visitor (who is not a scientist) only sees the oscillations of an iron bar which carries a mirror. The visitor asks the scientist what he is up to. "Measuring electric resistance" is the answer. When the visitor asks the scientist what mirrors and iron bars have to do with electric resistance, he is told to take a course in electricity: the explanation would be a very long one!³⁸ In cases like these, we can see that informal arguments (about electricity or other things) depend crucially upon important training. As N. R. Hanson rightly argued, "physical science is not just a systematic exposure of the senses to the world; it is also a way of thinking about the world, of forming conceptions."³⁹ In a similar way, Stephen Toulmin argued in *The Uses of Argument* that our standards of good argument are "field-dependent."⁴⁰ To learn the sciences is, in part, to be trained in a particular way of forming conceptions. Lawyers, too, go through such training. But the kind of informal thinking typical of law is quite different from that of natural science. Both practices are rational, yet they are not identical in their standards of what counts as good informal argument.

Perhaps the whole of Kuhn's philosophy of science cannot be accepted, yet he wrote one of the most influential academic books of the last century. His notion of "paradigms" at the basis of scientific practice and rationality has been widely accepted, and rightly so. But a paradigm includes both background facts and theories, as well as standards of rationality. Since Kuhn notoriously failed to give much of a definition of "paradigm" (a word he borrowed from Wittgenstein), perhaps I ought to indicate how I use the word. To students I explain that a scientific paradigm rests between a "theory" and an entire "worldview," and includes elements of both. Paradigms ground the traditions of rational inquiry in the humanities and sciences, including background knowledge and value judgments. Almost all of them find this a helpful comment, although it is also not a definition. Still, by a careful definition of "theory" in science, along with "worldview," we can use this concept of "between" to specify somewhat what a paradigm is.

The role of paradigms in scientific inference would indicate that we learn such standards, along with facts and theories, when we are initiated into a particular academic discipline. Imre Lakatos developed this notion further, with his theory of rival "research programmes" which have conflicting histories of development, conjecture, theories and practices within a particular science or academic discipline.⁴¹ I believe that Lakatos is right when he claimed that his theory of research programs is superior to the philosophy of science of both Kuhn and Karl Popper. Larry Laudan has developed these ideas in helpful directions, correcting Lakatos at some crucial points.⁴² But in broad terms, the point of a research program for our thesis is that such programs are (among other things) traditions of inquiry. Call them paradigms or research programs, scientific reasoning comes in traditional packages.⁴³ We may wish to borrow the term "doxastic practice" from William Alston, to speak of such traditional, learned rationality.⁴⁴ This includes the standards of informal reasoning accepted within that particular tradition.

The implications of this for the "problem of induction" are significant. For if a particular standard of good informal inference is an established part of a successful, progressive, and long-term research program, that standard has *prima facie* justification. We will need

some reason to doubt that standard, before we spend our precious time upon this earth in exploring the warrant for our belief in that standard. But what are these standards of informal inference?

III The diversity thesis

Reid believed that there was a short list of principles and faculties belonging to common sense, that are found at the foundations of our faith in perception and informal reasoning.⁴⁵ Despite the work of logicians like Mill and Russell, no one has made a convincing and complete list of such principles. I propose we give up the quest for a short list of self-evident principles. Instead, let us accept the fact that informal inference is a very varied practice, with many basic principles which guide good inference.⁴⁶ Since there are so many different kinds and patterns of informal inference, why should we think that there is some simple list of principles that underlie all informal reasoning? In the examination of particular forms of informal inference, we may find some similarities; but I expect we will discover a number of differences as well. Only careful and sustained exploration will clarify the number and scope of the principles of induction, and indeed we may never discover them all. Informal inference is certainly not based upon some simplistic principle, like “the uniformity of nature.”

There may in fact be more than one explanation for why we accept a particular informal argument. Let us take an example from Nelson Goodman.

- (8) All observed emeralds have been green.
- (9) Probably, the greenness of emeralds is a product of the chemical make-up of the mineral.
- (10) The chemical make-up of a mineral is stable across time and space.
- (11) Probably, all emeralds are green.

