The theory of confirmation sketched herein is subjectivist in a manner that will be explained. According to it, however, the degree of confirmation of a hypothesis is an objectively existing relative frequency (or a propensity, if one prefers). The resolution of this apparent paradox is simple, but its implications are, I believe, profound. I am convinced that they provide the means for resolving all of the current "paradoxes," "riddles," and "puzzles" of confirmation and induction. This is an overstatement only if one assumes, contra hypothesis, that all is pretty much all right with contemporary theories of confirmation and that resolution of difficulties will consist only of a little patching up here and there. On the contrary, I argue, these "paradoxes," "riddles," etc., are legitimate reductios ad absurdum of most current confirmation theories, and their resolution requires the rather radical approach indicated by the first two sentences of this essay.

Since the solution that I advocate is radical, I begin with a rather detailed and elementary discussion of some of the crucial aspects of the problem, including a few of the more relevant historical ones. This will explain, I hope, the extremely refractory nature of the problem as well as the surprisingly small amount of progress that has been made toward its solution since it was posed by Hume and, finally, the necessity for the drastic measures that I have been led to propose. This discussion will be followed by a brief outline of the proposed contingent (though not "empirical") theory of confirmation that allows us to escape the skepticism of Hume. This includes a discussion of the nature of the prior (or

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1 As is emphasized at greater length below, this is in no way to disparage the crucially important though, in my opinion, mostly negative results of a host of tireless workers in the area, several of whom will be discussed presently.
antecedent, or initial, etc.) probabilities that must be used in estimating degrees of confirmation. I shall mention some of the philosophical and scientific implications of the failure of other theories especially those of empiricist persuasion and some implications of the theory advocated here. Finally, in an appendix, several current approaches to the problems of confirmation that were not previously discussed are considered.

It would doubtless be foolhardy to try to persuade most contemporary philosophers that this notorious problem, the problem of induction, or the problem of nondeductive inference, or the problem of confirmation or corroboration — term it however we may — the problem posed so poignantly by Hume, has occupied far too little of their attention. Most would probably reply that, on the contrary, it is one of those typical traditional philosophical problems which, in spite of interminable, ad nauseam discussion, remains as apparently refractory as when first considered. Therefore, they would continue, perhaps we should begin to suspect that it is not a genuine problem; at any rate, they would say, the very most that should be accorded it is benign neglect. Those who reject such a comfortable rejoinder and continue to take seriously Hume’s critique do constitute a substantial minority. They include most if not all of the contributors to this volume as well as such luminaries as Carnap, Feigl, Feyerabend, Grünbaum, Hempel, Lakatos, Popper, Reichenbach, and Russell, to mention (alphabetically) only a few. An attempt to give a complete list would be tiresome and inevitably invidious anyway, and, certainly, some whose names are not included here have done work as extensive and important as that of those whose are. However, the names of Strawson, Edwards, and others who support the “ordinary-language” solution (or dissolution) of the problem are advisedly omitted because they arrive, quite honestly, of course, and apparently to many quite persuasively, at one variety of the “comfortable rejoinder” position mentioned above.

The view for which I shall argue is that few if any of us, excepting, possibly, Hume himself, have hitherto realized the enormity of the import of the problem, not even those of us who have devoted a large portion of our philosophical lives to it. What is at stake is not just a theory of evidence or confirmation, although this certainly is at stake and is a vitally important matter. But also in question is the basis, indeed the very nature, of epistemology and of knowledge itself. Involved are radical and pervasive implications concerning scientific methodology, the very nature of science and of philosophy as well as results that illuminate
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specific philosophical and scientific issues such as realism vs. phenomenalism (or other varieties of idealism, or instrumentalism, or operationism, etc.), the alleged line(s) of demarcation that separate(s) science from nonscience (or from metaphysics, or from pseudoscience, etc.), "empiricist meaning criteria," and the mind-body problem. In this paper I shall try to support, clarify, and explain some of the details of the prima facie extravagant and dramatic claims just made. (I have discussed some of these matters in other places.)

Hume's problem is, I contend, insoluble. To qualify this by saying that it is insoluble on empiricist grounds borders on redundancy, for it is a serious problem only for empiricists. Nonempiricists can circumvent it or dispose of it in a number of ways. Some might declare a set of principles of induction or of nondemonstrative inference to be valid by synthetic a priori warrant, while some would hold that statements of the basic regularities of the universe can be known directly and are themselves synthetic a priori; or one may take the position for which I shall eventually argue and accept a more relaxed, more realistic view about the nature of knowledge and the standards one should employ for its certification.

Such a "relaxed" position is not one that I have taken easily. Honest empiricism is certainly more than just a philosophical fad or fashion. Prima facie, it is both plausible and attractive, and the epistemological motives for holding it are not only understandable but, at least some of them, eminently admirable as well. Moreover, most philosophers, including myself, accept it as a truism that such developments as the applications of non-Euclidean geometry and indeterministic theories in physics, etc., have forever refuted Kant's reply to Hume, as well as all of the more drastic varieties of rationalism.

What indeed could be more reasonable and desirable than to build the entire edifice of knowledge using only bricks and mortar consisting

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of the two and only two kinds of knowledge claims that are pretty much secure and unproblematic: (1) singular propositions known by direct observation (whatever we take direct observation to be) and (2) propositions certifiable solely on the basis of the principles of logic (or of "pure" mathematics). And since, it would seem, the items in both of these categories can be known objectively, or, at least, intersubjectively, this highly prized property will be exemplified by any and all portions of the edifice. Our knowledge, fashioned then from these two impeccable materials, might not, at any one time, be perfect or certain, but it could be objectively certified as the best possible, given the bricks (evidence) available at the time.

To switch metaphors, such an epistemology and the methodology to which it gives rise are ideal for warding off or for chastising subjectivists, metaphysicians, takers on faith, and any unscientific charlatans that threaten to clutter up the philosophical, the scientific, and, even, in principle at least, the practical scenes. As one raised (philosophically) in the empiricist tradition, I speak after many years of experience and from the heart. To have become convinced that empiricism is gravely defective, both in letter and in spirit, has been a painful thing indeed. On the other hand, the anguish has been considerably mitigated both by the intellectual excitement of beginning to see definite answers, if somewhat negative ones, to the notorious problem of Hume and by the intellectual freedom made possible by shedding the epistemological straitjacket that empiricism turns out to be. Such freedom is, of course, one of the very things that, as empiricists, we feared from the beginning. Does it not open the floodgates to almost any sort of crackpotism imaginable? I shall say more on this later; but be that as it may, if empiricism is untenable, it is untenable. It is not a matter for legislation, no matter how nice it might be if only it were viable. To those, including a part of myself, who are still inclined to mourn its moribundness, I can only paraphrase (once more) Bertrand Russell and say that I am sorry, but it is not my fault.

Such rhetoric, I realize, risks offense or, at least, stylistic criticism, and certainly to some it will seem that I put my thesis in an extreme and unnecessarily provocative fashion when it becomes, in effect, the assertion of the poverty of empiricism. But I believe that continued

investigation and reflection will vindicate my so stating it, if, on nothing else, on didactic and pedagogical grounds. The results that contemporary studies in confirmation theory thrust upon us are extreme. Indeed, they are much more drastic, I think, than even most of the involved scholars have realized—so drastic that what at first seems to be an extreme assertion may well turn out to be an understatement.

To turn now from rhetoric and to the business at hand: What is empiricism, and what is its stake in the problem of induction? When I speak of empiricism here, I am concerned only with what some have called "judgment empiricism," which is a doctrine on how knowledge claims are "validated," confirmed, supported by evidence, or otherwise certified as being real knowledge, or probable knowledge, etc. In other words judgment empiricism is part of a theory of confirmation. Using the same terminology, concept empiricism is part of a theory of meaning, is logically independent of judgment empiricism, and will not be our concern here at all; hereafter "empiricism" will mean judgment empiricism unless otherwise stated. As a first approximation, we may begin by saying that empiricism is the doctrine that our (contingent) knowledge claims can be confirmed only by (1) observational knowledge, plus (2) logic. (I shall take the notion of observational knowledge to be unproblematic here, although of course it is not; however, I believe that what I have to say can be easily adapted to fit into almost any theory of observation.) The immediate problem is to decide what should be included under "logic." In addition to deduction, Hume taught us that some forms of nondeductive inference (or nondeductive logic, many would say) must be classified as legitimate if our knowledge is to go beyond observations of the moment. Empiricists, however, understandably have wanted the word "logic" to retain as much rigor and "toughness" as possible here. More specifically, it is vital for empiricism that even the nondeductive or "inductive" logic be, in some important sense, free of contingent elements. Whether this is possible and, if so, in just what sense, is, of course, the nub of the problem; but why empiricism

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5 I am assuming here that the principles of deductive logic are unproblematic. This was, I believe, an assumption of Hume and certainly is one of most contemporary empiricists; and the criticism of empiricism in this essay is, in the main, internal criticism. Moreover, I am convinced that this contemporary empiricist philosophy of deductive logic is essentially correct.

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must impose this condition has been obvious ever since Hume: logical
principles are universal principles and for this (and other) reasons can-
not be known directly by observation (or "experience") and, therefore,
if any one of them were contingent it would be just as much in need of
validation, confirmation, etc., as the most mundane "empirical" general-
ization. And, of course, any contingent generalization requires nondeduc-
tive inference for its support, and we are immediately caught in Hume's
vicious circle of "using induction to justify induction," etc. Empiricists,
of course, have recognized and even insisted on this point, but I think
it is fair to say, also, that they have required that logic, even "inductive
logic," retain whatever rigor and toughness that derives from the follow-
ing: (1) Analogously to deductive arguments owing their validity to
their purely formal properties, legitimate nondeductive arguments owe
whatever legitimacy they have to their purely formal properties: for ex-
ample, one of the allegedly legitimate arguments, induction by simple
enumeration, always has (something like) the form \( x_1 \) is \( A \) and \( x_1 \) is \( B \),
\( x_2 \) is \( A \) and \( x_2 \) is \( B \), \( x_3 \) . . . , . . . , therefore everything that is \( A \) is \( B \).
(2) The legitimate forms of inductive inference (and only these forms)
are legitimate in every possible world; i.e., they provide the "rational"
way to infer nondeductively, or the "best" way, or the best advised way,
etc., for every possible world, although, of course (being nondeductive)
they will fail in some possible worlds.

These two requirements are, obviously, pretty tough. However, it is
difficult to see how they can be significantly relaxed without reopening
the gates to Hume's devastating critique. Even something so apparently
innocuous as the "principle of total evidence," to which most inductive
logicians (and others) subscribe, introduces immediate trouble. On the
one hand its use seems necessary for avoidance of certain intolerable "in-
ductive paradoxes"; but, on the other, it seems highly questionable
whether it is a formal principle and, certainly, there are possible worlds
in which knowing subjects make much better inferences when they ig-
nore a certain portion of the evidence that they happen to have at hand.\(^6\)

Let me pause here to beg indulgence for laboring elementary points in
such an elementary fashion — points many of which were made by Hume
in a more elegant, more persuasive manner and points, moreover, that

\(^6\) Whether they would be well advised to do so and, if so, under what (specifiable?)
conditions is, of course, arguable. But this is just my point; it is arguable and, thus,
questionable in both directions.

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are made by most contemporary philosophers and methodologists who write on these problems. It does seem to me, however, that many such philosophers and methodologists, after explaining these elementary difficulties with persuasive clarity, then proceed to forget, ignore, or repress some of them at crucial junctures in the developments of their own "solutions" to Hume's problem. When we recall again, however, that it is the very life of empiricism that is at stake, such repeated instances of apparent perversity in intellectual history become much more understandable and easier to condone. Indeed it seems very nearly certain to me that the numerous and persistent attempts to "justify induction" or otherwise dispose of the problem will, in the long run, turn out to produce invaluable benefits in several areas of philosophy and, especially, in scientific methodology. The ingenuity, originality, and technical competence exemplified in many of these studies not only are intrinsically admirable but have produced results of lasting importance for other areas of thought as well as for the one of explicit concern, no matter what its future courses of development may be. In the meantime, however, given the current chaotic situation, I do not see any way to avoid continuing to reiterate some of Hume's basic insights.

One of the most ingenious and best known attempts to rescue empiricism from Hume is due to Herbert Feigl and Hans Reichenbach. Although they were acquainted and were most congenial, both personally and philosophically, with each other, their initial work on this was, as far as I can determine, begun independently; and it used similar but distinctly different approaches. In an article first published in 1934, Feigl contended that the principle of induction is not a statement but, rather, a rule. Thus, it is neither true nor false and, a fortiori, it is not contingent (or necessary). He felt that this enables inductive logic to avoid contingent principles. Nevertheless, the rule must sanction ampliative (non-deductive) inferences and therefore, as he recognized, it stands in obvious need of justification, since an unlimited number of alternative rules giving mutually incompatible results can be formulated. He also recognized, at least implicitly, that such justification could not be accomplished solely by means of (other) rules alone but must contain propositional elements, elements that were either contingent or neces-

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sary. This would seem to run the empiricist afoul of Hume again, for if the justificatory propositions are contingent we are immediately caught in Hume's circle, and if they are necessary, then, given the empiricist philosophy of logic (and of necessity), which is not in question here, they are logically true (or at least analytic) and, thus, factually empty. This would seem to render them impotent insofar as selecting among different rules that give different sets of contingent results, each set incompatible with every other one. But Feigl, at the time, seems to have disagreed. He thought that a tautologous justification could select our "normal" inductive procedures as being the best advised kinds. He admitted that it was, in a sense, a trivial justification, but it was, he thought, nevertheless illuminating. And its very triviality is, in a way, its great virtue, for it enables us to avoid the fatal introduction of contingent elements. As far as I know, this was the first of a number of interesting attempts to give deductive or analytic justifications of induction.

In a later, justly renowned article, Feigl made his famous distinction between validation and vindication, the two kinds of justification.8 Surely this is a very familiar matter, so I shall only say enough about it here to help set the stage for what is to follow. In a vindication, what is justified is an action, or a procedure, or a rule that sanctions a procedure or a course of action. A successful vindication is one that shows that the action or rule, etc., is one that is well advised relative to the goal or aim of the action, procedure, or rule. Preferably, the vindication will show that the procedure, etc., in question is a good method for attaining the goal desired and, most preferably, that it is the best method. Feigl, and soon others, hoped and evidently believed that induction (our "normal" inductive procedures) could be given such a vindication relative to our goals of obtaining knowledge of laws of nature, prediction of the future, etc.

Note that in talking about a good method and the best method an evaluative component is introduced in addition to the necessarily evaluative one connected with selecting final goals such as predicting the future, obtaining general knowledge. This might seem to open the way for troublesome complications. For example, someone might have such

an overwhelming preference for very easy methods that for him, armchair guessing would be the “best method” even if he could be persuaded that its chance of predicting the future, etc., was only a small fraction of what it would be if he applied, say, “Mill’s Methods.” A vindication, then, seems to be relative not only to a goal but also to preferences, or criteria, etc., concerning intrinsically the course of action itself. Once these are stated, or once such criteria are agreed upon, then what is a good method or the best method can be treated as a purely cognitive question; and the premises used to support such a vindicatory argument will all be propositional, true or false, and, thus, either necessary or contingent. Thus, while it is undoubtedly important and certainly true to point out that inductive procedures require vindication rather than validation, it is not at all clear how the shift from the latter to the former alters the situation one whit as far as Hume’s problem is concerned. Nevertheless, Feigl and others certainly appeared, again, to hope and believe that the truth of the cognitive proposition that induction (“our” kind of induction) satisfies the criteria for a “good” method or, even, the “best” method for predicting the future, securing general knowledge, etc., could be (deductively) demonstrated. To those unkind enough to remark that this is an obvious attempt to transform induction into deduction, there is a ready reply: No, if we succeeded in such a vindication we would not have shown that induction is always truth preserving as, of course, deduction is; we would have shown only that our inductive procedures provide us with a good (or the best) method for discovering those nondeductive inferences that lead from true premises to true conclusions although they will sometimes fail us in this. But after such a modest and apparently impeccable retort, Fate herself becomes unkind. Much more difficult obstacles arise when one attempts to give acceptable criteria for “good method” or “best method,” or, even, for “a method.” “A method” can certainly not be taken to mean a method that will attain the goal (of predicting, etc.) or even one that is likely to succeed. Such criteria cannot be known to hold for any method; the shade of Hume will not be denied. Coming up with criteria for “good” or “best” methods is beset with similar, even greater difficulties.

On the matter of induction, philosophers have been ready to settle for considerably less than half a loaf. There has often been much rejoicing and, perhaps, excessive gratitude over whatever crumbs may seem
to be falling from the table. For example, when it was realized that we could not vindicate (normal) induction by showing that it was the best method (using nonperverse criteria of "best") since there could easily exist other (abnormal) procedures which might turn out to give better predictions, etc., Reichenbach opted for a weaker but ingenious kind of vindication. (He used the term "pragmatic justification" rather than "vindication.") According to him, we can show that if any method will succeed, induction will succeed (or, contrapositively, if induction fails, then all other methods will fail). Now this still sounds pretty strong. The weakening, the nature and the handling of which constitute a great deal of the ingenuity, is twofold. First, the goal of induction is grossly weakened in a manner to be discussed; here I shall just note that it is no longer stated in such general terms as we have been using and that successful prediction, in effect I shall contend, is no longer a part of it. Second, it turns out that Reichenbach really means that if any method will succeed, then induction will succeed eventually (or if induction fails in the long run, then all other methods will fail in the long run); and it also turns out that eventually and in the long run may, insofar as the vindication goes, both turn out to be, literally, forever. This would be of little consequence in the epistemology of God or Omniscient Jones but for the rest of us it could mean a total epistemic debacle.

As we all know, the more modest goal that Reichenbach sets for induction is the determination of values of limits of relative frequencies in certain ordered classes.\(^9\) Reichenbach provides us with a method (actually a family of methods) which, if applied long enough, is deductively guaranteed to produce the correct value of the limit provided a limit exists. Again, this sounds impressive; if true, it would seem to provide us with the coveted deductive vindication of induction. Its defects, however, are well known. A sympathetic but thorough and incisive discussion of them is, again, provided by Wesley Salmon, a former student of Reichenbach and one of the outstanding figures in confirmation

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\(^9\) For anyone not familiar with this approach—or for anyone at all, for that matter—an excellent source of information is the work of Wesley Salmon, some of which will be discussed here. See, for example, his *Foundations of Scientific Inference*, Pittsburgh Paperbacks (Pittsburgh: University of Pittsburgh Press, 1966); reprinted from Robert G. Colodny, ed., *Mind and Cosmos*, *Pittsburgh Studies in the Philosophy of Science*, vol. 3 (Pittsburgh: University of Pittsburgh Press, 1966).
Nevertheless, a brief discussion of some of them here is necessary for reasons similar to ones given above.

First of all, as already hinted, as far as the deductive vindication goes, there is absolutely no reason to suppose that the "long enough" qualification (also signaled above by "eventually" and "in the long run") will not stretch into forever. Thus, as far as the vindication takes us, there is absolutely no reason to suppose that Reichenbach's method will ever provide any knowing subject with a value that is anywhere near the correct value, unless he assiduously applies it until, as Carnap once said, he reaches the ripe old age of infinity. Moreover, the vindication provides no reason for doubting that there are other methods, incompatible with Reichenbach's, that will provide much better estimates of the relative frequency unless, again, they are applied to portions of the sequence which are, for us, impossibly large; and this holds not only for values of the limit but, and this is the practical payoff, for values of the relative frequency for any unobserved portions of the sequence. This brings to mind another fatal practical defect of the vindication; even if the limit is known, there is no deductive relation between it (in either direction) and the value of the relative frequency in any finite portion of the sequence, so that, for example, the true value of the limit could do nothing, as far as the vindication is concerned, toward predicting frequencies in finite classes (the notorious "problem of the short run"). As just mentioned a "perverse" method might well be free of this defect, again, as far as Reichenbach's vindication is concerned. Thus for practical purposes such as prediction, and even for a practical method of getting a good estimate of the limit, the vindication is no vindication at all. For something labeled "the pragmatic justification of induction," this seems amiss.

There are other important difficulties with Reichenbach's program, but those given above will suffice here. The reader is again referred to Salmon, whose attempts to give a vindication of induction (specifically, a vindication of Reichenbach's "straight rule") that avoids the errors but retains the spirit of Reichenbach's work have been valiant, indeed. I have learned a large portion of whatever I know about confirmation

10 See *ibid.* and the references therein.
11 The *straight rule* enjoins us to use the value of the relative frequency determined for observed portions of the sequence as the estimate of the value of the limit: e.g., if one one-thousandth of observed opossums have been albinos, infer that one one-thousandth of all opossums are albinos.
theory from his admirable work, and the value of its by-products, especially the critiques of other approaches to confirmation theory, is beyond estimate. The attempts themselves, however, I believe to be doomed to failure for general reasons that I shall give presently, so I shall not discuss them specifically now.

Before leaving the subject, it will be instructive to look at a general moral to be drawn from Reichenbach's attempt. His deductive vindication of the straight rule amounts to the triviality that, if values of the relative frequency in a sequence do converge to a limit and if the value of the frequency in the sequence up to the point of convergence is determined (by observation of members of the sequence up to this point), then this determined value will be the value of the limit (plus or minus $\epsilon$, for any $\epsilon$) or, in simpler language: if the frequency converges to a limit and we observe the value at the point of convergence, then we will have observed the value of the limit (although, of course, even when, if ever, we do succeed in observing the value of the limit, we never know that it is the limit). It is difficult to believe that anything very significant — anything that is, even psychologically or therapeutically speaking, news — could follow from such a triviality, let alone something that is supposed to rescue empiricism from Hume's formidable skepticism.

In propounding their philosophy of logic (and their theory of necessity which they develop from it), empiricists have emphasized, rightly in my opinion, that from trivialities only trivialities follow. How, then, can one account for the curious lapse whereby many of them have hoped to dispose of the justly celebrated problem of induction by employing a few logical and mathematical trivialities? Well, for one thing, we have noted that the defining postulates of empiricism seem to preclude any other kind of solution. But apart from obvious human frailties such as wishful thinking and hope springing eternally, I believe that there is a more charitable explanation deriving from a more subtle factor, one that, again, was clearly anticipated by Hume. We are constitutionally so irresistibly disposed to employ certain "normal" modes of nondeductive reasoning that we often do so tacitly, implicitly, perhaps unconsciously — call it what you will. This built-in predisposition and prejudice may operate in at least two significant ways: (1) it may shed a spurious plausibility on attempts to justify such modes of inference and (2) it
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may subtly introduce an unnoticed circularity of the type exposed by Hume, whereby induction is “justified” inductively.

Reichenbach’s straight rule, for example, is so congenial with our inductive animal nature that, when he assures us that, in spite of Hume, if we only apply it long enough it is bound to produce the desired results if such results are attainable at all, we breathe a sigh of relief. We felt all along that it must be so; how nice that Reichenbach has been able to prove it! And when the full chilling force of the qualification, “long enough,” finally strikes us, we may feel that, anyway, it was a nice, plausible attempt and that perhaps the matter can be remedied by adding another triviality or two to the one used by Reichenbach. (Note that I have not claimed that either our inductive predispositions or the straight rule is “wrong,” but only that neither has been deductively vindicated.)