The meaning of “probably” in these statements is: more likely to be true than any alternative hypothesis. What reasons can we give for accepting this particular informal argument? First, we should note that (9) is a proposed explanation for (8). So the first move is an instance of what Peirce called “retroduction,” that is, inference that some explanation is better than rival hypotheses. Assessing the likelihood of this first step in the argument would require a good deal of chemical evidence and background knowledge. We would need to know the physical basis for color, and the chemical analysis of emeralds. Given all the data, we could then argue for (9), using the standards of argument found in the central research programs of physical chemistry.

Notice how our evaluation, so far, vindicates our major points already made in this essay. Assessing this informal argument required knowledge of the particular things and properties the argument is about. Merely formal issues may be necessary, but they are not sufficient. The argument that (9) is the best explanation for the fact of (8) will not be based upon data alone. It will also include principles for theory assessment in chemistry, and in general the “paradigm” of current chemical science. We may need to rely upon a principle of simplicity for physical properties within proposed chemical theories, for example, in order to rule out colors such as “grue” for minerals.⁴⁷ It is hard to specify in advance all of the principles and criteria we might need to substantiate the truth of (9) based (in part) upon (8). So our example also validates our diversity thesis.

Some have argued that there could be a nearly infinite number of hypotheses that fit the same facts. This is logically possible, but once again smuggles standards of argument from formal logic into the consideration of informal inference. In the actual practice of chemistry, there will be a limited number of hypotheses to explain a focused and limited amount of chemical data concerning some material. *We do not, in informal inference, have to argue against all logically possible hypotheses*, but merely against those actual hypotheses that investigators (including oneself), trained in the same traditions of inquiry, put forward.⁴⁸ This places a difficult burden upon those who would reject some theory in science: propose a better one! Simply poking holes in the current theories, without a new proposal, will not advance science in the long run.⁴⁹

Let us assume that, in a context of actual rival chemical hypotheses, we have given a good argument for (9) based upon (8). What about the next step? Here there are a number of metaphysical commitments that we rely upon in our move from (9) to (11). For example, we assert in (10) that central features of the chemical make-up of a mineral are part of the essential properties of that mineral. The standard example among philosophers of science is, "water is H₂O." We also assume that, in this particular case, the color green is a symptom of those essential properties. Now it is certainly logically possible that we may be wrong. Yet an important point must be made, in order to avoid a modal fallacy. Just because it is logically possible that I may be wrong, it does not follow that I am wrong. This is a modal fallacy, which some critics of induction need to avoid. What we need is some reason to believe that (11) is false, given (9) and (10). *The mere logical possibility of error is simply a part of the very nature of informal inference, and should be ignored.* "Somewhere within the realm of logical possibility may be lurking a defeater, but until it rears its ugly head, we need pay it no mind."⁵⁰ Of course, the critic may feel that anything less than logical certainty does not count as knowledge: but this is an opinion we have already discussed and rejected along with other elements of classical foundationalism.

The reasons, then, that (9) and (10) are good evidence for (11) are diverse. There is no simple principle at work here, but a detailed knowledge of the chemical make-up of emeralds, in the context of broad chemical knowledge concerning crystals, colors, and the doxastic practices of our chemical paradigm.

IV The contextual thesis

One important reason there is great diversity among the principles of informal inference is the great variety of such patterns of reasoning. Another reason, however, has to do with the many areas of research in which we use informal inference. If rationality is a traditional practice, it follows that there may well be principles of good informal argument which differ from science to science, and from discipline to discipline. Toulmin argued for this some time ago, following some of the insights of Wittgenstein. The principle of simplicity, for example, is one example of a principle of informal inference. But simplicity of theory in mathematics will be quite different from simplicity of theory in biology.

To take another example, the fictional detective Sherlock Holmes once remarked that (concerning criminal investigations) once we eliminate the impossible, whatever is left, however improbable, must be true.⁵¹ This principle is one that may help us decide who committed some crime. But it is of doubtful use in other areas of investigation, say, concerning theories of the origin of life. In the case of Sherlock's principle, one can reasonably assume we have a complete list of all possible explanations for the crime. Given the evidence

of the crime, we can create a reasonable list of all possible suspects and means. If we can eliminate some as impossible, the others must (if our assumptions are true) be the answer. This principle can, in fact, be verified by deductive logic. But this principle cannot be used universally. In the area of prebiotic evolution, which falls in the discipline of chemistry, we have no reason whatsoever to believe that we know all the possible ways in which life could possibly arise. Eliminating some theories as impossible, in this case, does not mean that whatever is left, however improbable, must be true.