As for the circularity, consider the following example. It is often claimed that, no matter what nondeductive procedure we might employ, we would and/or should test the procedure itself by “normal” induction, by, as a matter of fact, something like the straight rule. Suppose we employ a certain rule R, let us say, to make predictions. After a number of applications, we would and/or should note the number of successes and the number of failures of R. If it has been always or often successful, we would and/or should infer that it will continue to be always or often successful — that it is a “good” rule and if mostly unsuccessful that it is a “bad” one. This, the claim continues, shows the primacy of the straight rule and, it is sometimes added, again, shows that if any rule succeeds so will our normal inductive procedures such as that given by the straight rule. Again, this may sound very plausible initially, but, after the forewarning above, the circularity must now be obvious. It is, no doubt, a psychological fact that we would test R in such a manner; but this shows absolutely nothing of significance, except, perhaps, that Hume was right in claiming that we do have virtually irresistible animal (or other kinds of) impulses to use “normal” induction. And I do not see what warrant there is for claiming that we should so test R unless we assume what is at question: that “normal” induction or the straight rule does have primacy — is legitimate, etc. — for making nondeductive inferences, whether about successes of rules or about other kinds of entities. It would seem that the only things vindicated by this prima facie seductive line of thought turn out again to be the remarkable ruminations of Hume.
Another feature of the preceding example leads into what I consider to be the clincher of my argument. Such a test of R by using induction by simple enumeration or the straight rule is demonstrably worthless unless it is bolstered by very strong additional assumptions. For there will always be an infinite number of rules each of which gives results identical with those of R for all applications to date but which will give results incompatible with those of R (and with those of each other) for future applications. Thus, if such testing favors R, it will favor equally well an infinite number of other rules incompatible with R (and with each other). This can be easily proven deductively, as can be the following, related theorem: 12 For each simple inductive argument with true premises and true conclusions, there exists an indefinitely large number of inductive arguments each with the same logical form as the first and each with all premises true but with conclusions incompatible with that of the argument in question (and with those of each other). The phrase ‘simple inductive argument’ is used here in a technical sense to include induction by simple enumeration and its generalization into the “straight rule,” most other commonly encountered statistical inferences, “eliminative induction,” “Mill’s Methods,” and other similar “normal” inductive procedures. Thus, statistically speaking, as Russell has put it, induction leads infinitely more often to false, unacceptable results than it does to true or acceptable ones.

Now it is tempting to claim immediately that this demonstrates the worthlessness of all such forms of “normal” induction, for, even if Reichenbach or someone else “vindicated,” say, the straight rule, he would have merely vindicated, for any application, an indefinitely large family of possibilities only one of which could do us any good. 13 But while the second main clause of the preceding is true, it does not follow that the first is also; and it should immediately be acknowledged that interpretation of this rather spectacular theorem requires a large measure of restraint by “anti-justificationists” such as myself, a measure perhaps

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13 The existence of such a kind of family is acknowledged by Reichenbach and has received extended treatment by Salmon. It is, however, a different family from the one under consideration now. For example, it is not generated by the straight rule alone but by a family of rules, only one of which is “straight.”
already exceeded, even, by the remark just attributed to Russell. I shall not claim, for example, that it has thus been shown that there is on a given occasion a zero or only a very small probability of hitting upon a good inductive argument out of the indefinitely large number of mutually incompatible possibilities. To do so would be to assume, tacitly, some kind of principle of indifference; and apart from the general difficulties with such a principle, I believe, as will appear presently, that such an assumption, in this case, would be false. The correct position seems clearly to be that any assumption about the distribution of probabilities of success among the members of the family of mutually incompatible alternatives is a contingent assumption. It may be a true assumption, for example, that those applications of the straight rule that human beings employ have a much greater probability of success than other possible applications. As a matter of fact, I believe that it is true and of some importance, although I do not think that the straight rule has the essential role attributed to it by Reichenbach. The crucial point at hand now is that, without some such assumption, there is no satisfactory reason to suppose that any simple inductive inference will ever give us knowledge, where “satisfactory reason” means reason from a deductive vindication. The Reichenbachian may reply that this is not disturbing and that it would have been true even if Reichenbach’s vindication had accomplished all that he hoped. But the heart of Reichenbach’s attempt was to give us a unique, specified procedure that would succeed if any method would; and this program has collapsed, at least temporarily. An alternative application of the straight rule might well have succeeded where the one we selected failed. Now, I do not know how to prove that there is no deductively vindicable procedure that will provide a unique kind of application of simple inductive procedures—for example, a kind that would succeed if any other would. I am strongly convinced that no such vindicable procedure exists, and I am inclined to believe that its non-existence can be proved, remembering earlier considerations about nothing but trivialities following from trivialities, etc. But I am not too concerned that no such proof has been discovered, nor am I especially motivated to look for one. Proofs about such matters that are apparently rigorous can often be evaded by tactics such as somewhat modifying the conclusion—for example, a slightly different kind of vindication or a

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15 I am using ‘probability’ in the sense of relative frequency (or propensity).
somewhat different kind of "inductive" procedure might be proposed. Even such a celebrated proof as that of the impossibility of trisecting an angle using only compass and straight edge has to be "protected" by very precise and restrictive specifications on what is to count as a legitimate use of compass and straight edge. One of Mary Lou's (my wife's) psychotherapy patients (John C. Gustafson, a sane and intelligent person if I ever knew one) showed us a method whereby one may, by rapid and easily applied successive mutual adjustments of compass and straight edge, trisect an angle, approximating the accuracy of any "legitimate" use of compass and straight edge as closely as one pleases. Could one only do as well for vindicating induction, the problem would be solved. So, even if impressive impossibility proofs about vindication were discovered, I doubt very much that they would suffice to convince a confirmed vindicationist that his task is even as difficult as trisecting the angle or squaring the circle.¹⁶

Before leaving simple inductive inferences for other nondeductive procedures, consideration of a matter connected with Goodman's "new riddle of induction" may help to round out the discussion. Goodman's "pathological" predicates provide merely one among a number of ways of proving the theorem about the multiplicity of mutually incompatible inductive inferences discussed above. (More accurately, such predicates generate one of a number of infinitely large families of mutually incompatible inferences.) I have given such a proof,¹⁷ using, instead of Goodman's 'grue', 'bleen', etc., perfectly general predicates free of any explicit or tacit time dependence or other incorporation of individual constants. Wesley Salmon has attempted to provide a selection rule that eliminates all but one member of the family of mutually incompatible inferences that results from such predicates.¹⁸ He imposes a very strong condition that restricts such inferences to the use of "ostensive" (direct observation) predicates. Thus predicates such as 'red,' 'green,' 'warm' could be used in arguments by simple enumeration, but ones such as

¹⁶ I am indebted to Keith Gunderson for the analogy between justifying induction and squaring the circle. This, in turn, brought to mind our friend's trisection of the angle. For a fascinating account of other geometrical proofs, proof evasions, new "strengthened" proofs, new proof evasions, etc., etc., see Imre Lakatos, "Proofs and Refutations" (in four parts), British Journal for the Philosophy of Science, 14 (1963), 1–25, 120–39, 221–45, 296–342.

¹⁷ "Theories, Perception, and Structural Realism."

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'grue', 'bleen', and, presumably, 'diamagnetic' and 'ultraviolet' could not. Apart from the difficulty this would seem to create for many widely employed scientific inferences, Salmon's proposal has been criticized as being arbitrary and ad hoc. I am suspicious of criticisms that depend mainly on the use of the epithet "ad hoc," but it seems undeniable to me that such a strong requirement, especially when it has such serious consequences, is as much in need of vindication as the original inductive inference forms themselves or more so. It is true that Salmon's rule would remove the outrage to our inductive intuitions that is perpetrated by Goodmanesque, "pathological" predicates. But, again, surely Hume teaches us that, if inductive intuitions sufficed, our normal inductive procedures would have stood vindicated from the onset, and all the toil and sweat of Reichenbach, Feigl, a host of others, and especially Salmon himself would have been attempts to crash through open doors. It seems to me that, on his own grounds, Salmon's rule must at least fulfill Reichenbach's requirement: it must be shown that the kind of inference it selects will, in general, succeed if the kind exemplified by any other member of the family in question will. This may be true, but I do not believe that it can be demonstrated. In fact, whether or not it is true seems, certainly, to be a contingent matter. Surely there are possible worlds in which the color of emeralds changes suddenly at times and in which plumage pigmentation is a function of the period of rotation of the planet of habitation.

Is it, then, arrant anthropomorphism to suppose that, in the actual world, there is something very special about properties and features that happen to attract our attention and hold our interest or, even, those that our particular perceptual facilities enable us to observe directly, or something special about the expectations that such properties arouse in us and the kinds of inferences in which we are disposed to use these properties, something so special that our expectations are fulfilled and our inferences are successful a surprisingly large portion of the times?


20 Goodman's restriction of inductive inferences to the use of "entrenched" predicates is, of course, just as arbitrary and just as much in need of justification as Salmon's rule, but I am sure that this does not disturb Goodman.
Yes, outrageously anthropomorphic as far as logic is concerned, including, I maintain, any kind of “inductive logic” that can be vindicated deductively. Nevertheless, to peek ahead at the happy ending, I am going to maintain that, as a matter of contingent fact, the world does have these happy characteristics. Salmon’s restriction to directly observable properties is much too restrictive, but otherwise, his (contingent!) intuitions were in the right direction: there is (contingently) something special about those properties of the world about which we concern ourselves and about the regularities in which our predictions or projections (more properly, our theories) locate them. This is why knowledge is possible. True, this can be a happy ending only for those who abandon the empiricist goal of justifying all knowledge claims by means of observation and logic. For I certainly cannot deductively vindicate this very strong contingent assertion. Whether we are in any way justified in “believing” it is a matter to be discussed later in the paper. Even now, however, it is important to remark that these contingent assumptions do not provide a “method” or a specified mode of nondeductive inference. Those seeking escape from Feyerabend’s “methodological anarchy” will find scant comfort here.\(^{21}\)

It is now time to remind ourselves that the predominant mode of reasoning both in everyday life and certainly in science is hypothetico-deductive, although the simple inductive modes discussed above are used at times and are of some importance. This is generally acknowledged today even by most empiricist philosophers of science although the knowledge has not yet trickled down to those who set the most popular fashions in methodology for psychology and the social sciences. The mistaken belief that what I have been calling “simple inductive procedures” provide the only, or at least the predominant, or at the very least the fundamental, means of confirming significant knowledge claims has been responsible for a large number of the many dead ends reached in work on induction and confirmation as well as for much of the grief suffered in general scientific methodology and other areas of philosophy. For this error fosters the view that the accretion of knowledge, both in common-sense contexts and in “scientific” ones, proceeds according to a set pattern, a pattern consisting of the collection of (homogeneous,

observational) data or evidence which, when subjected to the appropriate simple inductive rule, automatically and mechanically leads us to the correct (and _eo ipso_, thusly confirmed) knowledge claim; for example, the evidence, consisting of the observation of ninety-six black crows (and no nonblack ones) is properly handled by induction by simple enumeration (or the "straight rule") and, thus (inductively) confirms and, moreover, leads us univocally to the one proper conclusion that all crows are black (or, if we are interested in singular conclusions, say, for predictions, to conclusions that the next crow will be black, the next ten crows will be black, etc.). We have already seen that no such univocality exists, but that is not the point of interest now. The important thing is that, in the more usual case, our data are not homogeneous nor do we proceed according to the simple inductive rules. Our data might consist of a number of different aspects of the behavior of gases, or a number of different properties of a newly prepared chemical compound, or a number of different clues connected with a certain crime. In each case, even if we wished to apply a mechanical (inductive) inference rule, none is available. What we do, in each case, is to try to dream up a theory or hypothesis which if true would _explain_ the data, where "explain" means to infer from a group of propositions some of which are general in character. The appropriate theory for the first example we believe to be the kinetic theory of gases, in the second case our theory would be the (tentative) assignment of a certain molecular structure to the new compound, and in the third case we would propose a singular hypothesis about who committed the crime, how it was accomplished, etc. (Let us note, even at this point, what I shall emphasize later, that, in each case, our proposed theory or hypothesis will be one among an indefinitely large number each of which will "explain" (imply) the data equally well, as far as logic is concerned.) In each case, we would probably want to claim that the data constitute evidence that confirms the theory or hypothesis in question by virtue of the _fact_ of the data being explained by

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(interable from) the theory or hypothesis. It should be remarked that the (explanatory) inference from theory (or hypothesis), even when the appropriate auxiliary theories, initial conditions, etc., are conjoined, to data is sometimes statistical or otherwise nondeductive. Therefore, ‘hypothetico-inferential’ is a better, less misleading term than ‘hypothetico-deductive’.

Although lip service is quite generally paid to most of the important features of this mode of reasoning, many of them are quite often forgotten or ignored. For this reason, I have to apologize again for explaining the obvious both above and in some of what is to follow.

First of all, the necessary prevalence of the hypothetico-inferential mode of reasoning accounts for the obvious “fact” that significant progress in the pursuit of knowledge is difficult and involves creativity. If “scientific method” consisted of using simple inductive inference rules, then all one would need to do is make observations to provide the data simply by looking or perhaps by performing experiments and then mechanically apply the appropriate inductive rule in order to reap, automatically, the correct (or most “rational,” etc.) conclusion. In the usual case, that is, the case involving hypothetico-inferential reasoning, there is no rule to take us from the data (more generally, to take us from the problem at hand) to the theory or hypothesis. The proposal of the theory — often a humble singular hypothesis, even — is, as Einstein once said, a free, creative leap of the mind. And, again, although this is generally acknowledged (not by everybody, however) and is perhaps obvious, we should note that, from it, it follows that there is no such thing as scientific method or inductive logic if either of these is supposed to produce out of the data at hand the “correct conclusion” (or the “most rational,” “best advised,” etc., conclusion). There are many “Yes, but —” replies possible to this point, some of which will be considered later, but, for now, I shall be content if granted the point itself.

As far as the justification or vindication of hypothetico-inferential reasoning as a mode of confirmation or as a means of nondeductive inference is concerned, it is in pretty much the same boat as simple inductive procedures. The crucial point is that, given any amount of data or evidence whatever, there will always be an infinitely large number of mutually incompatible theories or hypotheses from which (together with appropriate conjuncts of auxiliary theories, etc.) the data may be inferred and which, therefore, as far as the logic of the situation is concerned,
are all equally well confirmed by the data. Hypothetico-inferential confirmation, then, is no better off, vindication-wise, than simple inductive inference but, on the other hand, it does not seem to be any worse off either, since both appear to be infinitely bad off as far as deductive vindication is concerned. In fact some argue that hypothetico-deductive confirmation is just a special case of induction by simple enumeration, for, they point out, to assert that a theory or hypothesis is true is to assert that all its consequences are true, which, in turn, induction by simple enumeration would confirm by noting that this consequence is true, that consequence is true, etc. Although I believe that there are crucially important differences between hypothetico-inferential reasoning and simple inductive procedures, I shall not contest this claim for their equivalence as their (deductive) vindication as modes of confirmation is concerned. In this regard they are both hopeless. For example, every set of consequences of one theory (or hypothesis) is also a set of consequences of an infinite number of other, mutually incompatible theories. As far as logic is concerned, both simple induction and hypothetico-inferential reasoning confirm too much, so much that they are completely worthless unless bolstered by nonlogical, unvindicatable (I claim) assumptions. Since both modes are equally bad off, it would seem that we are just as well advised to embrace hypothetico-inferential reasoning in all its fullness as to limit ourselves to simple inductive procedures. I can see no good reason, logical or otherwise, to hold that simple inductive procedures are more fundamental or more basic or that they have any kind of priority at all, except, perhaps, a psychological one for those of us raised in the empiricist tradition. I shall assume then what is borne out by a careful examination of our common-sense knowledge and by even the most cursory and superficial attention to the history of science — that the most important, most fundamental, and most prevalent mode of nondeductive reasoning is hypothetico-inferential.

23 And, presumably, all other hypothetico-inferential confirmation, as well.
24 There is a legitimate prima facie objection against hypothetico-inferential reasoning that involves unobservables, but, following suggestions from the work of Russell and Frank Ramsey, I have shown how this objection loses its force (in "Structural Realism and the Meaning of Theoretical Terms," in Radner and Winokur, eds., Minnesota Studies in the Philosophy of Science, vol. 4, and "Theories, Perception, and Structural Realism").
25 Turnabout is fair play. If hypothetico-deductive reasoning can, in some sense, be reduced to induction by simple enumeration, the converse is also true. For example, the datum that this is black can be inferred from the "theory" that all crows
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This expository digression has been necessary for reasons that will, I hope, emerge presently. One of them is that it has prepared us for a brief examination of the approach of Sir Karl Popper, which, according to him, "preserves empiricism" (but not induction) from the Humean onslaught. And it happens to turn out that the discussion of Popper's proposals leads quite naturally to the heart of the matter with which this essay is concerned. Popper's views are well known and I have discussed them elsewhere at some length so I shall be very brief here. There are, I believe, two independently sufficient reasons for the failure of Popper's attempt: (1) Theories (the interesting and important ones) are not, in general, falsifiable, nor should they be so made by "methodological rules." (2) No matter how many theories we falsify (in a given case) there will always remain an infinite number of unfalsified theories that imply the data, only one of which can be true.

As to (1), Popper explicitly acknowledges that because of the necessity for using auxiliary theories ("background knowledge"), etc., theories are not, in general, deductively falsifiable by "basic" (observation) statements unless certain "methodological rules" are adopted. I do not believe that it is a distortion or an unfair simplification to say that his methodological rules amount to the injunction to assume that the auxiliary theories in use are true, in order that the theory in question be, thus, rendered falsifiable. I have argued, as have others that the history of science shows that not only have such methodological rules been honored more in the breach than in the observance, but also that this is as it should be; to have done otherwise would have been detrimental to scientific progress. Moreover, it is very misleading to say that the assumption of the truth of a (nonobservation) statement "makes" a theory deductively falsifiable by observation statements. Surely the assumptive component is transmitted to the (ostensibly) "falsified"

are black and the initial condition that this is a crow. (This, together with the "facts" that hypothetico-deductive reasoning is reducible — in the sense discussed earlier — to induction by simple enumeration and that any set of data is inferable from an infinite number of mutually incompatible theories, provides another proof that induction by simple enumeration leads to error infinitely more often than to truth.)

27 "Corroboration without Demarcation."
28 Ibid. See also, for example, I. Lakatos, "Falsification and the Methodology of Scientific Research Programs," in I. Lakatos and A. Musgrave, eds., Criticism and the Growth of Knowledge (New York: Cambridge University Press, 1970).
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theory. It would be no more misleading to say that in view of the (observed) falsity of the appropriate basic statement we assume that the theory is false in the light of our assumption that the auxiliary theory is true. But be that as it may, an empiricist must certainly recognize that the assumption enjoined by the “methodological” rule is contingent and, thus, must either be confirmed (which would mean running afoul of Hume’s charge of circularity) or be somehow “vindicated” (presumably, as before, deductively). Although he does not use this terminology, Popper seems to offer an attempt at a deductive vindication when he says that his methodological rules provide (at least a part of) a definition of science. But we must immediately ask why we should accept such a definition especially since, as noted above, it does not seem to correspond either to the actual practice of science or to science as it ought to be practiced.

Even more seriously, and this brings us to (2), we must ask what reason there is to suppose that “science” as defined by Popper ever produces knowledge or, even, that it is a “good” (vindicable, etc.) method for pursuing knowledge. I do not know of any such viable reason whatever. Suppose, contrary to what is the case, that theories were, generally, falsifiable, or suppose that we “make” them falsifiable by adopting Popper’s methodological rules. We are reminded by (2) that no matter how many theories we falsify (relative to a given problem or set of data) or how many we eliminate with methodological rules, there remains an infinite number of mutually incompatible theories each of which is as falsifiable (not falsified, of course) by the data as any other. Getting rid of a small (or even a large) number of false theories does not reduce this bewildering multiplicity one whit. And what reason is there to sup-

I do not wish to claim, of course, that we are never justified in rejecting a theory on the basis of the evidence provided by the experiment or observation in question plus our belief that we also have good evidence that the relevant auxiliary theories, etc., are true. Surely this is how we reject many of the theories that we believe to be false. See, for example, A. Grünbaum, Falsifiability and Rationality (Pittsburgh: University of Pittsburgh Press, forthcoming), as well as examples in his recently revised and greatly expanded Philosophical Problems of Space and Time, 2nd ed. (Boston: Reidel, 1973) and “Can We Ascertain the Falsity of a Scientific Hypothesis?” in M. Mandelbaum, ed., Observation and Theory (Baltimore: Johns Hopkins University Press, 1971) (reprinted in Philosophical Problems of Space and Time, 2nd ed.). Such an approach is not, of course, available to Popper, since it assumes the possibility of giving theories positive confirmation on the basis of evidence, a possibility that he emphatically rejects, and, as we shall see presently, his proposed purely negative reasons for tentatively accepting theories that have so far withstood falsification attempts do not suffice.

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pose that the as-yet-unfalsified theory (or theories) that our feeble efforts have been able to devise is the one true one (or is any “closer to the truth” than any of the others)? It is fair to say, I believe, that Popper, in spite of his “anti-justificationism,” tries to answer this question. And well he might. Otherwise, why go to all the trouble of proposing theories and trying to falsify them at all? In giving his answer Popper says that we aim at truth (Conjectures and Refutations, pp. 33–65). We are justified [sic!], he says, in accepting tentatively, even for practical purposes, a theory which has resisted our earnest attempts at falsification and which for all we know may be true. Well, returning temporarily to (1), for all we know the theories we have “falsified” may be true as well, since they have not really been falsified but only rejected by means of the contingent assumptions enjoined by the “methodological rules.”

But again, be that as it may, let us once more for the sake of argument suppose that falsification is possible and that the problem is the remaining infinite number of unfalsified theories only one of which can be true. Again, let us not make the mistake of (tacitly) applying a principle of indifference and assigning equal probabilities to each of these bewildering possibilities and, thus, concluding that we have zero probability of hitting upon the theory that is true.

Popper not only could point out such a mistake but would also be completely correct in saying that for all we know, we may hit upon the true theory (or one “close to the truth”) after a few falsifications (or even after none) in a sizable portion of our attempts to expand our knowledge. Put in this way, this unexceptionable statement might seem austere enough to avoid offending even the

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30 It will do no good to require that these assumptions themselves be testable (falsifiable) in other contexts. This would require use of other assumptions and we would be off on an infinite regress or into a circle. Popper maintains that such regress is not vicious, but this seems wrong to me unless there is something intrinsically exoneratory about individual attempts at falsification. But this, indeed, is the point at issue, and thus a vicious circle becomes explicit.

Why then (it might be asked) does the falsifiability-falsification account of scientific methodology have such great intuitive plausibility? Well, if it is (contingently!) true, as I contend, that the kinds of theories and hypotheses that we happen to propose (including auxiliary theories) have a much greater probability than the others that would also explain the data, then it will be important and productive to eliminate the false (or probably false) ones from this relatively small class, so that the probability of the remaining one(s) will be rendered even greater thereby (by virtue of their being assigned to a different, “better” reference class). If and only if something like this is the case can I see any point whatever to falsification attempts, “crucial experiments,” etc.

31 Zero probability in a frequency theory does not, of course, mean impossibility.
most rabid anti-justificationist. But reflection soon forces upon us the central, most crucial, and no doubt for many, the most awful fact of the entire epistemological enterprise: if we are to have an appreciable amount of nontrivial knowledge (or, even, of true beliefs), we must hit upon fairly often the true theory or hypothesis (or one reasonably close to the truth) after a relative tiny number of trials. As was the case with our earlier, exactly parallel considerations about the bewilderingly diverse results yielded by the “straight rule,” etc., time is too short for us to sift through (using falsifications to eliminate the chaff) more than an insignificantly small number of the infinite number of possibilities always sanctioned by the data we happen to have. It should be noted that this central assertion does presuppose that our attempts to increase our store of nontrivial, general knowledge and our attempts to predict the future, etc., proceed hypothetico-inferentially. (Recall also that hypothetico-inferential procedure includes simple inductive methods as special—rather degenerate—cases.) It is, of course (logically), possible that one might arrive at general knowledge (or, at least, at true beliefs) and make successful predictions using “pure” guesses or divine revelation quite apart from any kind of hypothetico-inferential strictures. And, emphatically, I must admit and insist that, although I subscribe to it, I do not believe that hypothetico-inferential procedure can be vindicated (in the strong sense required by empiricism) relative to the goals of obtaining general knowledge (or true belief) and making successful predictions. That is, I do not believe that it can be shown to be the “best” procedure or to be a procedure that will succeed if any procedure will.

This statement of this “awful” fact (surely it is, rather, a wonderful one) is equivalent to saying that, if significant knowledge is possible, the relative frequency of successes among human attempts at knowledge accretion must be very high indeed, given the possibilities of failure. This is, in turn, to say that the probability of success must be surprisingly high.