The principle of contextuality does not claim that these principles are invalid because they are not universally applicable. Instead, contextuality follows from the fact that we must know about the objects and properties involved in an informal argument, in order to assess its soundness or cogency. The more we understand about the things the premisses are asserting and assuming, the better able we are to assess the strength of the argument. If different disciplines and traditions of argument are involved in assessing informal arguments, it should not be surprising that criteria for what counts as a good inductive argument may differ from discipline to discipline. There are indeed general and abstract principles of inductive argument that apply to all disciplines. These are usually discussed in textbooks about critical thinking and formal logic. The law of non-contradiction, applied to what counts as a good theory, would be an example of such a general rule. But even these will need to be contextualized, to spell out their significance for the particular argument being assessed. And there will be plenty of room for argument about the *meaning* of these principles for particular arguments. Finally, in addition to these general principles there will be special principles that have been developed within each discipline.

Thomas Kuhn is one philosopher of science who famously argued for the contextual thesis.⁵² “[E]ach paradigm will be shown to satisfy more or less criteria that it dictates for itself and to fall short of those dictated by its opponents.”⁵³ This led him to conclude that choice between paradigms is a matter of “a mixture of objective and subjective factors, or of shared and individual criteria.”⁵⁴ Here I think we should distinguish between contextualism and relativism. I do not hold to the relativist position of radical incommensurability between paradigms. The historical fact that different scientists have been able to argue together, for example, would indicate that a strong principle of incommensurability is contrary to the actual practice of scientific debate. A good example of this would be the debate between Galileo and his Aristotelian critics. They were able to debate the issues just because their paradigms were not radically incommensurable. Members of the two paradigms in astronomy understood each other, even though they also meant slightly different things by the same words. As Kuhn himself notes, this kind of trans-paradigm dialogue is possible, but “for that we must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign.”⁵⁵

With respect to the criteria for standards of good informal inference, I would continue to hold to a diversity approach. Some standards may indeed be relevant only to that particular discipline, or even a particular research program in that discipline. But there are surely a large number of criteria and propositions that guide informal argument, are largely similar, and overlap the different paradigms. Kuhn called these “shared values.”⁵⁶ The more a standard or epistemic value appears to be based upon reason and common human experience, the more it may be a standard that can be shared between paradigms. For example, our criterion of comprehensiveness would be an example of a standard for informal inference

which can be used in a variety of contexts. However, the contextual thesis insists that the meaning and application of these general criteria or propositions *must be contextualized to be fully of use* in any given research program.

Perhaps philosophy of science, at this point, should borrow a page from the philosophy of Thomas Aquinas. Shared criteria of informal inference, which overlap between several research programs, may not have univocal meaning in all contexts. But that does not imply their different uses are equivocal. The similarities and differences between "simplicity" or "comprehensiveness" in different disciplines may best be describe as *analogous*. This provides both for a *weak* incommensurability thesis, and yet allows for criteria of good informal reasoning that are not relative to particular traditions of inquiry.

I suspect that a number of questions and objections may already have occurred to your mind, with respect to this somewhat postmodern approach to Reid's solution to the problem of induction. One can be immediately laid to rest. Have I really tackled the problem of induction, you may ask? After all, the problem is usually set forth in formal, logical terms. And I have not answered the problem in those terms. Instead, my approach has been to argue that *in these terms* there may not be a problem at all. Formal logic may be of limited use, of course, but not of central import for evaluating informal arguments

There are, however, informal fallacies of reasoning as well as formal ones. Charges like incoherence or circularity apply to both kinds of arguments. If, as is often maintained, the principles of informal reasoning are *circular* in nature, then there will be a genuine problem with the justification of our principles of informal inference. A circular argument is one that states its conclusion as a premiss, often in a disguised manner. If there is at least one principle for informal inference, whose justification is not based upon itself nor upon any other informal principle of reasoning, then it will be false that all of the principles of informal inference are circular in nature. We therefore need only one non-circular argument to refute the claim that all the principles are circular. I have already argued, above, that accepting the principle of comprehensiveness in theory choice is not based upon experience, nor informal argument. So there is at least one principle of informal inference that is not based upon any other. Therefore, they cannot all be circular.