Let me interrupt this train of thought briefly to emphasize that this illustrates the utility and, indeed, the primacy of a relative frequency interpretation of probability for dealing with at least some of the important areas concerned with the truth of statements, including universal statements, and, perhaps, it may open the door to a general, comprehensive relative frequency treatment of probability of statements — including their prior (though not a priori) probabilities. It seems inescapable that
the "payoff," from not only the practical but also the purely epistemic viewpoint, of the probability of a statement must eventually be in terms of frequencies. Suppose, for example, that we begin with interpreting "probability" as "degree of belief" (or "degree of rational belief"). It would still seem that the only reasonable warrant for having, say, a degree of belief of one-half in a certain statement would be the belief that statements of this kind turn out to be true about half of the time. The fact that we only tacitly fix the reference class by use of the phrase "this kind" does not seem to me a crucial objection to the claim that a frequency interpretation is really what is operative here. How else could the "(fair) betting odds" explication of "rational degree of belief" make any sense? Surely we would agree that the fair betting odds on the truth of proposition $p$ is 50-50 if and only if we believed that propositions like $p$ in certain relevant, important respects turn out to be true about half of the time.

Purely "logical" interpretations of probability, such as one based on some kind of inverse of the logical content of statements, may have their uses; but they have nothing to do with the likelihood of statements being true — except in certain degenerate and usually unimportant cases, cases which, moreover, by virtue of the calculus of probability, hold in frequency (as well as all other) interpretations. Popper is quite correct, for example, when he says that science aims at low probabilities (other things being equal, he should always add) when he is talking about probability in the sense of the inverse of the content of statements. But it would be grotesque to conclude from this that, as far as science is concerned, the lower the (objective) likelihood of a statement's being true the better, i.e., to conclude that science aims at low probabilities in the sense of relative frequency of truth in appropriately selected reference classes. It may also be true, as Popper claims, that the probability of a (contingent) universal statement is always zero in the sense of logical probability, although I am dubious about this. But if so, it must be such that this has nothing to do with the likelihood (in the sense of relative frequency, as above) of a given universal statement's being true. Clearly it is a contingent matter about which logic is silent concerning how often statements that resemble in certain important, relevant respects "All swans are white" turn out to be true. Popper explicitly recognizes that science aims at truth (or closeness to truth), as does all epistemic activity. So if he seriously contemplates the possibility of the existence
of knowledge, I do not see how he can avoid assent to the considerations about probability of statements, in the sense of relative frequency of truth (or relative frequency of successful selection of true hypotheses), that have just been outlined. (It is true that Popper has argued against the viability of using truth-frequency interpretations for the probability of statements. However, I do not believe that his arguments touch the kind of interpretation that I, following Salmon, advocate herein. [See Salmon, The Foundations of Scientific Inference, and my “Corroboration without Demarcation.”]).

Finally, I should note that my claim of the primacy of relative frequency does not entail a simple limiting frequency interpretation. Indeed, I believe that, in order for a frequency interpretation to be applicable with any utility, quite strong (contingent) conditions of distributions of frequencies in finite segments or subclasses of the reference class must hold. I have proposed (in “Corroboration without Demarcation”) a (frequency) “definition” of ‘probability’ that imposes one such set of conditions, although I am not positive that a better one may not be given.

The relatively high probability of success of our attempts to attain knowledge (or true beliefs) cannot, of course, be guaranteed, nor can the assumption of its holding be “justified,” in the sense of deductive vindication. Or, at any rate, I do not believe that it can be. About this I am completely with the anti-justificationists: neither the general assumption that we have (and shall continue to have) knowledge (or even true belief) nor specific knowledge claims about the world can be justified in the strong sense that has preoccupied empiricists in general and vindicationists in particular. Any justification must be in terms of other contingent assumptions, and this of course is of no comfort to the empiricist. What many philosophers and most empiricists have held to be the central concern of epistemology must, therefore, be discarded. We must discard the question How are our knowledge claims to be justified? They cannot be justified at all in the sense required by those who usually ask the question.

On the other hand, the necessary subscription of the anti-justificationist falsificationists to hypothetico-inferential procedure leaves most of them no better off than the justificationists, for they are forced to face the dilemma: Either we do not have an appreciable amount of significant general knowledge (or true beliefs) and henceforth shall not
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attain an appreciable degree of success in our predictions or the probability (relative frequency) of success in these matters is relatively very high. It is highly irresponsible, I believe, to take a totally agnostic attitude toward this dilemma. For if we want seriously to contemplate the possibility of knowledge (or true belief) and successful predictions, we must be willing to examine the consequences of this possibility for other central concerns of philosophy, science, and everyday affairs. One of many of these crucial consequences, I contend, is the untenability of empiricism.

It is time now to summarize the central contentions of this paper. The fundamental question of theory of knowledge is not How are our knowledge claims to be justified (or how should they be)? It is rather, What are the conditions that if fulfilled can or will result in the existence of significant knowledge (or true belief)? I have put the matter vaguely and perhaps ungrammatically because I want to leave it open, for the present, whether the conditions are necessary ones, sufficient ones, or both. The answer to this central question that has emerged, assuming, as always, that the search for knowledge proceeds hypothetico-inferentially, is this: The theories and hypotheses that humans propose (i.e., the guesses or knowledge claims that they make) to meet problem situations and account for data or "evidence" at hand must be true (or reasonably "close to the truth") a relatively great portion of the time, given the number of false though otherwise adequate possibilities that always exist. (To say that they are "otherwise adequate" means not only that they are logically adequate but that they fill all of the reasonable requirements of hypothetico-inferential procedure.) Since, in any given case, the proposed theory (or hypothesis) and all of the other adequate theories are equally well supported by the evidence as far as hypothetico-inferential requirements (and, a fortiori, as far as simple inductive requirements) are concerned, it seems quite natural — indeed we seem to be inexorably led — to equate the frequency of success (i.e., of truth or reasonable closeness to truth) of the kind of theory we have proposed in a given situation to the prior (not a priori) probability of the theory (or hypothesis). Since this is probability in the sense of relative frequency (or if one chooses propensity), problems about the nature of the "best" reference class, of course, arise. These are difficult but not, I believe, insuperable. I shall discuss them later but, for the time being, shall just assume that they have been solved in some reasonably satisfactory man-
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ner. Notice that in order to attain knowledge (or, at any rate, true beliefs, or true proposed hypotheses) it is not necessary for the knowing subject to know the values of these probabilities. Strictly speaking, it is not logically necessary, even, for him to estimate or guess at their values. All that is necessary is that a significant portion of the theories and hypotheses that he proposes, believes, uses for predictions, etc., actually have fairly high prior probabilities. The actual values of the probabilities, being relative frequencies (or propensities) are, of course, “objective”; they are what they are completely independently of whether anyone ever knows what they are, ever “measures” them, or ever even makes guesses about or otherwise contemplates them.

But estimation of prior probabilities by the knowing subject, although not logically necessary for true belief, success in prediction, etc., may, nevertheless, turn out to be desirable. And although such estimation is virtually never done explicitly in everyday and in scientific contexts, it surely occurs tacitly fairly often. When we suddenly feel quite sure that the theory or hypothesis that has just occurred to us must be the correct explanation of some hitherto puzzling set of circumstances, whether we be detectives faced with strange, apparently unrelated clues or scientists trying to account for whatever data there are at hand, we are tacitly estimating a fairly high value for the prior probability of our proposed hypothesis or theory. We may (or may not) signal this by telling ourselves or others that this hypothesis “just sounds right” or that we “just feel it in our bones that it must be the right one.” Or, again, when we select among several competing hypotheses or theories proposed for a given set of data, we are ranking them on the basis of our tacit estimation of their prior probabilities. In such a case we might say something like “This hypothesis seems so much more plausible than any of the others (although as far as the evidence is concerned, i.e., as far as hypothetico-inferential [and/or simple inductive] requirements are concerned, all of the hypotheses fare equally well).” Or we might come even closer to

32 We are also tacitly estimating a fairly low or, at any rate, not-too-high value for the prior probability of the data or the clues — the evidence; but more of this later.

33 Here it is not necessary to consider the prior probability of the evidence since it (the evidence) is the same for each hypothesis.

34 It might be pointed out that both in this case and in the previous one, our strong partiality toward the selected hypothesis might be based not only on the current evidence and our guesses about prior probabilities but on past experience, coherence with other, accepted theories, old but relevant evidence, etc.—let us call all such factors “background knowledge.” This is true, but it merely pushes the need
being explicit by saying something like “Hypotheses like this one (adding perhaps ‘in cases like this’) surely turn out to be true (or quite close to the truth) more often than hypotheses like the others that are before us.” Here the reference to frequency and thus to probability breaks out into the open as does one of the central contentions of this paper: Prior probabilities are (or, at any rate, ought to be) objectively existing relative frequencies of the kind discussed both above and below. Our efforts to estimate them may often fall way off the mark, but there is no other viable way of computing or estimating the extent to which our knowledge claims are confirmed by the evidence at hand. The fact that the reference class signaled by the phrase “like this one” is not explicitly delineated is not a conclusive objection; it may be, indeed I claim that it is, true that we have the ability to group theories and hypotheses on the basis of intuited similarities and differences even when we are unable to state explicitly what the similarities and differences are.

The claims just made are, of course, contingent, as are virtually all claims that I have been making about our estimating prior probabilities. I am emphatically not turning my claims into trivialities by stipulatively (and “persuasively”) defining “making an estimate of prior probabilities” as meaning the same as “proposing and strongly believing certain hypotheses or selecting certain hypotheses as opposed to others proposed.” This would be a false report of an actual meaning and a bad proposal for a new one. Although it is necessarily true that if we are to have significant knowledge, the probability of the hypotheses we select must be relatively high (assuming hypothetico-inferential procedure), this does not get us very far toward a theory of confirmation — i.e., toward a theory providing for estimates of probabilities of theories and hypotheses (prior and posterior). As far as this necessary condition is concerned, it is logically possible, for example, that God implants in our minds a good hypothesis or guides us to make a good choice in a relatively high proportion of instances, quite apart from even a tacit concern on our part with prior probabilities. My contentions above imply, of course, the (contingent) falsity for an estimate of prior probability back a step. For, again, there will be another indefinitely large family of mutually incompatible theories or hypotheses all of which fare equally well as far as the evidence and the background knowledge are concerned. So no matter how much background knowledge is operative, we eventually have to make proposals or make selections from among alternatives toward which it is neutral, and, according to the line of thought we have been pursuing, this involves making tacit or explicit estimates of prior probabilities.
of such a possibility. Moreover, I contend further that we have the innate ability, or, perhaps better, the innate capacity to develop, given appropriate environmental history, the ability to make estimates of prior probabilities that, in a significant proportion of trials, are not hopelessly at variance with the true values. This too is, of course, contingent, especially if one holds, as I do, that abilities and capacities are always due to intrinsic, structural characteristics of the individual.

We now have (most of) the ingredients for a contingent theory of confirmation. The expression \( P(X,Y) \) is to be read ‘the probability that something is an X, given that it is a Y’\(^{35}\) and the value of the probability is, by definition, numerically equal to the relative frequency of X’s among Y’s (i.e., the number of things that are both X and Y divided by the number of things that are Y), provided certain distributions of relative frequencies hold in certain appropriate subclasses of Y.\(^{36}\)

Now consider the following form of Bayes’s theorem (the “division theorem” of the calculus of probability):

\[
P(B,C \cap A) = \frac{P(B,A) P(C,A \cap B)}{P(C,A)}
\]

where ‘\( \cap \)’, the “cap,” indicates class intersection (\( A \cap C \) is the class of all things that are [in] both A and C). Now, following Salmon (The Foundations of Scientific Inference) let A be the class of all theories (or hypotheses) like H (the theory [or hypothesis] in question) in (I add)

\(^{35}\) I used to use the Reichenbach-Salmon notation with the arguments reversed, i.e., with \( P(Y,X) \) to give the reading indicated, but I have yielded to numerous protests that this is confusing.

\(^{36}\) More details are given in “Corroboration without Demarcation,” and space does not permit their repetition here. Briefly it is required that the relative frequency in most appropriately selected (e.g., randomly selected) subclasses remains fairly uniform. (The vagueness due to the italicized words is intentional and, I argue, felicitous.) It should be noted here, however, that whether the distribution restrictions hold is, in every case, contingent. I argue that any satisfactory and workable theory of probability or of statistical inference requires some, fairly strong, contingent assumptions about distributions. It may be of some interest that, if the reference class is treated as a sequence (my definition does not require this) and if the restrictions I impose hold for all the sequence in question, then the probability as I define it will coincide with the limit of the relative frequency if the sequence is “long enough” (strictly speaking, infinite). However, all that is necessary in order for this concept of probability to have considerable applicability — especially for “practical” purposes — is for the distribution restrictions to hold for just those subclasses with which we happen to be concerned on a given occasion; as we have noted earlier, in order to have a viable interpretation of probability, it is neither necessary nor sufficient that probability and the limit of the relative frequency coincide (although, admittedly, it is nice when they happen to do so).
certain relevant, important respects (whether these respects be intuited, stated explicitly, or a mixture of both). Let \( B \) be the class of true theories (and hypotheses).\(^{37}\) My treatment of \( C \) is quite different from Salmon's (ibid.). Let 'O' designate all the existing and immediately projected evidence (the conjunction of all relevant data statements) that, on a given occasion, is being considered for our theory or hypothesis of interest, \( H.\)\(^{38}\) Let us refer to such an occasion as a "test situation (for \( H \))" and designate it by 'E'. (We assume that O meets all of the hypothetico-inferential requirements for \( H.\) Now if the evidence really is at hand, that is, if \( O \) actually obtains, we say that \( H \) has passed the test (situation) \( E.\) This is not trivial, for \( O \) may consist in part or in whole of projected evidence, i.e., predictions and, thus, may turn out not to obtain (the predictions may be false), in which case \( H \) fails \( E.\) We now let \( C \) be the class of theories (or hypotheses) that pass (or would pass) tests of the same degree of severity as \( E.\)\(^{39}\) The denominator \( P(C,A) \) will be, thus, the relative frequency of hypotheses that pass tests of the degree of severity of \( E \) among hypotheses like \( H \) (i.e., the fraction of hypotheses like \( H \) that pass tests as severe as \( E \)). We now define the severity of \( E \) as the reciprocal of the probability that \( H \) will pass \( E.\) Since \( H \)'s passing \( E \) is by definition \( O \)'s turning out to be true, it follows that the probability of \( H \) passing \( E \) is the probability of \( O \)'s being true given that the test conditions of \( E \) obtain. If we let \( F \) be the class of occasions when events like those that \( O \) states will occur do occur and \( G \) the class of occasions when test results like those of \( E \) obtain, then this probability, i.e., the probability that \( O \) will be true given that the test conditions of \( E \) obtain,

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\(^{37}\) More realistically \( B \) may be taken to be the class of theories (and hypotheses) whose "closeness to the truth" is above a certain reasonable degree. This introduces complications but not serious ones. The matter is discussed in my "Corroboration without Demarcation." Popper has rightly emphasized the importance of the notion of closeness to the truth (see his Conjectures and Refutations). However, I am uncertain whether his technical concept, "verisimilitude," is a viable explication of it. For the present, I am content with using homely examples such as "Thanksgiving (holiday) (in the U.S.A.) is always on Wednesday" (though false) is closer to the truth than "Thanksgiving always comes on Monday," or "New Orleans has a tropical climate" (though false) is closer to the truth than "New Orleans has a frigid climate."

\(^{38}\) Immediately projected evidence consists of positive outcomes of impending experiments or other observations — outcomes that are (hypothesico-inferentially) predicted by \( H.\)

\(^{39}\) This amounts to letting \( C \) be the class whose members pass tests at least as severe as \( E, \) for if a theory would pass a test more severe than \( E, \) it would, in general, also pass \( E.\)
which is surely the prime candidate for being the prior probability of the evidence \( O \), will be the probability that something is a member of \( G \) given it is a member of \( F \), i.e., \( P(G,F) \), and thus equal to the relative frequency of \( G \)'s among \( F \)'s. Now, obviously, given the definition of 'severity of \( E \)', the probability that a theory or hypothesis like \( H \) will pass a test as severe as \( E \), i.e., \( P(C,A) \), must be equal to \( P(G,F) \), which is the prior probability of the evidence.

The left-hand term of Bayes’s theorem now is the relative frequency of true hypotheses among hypotheses like \( H \) that have passed tests as severe as \( E \). Surely this is an appropriate value for the posterior (relative to \( E \)) probability of \( H \). Let us now consider first those cases in which either \( H \) (together, perhaps, with unproblematic initial conditions) entails \( O \) or in which we take it to be unproblematic that \( H \) at least implies \( O \) (because, for example, we take the operative auxiliary theories to be unproblematic). In these cases, the second factor of the numerator of the right-hand side, \( P(C,A \cap B) \), will be equal to one. For it is the relative frequency with which hypotheses like \( H \) that are true pass tests as severe as \( E \) and, according to our definitions, a true hypothesis like \( H \) will always pass a test like \( E \). For such cases, then, the expression becomes

\[
P(B,A \cap C) = \frac{P(B,A)}{P(C,A)}
\]

The numerator, \( P(B,A) \), is the relative frequency with which hypotheses like \( H \) are true (irrespective of all other factors such as evidence situation) and, therefore, deserves to be designated the prior probability of \( H \). Thus, for these cases, we have arrived at the familiar result that the posterior probability of the theory or hypothesis of interest, \( H \), is equal to the prior probability of \( H \) divided by the prior probability of the evidence.\(^{40}\) The distinguishing and not-so-familiar feature of this result is that we use a relative frequency interpretation of probability in

\(^{40}\) Cases where the second factor of the numerator is not unity or near thereto are, of course, more complicated but, often, not hopelessly so. For example, when \( H \) is a statistical hypothesis (theory) about the long-run relative frequency (probability) of some attribute of interest in some reference class and \( O \) is a statement of (an interval) of the relative frequency in a subclass, the value of \( P(C,A \cap B) \) may be calculated, using the binomial theorem. (The distribution requirements that my definition of "probability" impose are almost equivalent to requiring randomness, so that, for all practical purposes in virtually all cases, the binomial theorem will apply.) Still more complicated cases require more guesswork, but, I shall argue, this is not a fatal objection.
our application of Bayes's theorem instead of a directly propositional interpretation such as some so-called "logical" interpretation or degree-of-belief (or degree-of-rational-belief) interpretation.

Partisans of directly propositional interpretations may justly claim that, for practical purposes, there is little difference between the two kinds of interpretations — justly, that is, if they grant my earlier contention that both the theoretical and practical significance of any probability ("degree-of-belief," "logical," or whatever) must be grounded in likelihood of truth (or a likelihood of closeness to truth) which, in turn, must be in terms of (objectively existing though perhaps never precisely known) relative frequencies (of the truth of propositions of such and such kinds in such and such circumstances, etc.). To put this qualification in other words: Even if we use a degree-of-belief approach, the following crucial point must be recognized — only if the positive correlations of our degrees of belief with such (objectively existing) truth frequencies is not hopelessly low can our attempts to accumulate significant knowledge (or true beliefs), predict the future, etc., meet with appreciable success. (I have claimed, moreover, that it is a psychological fact that we often think in terms of such relative frequencies, sometimes implicitly, sometimes explicitly.) Thus, for epistemological purposes, for the purposes of the foundations of a theory of nondemonstrative inference, the foundations of a theory of scientific and common-sense knowledge, in general, and the foundations of such disciplines as statistics, in particular, the relative frequency approach must be taken as basic.

According to this theory of confirmation, then, the procedure followed when we wish to estimate the probability (or the "degree of confirm-

41 A directly propositional interpretation is one in which the argument variables, say, 'X' and 'Y' of the probability functor P(X,Y)', take (directly) as their values individual propositions (theories, hypotheses, etc.); for example, an expression such as P(q,r)' would be read 'the probability [in this case not relative frequency, of course] that q is true given that r is true'. In a relative frequency (or in a propensity) interpretation, the values of these variables are, of course, always classes. Moreover, I contend, there is a very wide latitude in the kind of members such classes may have; in the main cases we have been considering, the members of the classes are propositions (or statements or sentences for those who so prefer); this yields what could be called an "indirectly propositional interpretation."

42 Common-sense likelihood, not just likelihood in the statistician's sense.

43 I have used the qualifying phrase "a theory of" advisedly, for I certainly do not want to contend that knowledge (or nondemonstrative inference or science) has a foundation in the sense of a firm, unproblematic base. Even the observational basis of knowledge may be, and of course has been, challenged, although it is not a purpose of this essay to do so.
tion") of a theory (or hypothesis) of interest, \( H \), relative to the evidence at hand, \( O \), is something like the following. (For simplicity, let us assume that \( P(C,A \cap B) \) is close enough to unity to be neglected.) We make, explicitly or implicitly, estimates of the values of \( P(B,A) \) and \( P(C,A) \) and divide the former by the latter to yield the value of \( P(B,A \cap C) \). That is to say, we divide our estimate of the prior probability of the theory (or hypothesis) by our estimate of the prior probability of the evidence to yield the posterior probability ("degree of confirmation,” etc.) of the theory (or hypothesis).

Regarding this procedure, I wish to defend a claim and a recommendation. I claim that it is a pretty accurate "reconstruction" of the reasoning that we often do perform — sometimes explicitly, more often, perhaps, implicitly — both in science and in everyday life. Much more importantly, I recommend that this theory be adopted explicitly in those cases where a theory of confirmation is needed; that is, I say that our (non-deductive) reasoning ought to conform to this model.

To defend the claim, I would cite some of the familiar arguments of the "personalists" and other so-called "Bayesians." Surely, for example, it is a psychological fact that we have a high degree of confidence in a hypothesis or theory (a high estimate of the frequency with which such theories are true) when the theory accounts for "facts" (evidence) that otherwise would be very surprising or mysterious, i.e., when we estimate the prior probability of the evidence to be quite low (provided that our estimate of the prior probability of the theory is high enough to prevent us from discounting it in spite of the evidence). And I have already given arguments to the effect that we often do account for our degrees of confidence in terms of estimated (truth) frequencies and that, if they are to have practical or theoretical utility, we are obliged to do so.

Part of the defense of the recommendation has been given already. I have argued for the inadequacy, indeed the total impotence, of purely "logical" approaches to confirmation theory (or [tentative] hypothesis...
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acceptance) such as vindicationism and falsificationism. I take it to be unproblematic that common sense and scientific inquiry proceed hypothetico-inferentially (remembering that simple inductive procedures are special cases of hypothetico-inferential ones) and, thus, that it is now established that, if we are to have an appreciable amount of significant knowledge (or true belief), the relative frequency of our proposal and selection of true (or reasonably close to true) theories and hypotheses must be relatively high. Now I have made the contingent claim that, if this appreciable amount of significant knowledge (or true belief) does exist, it is explained by our constitutional abilities to propose, not too infrequently, pretty good theories and hypotheses, to make, fairly often, estimates of prior probabilities that are not hopelessly off the mark, etc. (This explanation is opposed to other contingent possibilities such as God’s popping a good hypothesis or a good estimate into our heads not too infrequently.) From this claim arose the (contingent) theory of confirmation just proposed.

The contingent explanation just cited is, of course, problematic. In “Corroboration without Demarcation,” I purported to give a kind of (very weak) “vindication” of it, but I wish now to withdraw any such pretension, especially in view of the high hopes that have become associated with “vindication” attempts. By way of defense now, I can only say that this claim along with the resulting theory of confirmation seems as adequate to its task as is possible and seems to avoid the fatal objections to all other such explanations and theories that have been proposed. Being contingent, it must, of course, admit of alternatives, and, when any of them is proposed, it should be carefully considered with open-minded interest. I certainly do not want to be inconsistent enough to contend that my theory or any alternative can ever have a conclusive justification or vindication. Needless to say, if someone offers an alternative and argues that it should be accepted because its implications are more congenial with his, or with the prevailing philosophical tastes, I shall be unmoved unless his theory also accounts for and provides for the other crucial aspects of the epistemological predicament as well as mine does. I can only end this defense on still another autobiographical note: So far, owing no doubt to lack of ability or lack of motivation or both, I have not been able to actually construct any adequate, plausible alternative to make do with observation statements plus logic (including the broad but still “tough” sense of “logic” explained earlier) alone.
Now it must be admitted, even insisted, that the necessary reliance on personal estimates of prior probabilities introduces an irreducibly subjective component into every value of the probability or degree of confirmation that we are ever able to calculate for a theory or hypothesis. (This does not mean that the actual value of the probability [which, recall, is an "objectively existing" relative frequency] does not exist, or is itself subjective, or is something about which it is pointless to concern ourselves — any more than the fact that in order to forecast tomorrow's weather we must make subjective estimates, guesses, hunches, etc., means that tomorrow's weather [when or before it happens] does not exist or is subjective, or is something about which it is pointless for meteorologists to concern themselves.) I have no desire to make a virtue out of this subjective component. But there it is, and unavoidably so. As noted earlier, we may be able to postpone the "bare" estimation of prior probabilities one or two steps backward by relying on old but relevant evidence, or rather, "background knowledge." But there will always be (ad nauseam) the troublesome multiplicity of hypotheses that fare equally well with any amount of old evidence and background knowledge. Eventually, we must make an (evidentially) unaided estimate or, if you choose, a pure raw guess about the relevant prior probabilities.