What we really need here is a particular argument that a specific principle of informal inference is either circular, or perhaps baseless. We can then look and see if that is the case. Wholesale arguments to this conclusion are not very convincing. Typically, we justify a principle of informal argument by appeal to our background knowledge and/or our common sense. Here I think Reid is right. For example, it is a principle of textual criticism that (all other things being equal) the more difficult reading is more original. Asked to justify this practice, the textual critic might point to our knowledge of the way that ancient texts were copied, and the fact that scribes would be more likely to smooth over a difficult reading than to make it more complicated. If pressed further, the textual critic would supply evidence from historical manuscripts describing the practices of ancient scribes, and so forth. What would not be appealed to is the principle itself, in its own justification. It is unlikely, as this example shows, that warrant for a principle of informal inference will circle back upon itself *as a general rule*.

I have also suggested that some of the principles of informal inference from specific traditions of inquiry may be justified by appeal to noetic warrant. Are there not serious problems with such an appeal? Some philosophers may complain that supposed rational intuitions are merely arbitrary. One philosopher's intuition is another philosopher's nonsense. I have tried, above, to give good reasons why that need not be so. In particular, there

seem to be signs or symptoms of times when an appeal to reason is legitimate and to be expected. Moreover, the process of vindication would cut down on any arbitrariness. This objection loses its force when we open proposed noetic concepts up to revision and criticism. Other philosophers might complain that truths of reason are not subject to correction at all. Yet this complaint assumes we have a true, complete, and clear expression of the truth, and this may not often be the case. Our particular way of expressing our rational insight may need revision; or it may not turn out to be true at all, upon further investigation. We can only talk about proposed noetic concepts that have been subject to careful critique. These are fallible, however well established, and subject to further refinement. I should also make clear that what we are aiming for here is truth, not “agreement” nor “consensus” nor something “non-controversial.”

Finally, some philosophers seem to be afraid of anything that smacks of the postmodern, or the post-foundational. I should clarify, then, that I have rejected classical foundationalism. The alternative is not relativism; at least that is not my conclusion. I am open to a humble and modest form of “foundation” or ground for epistemic criteria.⁵⁷ Indeed, the concept of noetic warrant developed here could qualify as just that.

Back to the Rationality of Religion

Having spent so much time on the problem of induction, I think we are now ready to investigate the rationality of truth-claims in the great world religions. The quest for knowledge in both science and theology shares in common the general characteristics of informal reasoning we discovered in the philosophy of science and in general terms in sound informal argument. The principles of reasoning in religions will be fallible, traditional, diverse, and contextual. Yet here the context and tradition become very significant, as does the personal character of this quest for truth. The religious quest involves all that we are, including our feelings and our spirituality, as well as practices, morality, and rationality. There is no quest for the religious ultimate that does not involved the seeker in the very act of joining in the quest. The idea of an objective or value-free religious insight is a contradiction in terms. Because the religious quest is so complex, so self-involved on the part of the investigator and her community of faith, is it really so strange to find different cultures and contexts coming up with diverse sacred writings and understandings of sacred reality or God? For these reasons all theological truth-claims take place within diverse traditions of inquiry. Generic theology based upon common human experience and nature does not get us very far. And some progress can be made, and has been made, in the moral and rational critique of various world religions. Some religions, like classical polytheism, have been abandoned on rational grounds. But any developed understanding of sacred reality will demand moving beyond common human experience and reason. The great world religions are compatible with our experience, with reason, with science, and with most of our common sense moral beliefs. They all call us to insights and ways of life that lead us beyond the everyday world of nature, science, technology and common sense. It seems the world has been made in such a way that many religious faiths, or none at all, can be equally rational. Religious wisdom, therefore, is not irrational. It is just as valid a form of informal reasoning as the informal logic of specific sciences. But religious wisdom demands participation in longstanding and conflicting traditions of inquiry. Religious truth is not irrational, nor is it unscientific, but it draws the worshipper beyond nature and common

sense. The self-involving, faith-based religious quest for truth calls upon each of us to contemplate that which lies beyond and behind our everyday world.