As noted earlier, part of my proposed interpretation of Bayes's theorem is due to the pioneering work of Wesley Salmon. He has proposed (in *The Foundations of Scientific Inference*) that the needed values for the prior probabilities be obtained (at least in part) by using induction by simple enumeration. Just how this is to be accomplished is not completely clear to me, but it does not matter: we must note that any application of induction by simple enumeration (like all other kinds of hypothetico-inferential procedures) is totally useless without some (at least tacit) use of prior probabilities. (E.g., for any theory or hypothesis supported by such application of induction by simple enumeration there will be our multiplicity of other theories equally well supported by equally "good" [logically speaking] applications of induction by simple enumeration.) So simple inductive procedures can in no way provide means for avoiding "bare" estimates of prior probabilities.

*See Salmon, *The Foundations of Scientific Inference*, and my "Corroboration without Demarcation."*
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Involved in this use of Bayes's theorem there is another irreducibly personal subjective factor. This concerns the selection of the reference classes needed for specification of the relevant relative frequencies (probabilities), especially $A$, the class of hypotheses (or theories) like $H$, and $F$, the class of occasions when events like those that $O$ states will occur do occur. To say that the members of $A$ are like $H$ in certain important, relevant respects does not, of course, go very far. We might like to be able to know and to state explicitly, in specific cases, just what these respects are taken to be. The difficulties are partly, though by no means altogether, ameliorated by one of the properties of (nicely behaved, as ours are required to be) relative frequencies: as long as our location of members in their respective classes is consistent, then, in the long run, our inferences about (as yet unobserved) relative frequencies (and, thus, probabilities) will, for the most part, be fairly accurate. (Fortunately, if our distribution requirements hold, there is no danger of the long run becoming infinite. It is contingently possible that they do not hold, of course, in which cases "probability" would not be defined.) It must be admitted, however, that, in practice, some reference classes serve our ends considerably better than others and, on the other hand, that we would often find it difficult or impossible to say explicitly just what are the characteristics of our theory of interest, $H$, that, we believe, delineate a class of theories, a fairly high portion of which are true or close to the truth. We may be able to do this at times, but certainly, on many occasions, we must rely (1) on our personal conviction that there is some such set of in-principle manageable character-

48 The importance of this problem was also noted by Salmon, The Foundations of Scientific Inference.

49 For example, no matter what reference class an insurance company uses, if its actuarial data are representative, it can adjust premiums and policy values to yield reliably any predetermined margin of profit. In practice, of course, companies select reference classes such that the margin of profit is high even though premiums are relatively low. In this way, they sell many more policies and also avoid the outrages against a "free society" that would result if they fostered conditions whereby the fortunate (the "good risks") bear part of the burden of the unfortunate (the "uninsurable"). (Actually this is more a description of principle than of practice. Insurance companies have almost succeeded in consistently collecting huge premiums with very little risk of paying out benefits, especially where insurance is virtually mandatory as with homestead and automobile insurance. The very real danger of policy cancellation or of even much higher premiums often prevents policyholders from reporting quite significant, legitimate, and innocent losses.)

50 Without this or similar qualification, this requirement would always be (trivially) satisfied. For, owing to reasons similar to those given in connection with other trouble-
istics and (2) on our constitutional ability to classify theories according to such characteristics even though we do not have explicit knowledge of what they are.

Of those who may object to "all of this subjectivity" I can only ask, "What is the alternative?" It seems undeniable to me that we do proceed in something very much like the manner I have described in actual cases of significant knowledge accretion (assuming that such cases occur). There are logically possible alternatives, but the ones that have occurred to me, such as divine intervention in all or in most individual successful trials, do not seem very plausible, whether they be claims about what actually occurs or recommendations on what confirmation procedures, given the difficulties we have considered, we ought to employ. Again, however, I am open to suggestion.

The necessity for these two "subjective" activities, the estimation of prior probabilities and the grouping of theories and hypotheses into fairly appropriate reference classes, seems to me to constitute the considerable kernel of truth in the "new subjectivism" attributed to figures such as Paul Feyerabend, Thomas Kuhn, and Michael Polanyi. (I hope that I am not too far off the mark if, on the basis of his essay in this volume, I place Walter Weimer in the same camp.) But, although, as emphasized earlier, arguments from wishful thinking are of no avail, I do not believe that the consequences of this required amount of subjective activity are nearly as disastrous as empiricists fear nor, even, quite as extreme as, say, Feyerabend and Polanyi sometimes represent them.

Certainly skepticism is not a consequence of my views except for those whose standard of certification or "definition" of knowledge is unrealistically and misguided rigorously or who will not be satisfied with quite adequate though, of course, not completely conclusive support for holding that given beliefs are true or close to true. I certainly believe very strongly that we attain true beliefs fairly often (though not as often as we commonly sensibly believe that we do). Of course, I do not know this for certain or even believe it beyond all shadow of doubt. I also believe that we have a certain amount of significant knowledge (again, probably not as much as we think we do), and I am quite willing, even, to say that I know that we do, though I do not know it for certain. I am willing, even, to propose a definition of 'knowledge' with the under-

some multiplicities, there will always be many classes of theories with high truth frequencies each delineated by some subset of characteristics of \( H \).
standing that it is only approximate, probably needs further qualification, will not cover all cases in all contexts, etc.\textsuperscript{51} It is as follows:

\begin{itemize}
\item[(1)] p is true or fairly close to the truth
\item[(2)] X believes p (or, perhaps, X estimates that the probability of p is quite high, or X sincerely proposes p as an hypothesis that ought to be accepted tentatively, etc.)
\item[(3)] p, when conjoined with appropriate theories, auxiliary theories, etc., and/or “initial conditions,” yields as a hypothetico-inferential consequence statements of the evidence that X has (or X knows that p “directly” [by observation, etc.])
\item[(4)] The (actual) prior probability of p and that of the evidence as well as X’s estimates of these prior probabilities are such that the actual value and X’s resulting calculated value of the posterior probability of p are both quite high (although, in general, they will not coincide).\textsuperscript{52}
\end{itemize}

If someone were to say that this is just the old definition in new clothes, I would not object but would have to contend that clothes make the definition. I have no objection to saying that statements of conditions (3) and (4) amount to stating that X has good or adequate evidence for p or that X is justified in believing p or in proposing p for tentative acceptance. But it is imperative to note that these standards or “criteria” for good or adequate evidence, for justified belief or justified proposal, and, thus, for knowledge, are a far cry from those that most philosophers — certainly most empiricists — want to require. It may be of special concern that the subjective component is duly incorporated into the definition.

By now it is, no doubt, anticlimactic and redundant to criticize empiricism on the basis of the (contingent) confirmation theory proposed here. It was, indeed, at least partly because of the fatal logical defects of empiricism (discussed earlier here, although they were adequately and elegantly demonstrated by Hume) that the theory was proposed. Perhaps

\textsuperscript{51} The wisdom of Bertrand Russell concerning the “nature” of knowledge is worth quoting over and over, but rather than repeat it here, I have reserved it for the final footnote of this paper (the final footnote of the appendix).

\textsuperscript{52} I should have added that X’s selection of the reference classes or X’s (implicit) classification of p and the evidence is adequately appropriate and, no doubt, still more besides, but the definition is too long already.
it is not amiss, however, to try to gather up a few loose ends on this score. We are not foolish enough to deny that we learn at least a great portion of what we know by experience, nor do we wish to minimize the crucial epistemological role of (observational) evidence and, even, of justification in our attenuated sense. But we do have to maintain that testing by confrontation with evidence is crucially important because of certain contingent but untestable propositions that must be true if knowledge exists. We have seen that unless the relative frequency of success after a fairly small number of knowledge trials is considerably greater than infinitesimal we cannot obtain an appreciable amount of significant knowledge. Unless this condition holds, no amount of testing, no amount of evidence, will, in general, be of any avail. On the other hand, if the condition does hold, i.e., if we are going to have to consider only a relatively small number of hypotheses, then it is valuable, perhaps imperative, to have evidence that may eliminate some that are false if (it is often a big "if") we have relatively unproblematic background knowledge (background theories with high prior and/or posterior probability) and knowledge of initial conditions, etc. (the kernel of truth in falsificationism), and it is perhaps equally important to have evidence which, when properly evaluated for its prior probability and when combined with the prior probabilities of the remaining theories or hypotheses of interest will give us an estimate of the posterior probabilities of these hypotheses or theories of interest. It thus turns out that we cannot learn from experience unless empiricism is false. "Empirical" testing is worthless unless empiricism is false and unless, moreover, certain unempirical though contingent propositions are true. If the conditions in question do hold (including) our ability to estimate prior probabilities and to group propositions into useful reference classes, then it follows obviously and necessarily that evidence can produce the desirable results just mentioned.

It is sometimes claimed that Bayes’s theorem has very limited applicability or that in some contexts, including attempted calculation of the probability of theories, its use is illegitimate. However, with any relative frequency interpretation, all the theorems of the calculus of probability are (trivial) truths of arithmetic, or rather, of set theory; there can be no question of legitimacy or of the validity of the results when relative frequencies are substituted into Bayes’s theorem, and, thus, no objection to the use to which we have put Bayes’s theorem.
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There is, however, a legitimate and prima facie troublesome question regarding the efficacy of evidence. It might be asked: Since the calculation of the posterior probability of $H$ is made solely from prior probabilities (of the evidence and of $H$), what reason is there to prefer the value of the posterior probability over the prior probability of $H$, i.e., why prefer the reference class $A \cap C$ to the reference class $A$? Both values are values of relative frequencies and, as we saw earlier, predictions (of frequencies) on the basis of one (long-run) frequency will, in the long run, turn out as well as predictions made on the basis of any other (long-run) frequency. To state the puzzle thus adequately is to solve it. We are interested not only in being right about (truth) frequencies but in being right as often as possible about the truth of individual hypotheses. If we can locate our hypotheses of interest in “good” reference classes, the classes corresponding to $A \cap C$ that have, we may hope, higher frequencies of truth than those used in estimating prior probabilities, those corresponding to $A$, then when we act on assumptions like the assumption that $H$ is true, we will be right more often than when the frequency has the lower value corresponding to $A$. Our knowledge of the evidence provides us with the knowledge that $H$ is, in addition to being a member of $A$, a class with truth frequency, $P(B,A)$, it is also a member of $A \cap C$, which has the higher truth frequency, $P(B,A \cap C)$. It is also obvious, at least in the cases where $H$ implies $O$, why we want to collect as much evidence as possible, ceteris paribus. New evidence, it is also easy to explain why new evidence of the same kind as we already have usually counts for very little (and thus why new kinds of evidence count for much more) and why the probability of a universal proposition (theory) is not necessarily zero, contra Popper and Carnap. (In this connection, of course, Carnap and Popper use [different kinds of] “logical” interpretations of probability. I argued earlier that such interpretations are worse than useless for the epistemic purposes with which we and they are concerned. The fact that such interpretations yield such a strange and undesirable result [zero probability for any universal statement] seems to me to provide another strong argument against their legitimacy.)