Notes

- 1 Among many responses to Richard Dawkins, *The God Delusion* (New York: Houghton Mifflin, 2006), see John F. Haught, *God and the New Atheism* (Louisville: Westminster John Knox, 2008).
- 2 Witness, for example, this well-known passage from Hume:

When he [the Pyrrhonian skeptic] awakes from his dream, he will be the first to join in the laugh against himself and to confess that all his objections are mere amusement, and can have no other tendency than to show the whimsical condition of mankind, who must act and reason and believe, though they are not able, by their most diligent inquiry, to satisfy themselves concerning the foundation of these operations or to remove the objections which may be raised against them. (*An Inquiry Concerning Human Understanding*, § XII, part II, ed. C. W. Hendel. Indianapolis: Bobbs-Merrill, 1955, p. 168 f)

- 3 For a careful analysis of Hume's argument against induction, see D. C. Stove, *Probability and Hume's Inductive Scepticism* (Oxford: Clarendon Press, 1973).
- 4 I have been led to Reid by William P. Alston, among others. See for example his essay "Thomas Reid on epistemic principles," *History of Philosophy Quarterly* 2 (1985), 435–52.
- 5 H. E. Kyburg, Jr., *Probability and Inductive Logic* (New York: Macmillan, 1970), p. 126. See also P. Lipton, *Inference to the Best Explanation* (New York: Routledge, 1991), pp. 6–10.
- 6 For brief introductions to Reid's philosophy, see the article on Reid by Robert Gallie, in the *Routledge Encyclopedia of Philosophy*, 8, pp. 171–80, or Colin Brown, *Christianity and Western Thought*, vol. 1 (Downers Grove, IL: InterVarsity Press, 1990; Chinese translation published by Peking University Press), pp. 259–68. For Reid on induction, see Peter Anstey, "Reid on the problem of induction," *History of Philosophy Quarterly* 12 (1995), 77–93. For a fuller general discussion, see the excellent volume by K. Lehrer, *Thomas Reid* (Boston: Routledge, 1989).
- 7 See recently, James Cargile, "The problem of induction," *Philosophy* 73 (1998), 247–75, who makes a similar argument.
- 8 *Inquiry*, ch. 2, §6; reprinted in Reid's *Inquiry and Essays*, ed. R. E. Beanblossom and K. Lehrer (Indianapolis: Hackett, 1983), 20.
- 9 See the chapter, "The first principles of contingent truths," ch. 5, essay 6 of Reid's *Essays on the Intellectual Powers of Man* (ed. Beanblossom), pp. 266–84.
- 10 Reid articulates this principle in the chapter just cited above. I follow Alvin Plantinga in calling "warrant" that thing (or "quantity") which, when added to true belief, makes for knowledge. See his *Warrant: The Current Debate* (New York: Oxford University Press, 1993).
- 11 An argument is sound when the premisses are true and the inference is valid.
- 12 I thank Bruce Reichenbach for this term. See his *An Introduction to Critical Thinking* (New York: McGraw-Hill, 2000).
- 13 I assume that argument to the conclusion that p is false will be support for the truth of this larger proposition, "it is false that p ."
- 14 I borrow the term "classical foundationalism" from Alvin Plantinga. See the well-known collection of essays, ed. Plantinga and Nicholas Wolterstorff, *Faith and Rationality* (Notre Dame, IN: University of Notre Dame Press, 1983), quoting p. 72.
- 15 Princeton, NJ: Princeton University Press, 1979. For other criticism of classical foundationalism, see Fredrick Will, *Induction and Justification* (Ithaca, NY: Cornell University Press, 1974); or Plantinga and Wolterstorff, *Faith and Rationality*. A. J. Ayer rejected the view that something

- must be certain, in order to be known, in his *The Problem of Knowledge* (Harmondsworth, UK: Penguin, 1956).
- 16 In *Proofs and Refutations: The Logic of Mathematical Discovery* (Cambridge: Cambridge University Press, 1976), Imre Lakatos argued that even mathematics is based in the long run on informal reasoning. "An investigation of *informal* mathematics will yield a rich situational logic for working mathematicians, a situational logic which is neither mechanical nor irrational, but which cannot be recognized and still less, stimulated, by the formalist philosophy" (p. 4, his italics).
 - 17 Many of the points I am making here, about the nature of informal inference, follow the work of D. C. Stove, *The Rationality of Induction* (Oxford: Clarendon Press, 1983), and Fredrick Will, *Induction and Justification*.
 - 18 See, for example, Carl Hempel, "Recent problems of induction," in R. G. Colodny (ed.), *Mind and Cosmos* (Pittsburg: University of Pittsburg Press, 1966). This work in turn depends upon, and is critical of, the inductive criteria of Jean Nicod, *Foundations of Geometry and Induction* (New York: Harcourt, 1930).
 - 19 "The paradox of confirmation," *British Journal for the Philosophy of Science* 13 (1963), 265–277; reprinted in P. H. Nidditch (ed.), *The Philosophy of Science* (Oxford: Oxford University Press, 1968), quoting p.165.
 - 20 At least, that is the view many hold, including the present writer. See further Ernan McMullin, *The Inference that Makes Science* (Milwaukee: Marquette University Press, 1992).
 - 21 See further John Earman, *Bayes or Bust?: A Critical Examination of Bayesian Confirmation Theory* (Cambridge, MA: MIT Press, 1992).
 - 22 This would be true even if we agree with the Baysean that the objective, epistemic weight which evidence (*E*) gives a conclusion (*H*) is a necessary truth. When we are seeking justification for a particular informal inference, we are seeking human knowledge, that is, something that we are going to believe. In order for us to come to know the (relative) epistemic weight which evidence gives a conclusion (which is infrequently expressible in numerical terms), I need to understand the evidence and the conclusion. Only then can we judge the soundness of the argument.
 - 23 See, *inter alia*, Dennis Temple, "Grue-green and some mistakes in confirmation theory," *Dialectica* 228 (1974), 197–210 who argues that we may need "total information" to fully decide between grue and green as properties.
 - 24 Although Reid was a fallibilist in general, I am not sure he was a fallibilist with respect to first principles. For example, he wrote that "one of the surest marks of a first principle" is that "no man ever pretended to prove it, and yet no man in his wits calls it into question," *Intellectual Powers*, Essay 6, ch. 5, pt. 3 (p. 270).
 - 25 I do not claim that all types of foundationalism should collapse, just the classical variety.
 - 26 Such a conception is in the air these days: see, e.g., Donna Summerfield, "Modest a priori knowledge," *Philosophy and Phenomenological Research* 51 (1991), 39–66; M. Giaquinto, "Non-analytic conceptual knowledge," *Mind* 105 (1996), 249–68; or Noah Lemos, "Common sense and a priori epistemology," *Monist* 81 (1998), 473–87, and most recently Laurence Bonjour, *In Defense of Pure Reason* (Cambridge: Cambridge University Press, 1998).
 - 27 I am hardly alone in finding insight or intuition at the basis of rationality. Aristotle held that the basic truths (*arche*) upon which scientific or demonstrative reasoning was based were known through rational intuition (*nous*). See *Posterior Analytics*, II.xix (99B–100B). This was also Reid's position. See in more recent times, e.g., Bernard Lonergan, *Insight* (London: Longman, Green, 1957), or for induction, D. C. Williams, *The Ground of Induction* (Cambridge, MA: Harvard University Press, 1947), and Bonjour, *Defense*.
 - 28 I have been influenced in my thinking about noetic warrant and concepts by Hilary Putnam, "Analyticity and Apriority," *Midwest Studies in Philosophy* 4 (1979), 423–471; rpt. in *A Priori Knowledge*, ed. P. K. Moser (Oxford: Oxford University Press, 1987); and by S. Kripke, *Naming and Necessity* (Cambridge, MA: Harvard University Press, 1980).

- 29 In Moser, *A Priori Knowledge*, 104.
- 30 R. G. Swinburne, "Analyticity, necessity, and apriority," *Mind* 84 (1975), 225–43, also rpt. in Moser.
- 31 This point is made in Louise Marcil-Lacoste, *Claude Buffier and Thomas Reid* (Montreal: McGill-Queen's University Press, 1982), 117.
- 32 Paul Moser makes this point in Moser, *A Priori Knowledge*, 7–9.
- 33 This kind of exploration does not validate the noetic warrant, but it can vindicate it. See, on this distinction, H. Feigl, "De principiis non disputandum...?", in *Philosophical Analysis*, ed. M. Black (Ithaca, NY: Cornell University Press, 1950).
- 34 This procedure should satisfy Larry Laudan's complaint against all intuitionist meta-methodologies that an intuition is not subject to further correction. See his "Some Problems Facing Intuitionist Meta-methodologies," *Synthese* 67 (1986), 115–29.
- 35 *Inquiry*, ch. 6, §20, (p. 83).
- 36 Imre Lakatos argued this for research programs in the natural sciences, in *The Methodology of Scientific Research Programmes: Philosophical Papers*, vol. 1 (Cambridge: Cambridge University Press, 1977), while Alasdair MacIntyre makes a similar argument for first principles in moral philosophy, in *First Principles, Final Ends, and Contemporary Philosophical Issues* (Aquinas Lecture; Milwaukee: Marquette University Press, 1990).
- 37 I assume my opponent will agree that the meaning of a term comes from its use in a language-game.
- 38 Duhem, *La théorie physique* (Paris: Chevalier & Riviere, 1906), p. 218, cited in N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1961), p. 16. This work was translated into English by P. Wiener, as *The Aim and Structure of Physical Theory* (Princeton, NJ: Princeton University Press, 1954).
- 39 Hanson, *Patterns of Discovery*, 30.
- 40 S. Toulmin, *The Uses of Argument* (Cambridge: Cambridge University Press, 1964), p. 15.
- 41 Lakatos explains himself most clearly in his radio lecture for the Open University, published as "Science and pseudoscience," in his *Methodology*.
- 42 See most recently, Larry Laudan, *Beyond Positivism and Relativism: Theory, Method, and Evidence* (Boulder, CO: Westview Press, 1996), which contains frequent references to Lakatos.
- 43 See also Alasdair MacIntyre, "Epistemological crises, dramatic narrative, and the philosophy of science," *Monist* 60 (1977), 453–72, reprinted in A. MacIntyre, *The Tasks of Philosophy* (Cambridge: Cambridge University Press, 2006).
- 44 W. P. Alston, "A 'doxastic practice' approach to epistemology," in M. Clay and K. Lehrer (eds.) *Knowledge and Skepticism*. (Boulder, CO: Westview Press, 1989).
- 45 See his "Cura Prima," printed in Marcil-Lacoste, *Claude Buffier and Thomas Reid*, p. 197. In an unpublished letter, Reid noted, "That there may be innumerable self-evident Propositions I acknowledge; but the greater part of these will be found to be trifling propositions as Mr. Locke very justly calls them." Birkwood Collection, T. Reid 2131.6 (II) (6); cited in Marcil-Lacoste, 68 n.
- 46 This point, which is basically Wittgensteinian, has been made by many authors, e.g., Will, *Induction and Justification*.
- 47 Grue, you will recall, is a very complex color which is time-indexed. See Nelson Goodman, *Fact, Fiction and Forecast* (Cambridge, MA: Harvard University Press, 1955), p. 74. For a good defense and explanation of the principle of simplicity, see Richard Swinburne, *Simplicity as Evidence of Truth* (Milwaukee: Marquette University Press, 1997).
- 48 In other words, an inference that an explanation is "best" takes place against the background of rival hypotheses. See Dennis Temple, "The contrast theory of why-questions," *Philosophy of Science* 55 (1988), 141–151; Peter Lipton, "The contrastive explanation and causal triangulation," *Philosophy of Science* 58 (1991), 687–97 and his *Inference to the Best Explanation*, 32–55.
- 49 Of course, in the short run, it may well be quite useful.

- 50 From private correspondence with Charles Hughes concerning an earlier version of this essay.
- 51 "Eliminate all other factors and the one which remains must be the truth." See the beginning of the first chapter, entitled "The science of deduction," in Arthur Conan Doyle's story, *The Sign of Four*.
- 52 David B. Annis uses this name in a different way in "A contextualist theory of epistemic justification," *American Philosophical Quarterly* 15 (1978), 213–19. I discuss the view sometimes called contextualism under the rubrics of fallibilism and diversity, above.
- 53 T. Kuhn, *The Essential Tension* (Chicago: University of Chicago Press, 1977), p. 325.
- 54 T. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 108 f.
- 55 Kuhn, *Structure*, p. 204.
- 56 Kuhn, *Essential*, pp. 321–22.
- 57 Such as that defended by William Alston. See his *Epistemic Justification* (Ithaca, NY: Cornell University Press, 1989).

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