Let $O$ be some evidence statement and let $O'$ be a statement that is true if and only if a repetition of the “same” experiment or the same relevant conditions of observation produce the same results as those stated to obtain by $O$ (i.e., same experiment, same result). Now even if the prior probability of $O$ is quite low, the prior probability of the conjunction of $O$ and $O'$ will, in general, be only negligibly less than that of $O$ alone, or, using a somewhat different approach, the probability of $O'$ given that $O$ obtains (the prior probability relative to the new evidence) is relatively high. It is merely a shibboleth that the (relevant) probabilities of $O$ and $O'$ must be independent of and equal to each other. Using our frequency approach, this is easy to see. We use for the prior probability of $O$, of course, the frequency of
occasions that events like those "predicted" by O occur among occasions like those of the experiment or occasion of observation. Now obviously, although such frequency may be low, it may be not much greater than that of occasions when two such events like those predicted by O occur when conditions like those of the experiment hold twice. Or, to proceed somewhat differently, the frequency with which we observe events like the one stated to occur by O among the relevant observational occasions may be quite low, but once we observe such an event to occur, we should put other events including, of course, the one corresponding to O' in a different reference class; specifically, the frequency with which occasions in which events like those corresponding to O' occur among occasions in which the relevant observational conditions obtain and which occur after a very similar event (the one predicted by O) has occurred — this frequency will, in general, be relatively high. That this is true is of course contingent, and this is as it should be; it seems to be a contingency that is well supported by the evidence. (The latter contention may seem very problematic; it is discussed in fn. 56.) These much higher prior probabilities (prior relative to a given evidential situation) of repeated instances mean, of course, that they will raise the posterior probability of the theory by a very small, perhaps a negligible amount; and since the relevant probabilities of the instances of a universal statement, as they occur, or as they will occur, or as they would occur, are neither equal to nor independent of each other, we cannot conclude that the probability of the universal statement is zero; i.e., this probability cannot be calculated by taking the product of the probabilities of the instances and even if it could it would not be zero because of the rapid increase (approaching unity) of the probabilities of repeated instances.

These considerations, along with previous ones, also lead us to reject or, at best, severely qualify the widely held contention that values for prior probabilities are not so important after all because widely differing estimated values will produce converging results for posterior probabilities as the amount of evidence collected increases, or, as it is sometimes put, the prior probabilities "wash out" as more evidence rolls in. Some qualifications are generally recognized in connection with this claim, such as requiring that the estimates for the prior probability of the theory of interest be fairly low but not too near zero and that the estimates for that of the evidence not be too near to unity or to that of the theory (in which, latter case, the convergence will proceed much faster than is reasonable). Even this weakens considerably the original claim. For example, surely there are many cases in which the prior probability should be estimated as being near unity. Moreover, the two following qualifications must be added: (1) the prior probability of the theory of interest can never "wash out" relative to other competing theories, and (2) in order for there to be any appreciable "washing out," the new evidence that "rolls in" must always be different in kind from the old, otherwise its prior probability certainly should be estimated as being close to unity.

On the other hand, it should be admitted and insisted that our estimates of prior probabilities will virtually never yield anything like precise values to be used in Bayes's theorem. This is no great handicap. In many cases, especially when we have a considerable amount of varied evidence, we will be in good shape if our estimates are within a few orders of magnitude of the true values; there is some "washing out" effect and it is of some importance. In fact, in most cases we will simply say something like "I estimate the prior probability of the theory of interest to be not too low, say between 0.05 and 0.5, and that of the evidence to be not too high, say between 0.06 and 0.6." (Obviously, when H implies O, the probability of O must be at least as great as that of H.) Also, when the problem is to select among competing hypotheses (relative to the same evidence), all that is necessary is to order the hypotheses according to their prior probabilities; no estimate of numerical values are required.
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if favorable to \( H \) (if \( H \) passes the test, in our sense), will tell us that \( H \) is a member of a class with an even higher truth frequency; if it is unfavorable, we will reject \( H \) as false, and the more false hypotheses we eliminate from our (contingently) limited number of viable possibilities, the more likely it is that the next one for which we collect a store of favorable evidence will be true. All of this may be painfully obvious by now, but it clears up a number of puzzles, I believe. It explains why and how it is that we “learn from experience” or from confronting our theories with evidence rather than having to postulate that this is how knowledge grows.\(^{54}\) And for the simple cases (where \( H \) implies \( O \)) our theory justifies the “principle of total evidence” as well as the “conditionalization” of probabilities (conditionalized upon evidence or conditionalized again upon new(er) evidence) — a problem for the personalists and perhaps other directly propositional interpreters of probability\(^{55}\) — and certainly our approach does not need any further justification of the use of the calculus of probability for dealing with evidence, etc., such as the possibility of “making Dutch book” if one doesn’t.\(^{56}\)

Space does not permit discussion of the more complicated cases where \( H \) does not imply \( O \) (except “probabilistically”) or the less rigorous, more complicated but better advised policy of taking \( B \) to be the class

\(^{54}\) A fault shared, it seems to me, by the systems of both Carnap and Popper (among others) even though they may widely diverge otherwise.


\(^{56}\) I do not want to take the space, in an essay already becoming too long, to list and explain all the cases in which, I believe, this theory clears up rather notorious difficulties in confirmation theory. In almost every case the difficulty disappears owing either to the abandonment of the mistaken idea that the confirmation relationship between evidence and theory (in that direction) is purely logical or to the use of the (truth) frequency interpretation of probability (or to both). For example, I have explained in “Corroboration without Demarcation” how this theory (or almost any Bayesian confirmation theory) “solves” Goodman’s “new riddle of induction” and Hempel’s notorious “paradoxes of confirmation.” (The first case should be obvious by now. “All emeralds are grue” is just one of the infinite number of “theories” that compete with “All emeralds are green,” relative to the present evidence (lots of observed green [or grue, etc.] emeralds); we reject it and the other competitors on the basis of the much higher prior probability accorded by our estimates to “All emeralds are green.” In the latter case, the prior probability of the next crow’s being black is low relative to the prior probability of the next nonblack thing’s being a noncrow so that the latter piece of “evidence” confirms “All crows are black” (and, of course, its logical equivalent, “All nonblack things are non-crows”) much, much less than does the next crow’s being black.)
of hypotheses whose closeness to the truth is equal to or above a certain desirable value instead of the class of true hypotheses. The results are similar to those for the simple cases, though not as neat, and they rely, at times, even more heavily, on something like the distribution requirements imposed by our definition of 'probability'.

Nature is, of course, by no means necessarily bound to be so obliging as to fulfill such requirements. But one prime result to which we have returned again and again is that, unless nature is kind in some vital respects, knowledge is impossible. The vital respect that I have stressed and in which I have assumed such kindness to obtain has been the existence in us (a part of nature) of constitutional abilities to cope with the epistemic predicament. In principle, this could be sufficient. No matter how complicated nature may be and even if our distribution requirements do not obtain, given any set of data (including any set of distribution data), there will always be some function (and, thus, some theory), indeed an infinite number of them, that will account for the data and accurately predict future occurrences in the domain in question; this holds, a fortiori, for distributions of frequencies. If our distribution requirements do not hold we could, in principle, drop them and possibly discover, in each case, a distribution function that would do the job. As a contingent matter of fact, however, it seems probable that the existence of knowledge, if it does exist, is due both to our constitutional abilities and to a certain (additional) amount of simplicity in nature — enough, for example, so that the distribution requirements hold: I must admit that, at present, I see no satisfactory way of constructing my (contingent!) theory of confirmation without them. In fact, more generally, it must be admitted that, in addition to our contingent assumptions about our constitutional capacities, we also, in effect, are assuming that the comparatively limited amount of evidence that we are able to accumulate is not hopelessly unrepresentative. Some have remarked that it would be perverse or self-stultifying not to so assume. This is in all likelihood true, but it should be recognized that such an assumption, though contingent, is not an empirical one.

These (contingent!) assumptions, just discussed, are, in effect, the postulates of my proposed theory of confirmation. Additional (contin-
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gent) "postulates," of a kind, enter, of course, into each individual case of confirmation (or disconfirmation); they are our estimates of the appropriate prior probabilities. These postulates may be compared and contrasted with Bertrand Russell's "postulates of scientific inference" (Human Knowledge: Its Scope and Limits). As important as is Russell's pioneering criticism of empiricist approaches to confirmation and as much as I am indebted to him, we part ways in our constructive approaches to confirmation. His "postulates" may very well reflect an important part of our scientific and common-sense knowledge, but they are, I believe, neither sufficient nor necessary for a viable theory of confirmation. But, if my arguments ad nauseam have been of any avail, it is clear that some postulation is absolutely necessary if confirmation is to proceed and be of any use whatever. I must, therefore, defend Russell against my dear friend Wes Salmon, who twits him for succumbing to the "temptation" to use postulates and, thus, to his (Russell's) own bête noire of choosing the "advantages of theft over honest toil."58 For, just as we know now that an infinitude of "honest toil" could never derive Euclid's parallel postulate from his others and that accepting it as a real, operative postulate (for Euclidean geometry) is not theft but bare necessity, we must now recognize that abandonment of the search for confirmation without postulation, far from smacking of larceny, is the abandonment of futile wishful thinking for the honest confrontation of epistemic reality.

Does the untenability of empiricism and the unavoidable reliance on the importance of subjective elements in confirmation open the floodgates to all sorts of crackpotism, dogmatism, pseudoscience, etc., or at best, to either scientific chaos, on the one hand, or intellectual "elitism," on the other? Well, as remarked earlier, there is no way of preventing the chips from falling where they may. I do not believe, however, that things are as bad as some may fear; and, again, this is a contingent matter but, this time, one regarding which it is proper to cite evidence at hand, and, if desired, to collect more. The evidence seems to support the view that, in general, the best success is obtained, so far at least, and also as far as such things as success in prediction and having theories that fare best as more evidence is collected are concerned, by those who

58 W. Salmon, "Russell on Scientific Inference or Will the Real Deductivist Please Stand Up?" in G. Nakhnikian, ed., Russell's Philosophy (London: Duckworth, 1974).
"keep an open mind," who propose theories that are amenable to con-
firmation and disconfirmation by evidence, who collect as much evi-
dence as is feasible and invite others to do likewise, as opposed to those
who scorn evidence altogether or who accord extremely high prior prob-
abilities to their "pet" theories, extremely low prior probabilities to
evidence that supports them and extremely high ones to evidence that
disconfirms them, etc. (There are, of course, important and brilliant
exceptions in the history of science. The existence there of a little,
though not a great deal, of "elitism" seems to be indicated by the evi-
dence.) This claim is also supported by our theory of confirmation, for,
as we have seen, it gives a rationale for collecting as much and as varied
evidence as is feasible, attempting either to increase the degree of con-
firmation or to eliminate false (or not close enough to true) theories by
falsification or disconfirmation. It is time not only to admit but to insist
that it is a contingent fact (if it is a fact), apparently well supported by
the evidence (and other good contingent and logical considerations),
that "good scientific practice" produces, in general, better results than
such alternatives as dogmatism and reliance on authority or (purported)
"revelation." This claim assumes of course that our estimate of the prior
probability of the "theory" (that "good" scientific practice is better than
dogmatism, etc.) is not extremely low and that our estimation of prior
probabilities for the various bits of evidence is not extremely high.

We must now face an important and inevitable question: What, if
anything, can be done to resolve disagreements due to interpersonal
differences in (subjective) estimates of prior probabilities? Well, one
can recommend that one's opponent reexamine the "background knowl-
edge," that he take into account alternative auxiliary theories or "old"
evidence of which he may not have been aware, etc. If this is of no
avail, we might try collecting more evidence of a different kind, hoping
that his estimate of its prior probability will be quite low. Finally, we
might try reconciling our selection of appropriate reference classes. If
none of this is of any avail, I do not see anything to do but to recognize
the disagreement. We may feel supremely confident that our opponent
is wrong on this matter or, even, that he is stupid — that he is such a
poor estimator of prior probabilities and such a poor selector of reference
classes that he will never attain enlightenment — but I do not know of
any additional means whereby such a "feeling" can be established. I
hope that it will, before too much longer, be considered a mark of
epistemological maturity to have relinquished the quest for "logical" or "methodological" whips with which to chastise those so misguided as to disagree with one's own cherished conclusions.

I should like now to rest both the case against empiricism and the case for the alternative theory of confirmation that I have proposed. I hope that it is unnecessary to add a personal note warning against misinterpretation of my criticism, some of it perhaps too severe, of the work of others, especially that of Feigl, Popper, and Salmon. If any of my proposals have any value at all, it is due mainly to the foundations laid by their vital pioneering efforts in the field (plus those of Bertrand Russell). And I take it that it is a truism that, if some or even many of their basic contentions require modification or abandonment, this is what one should expect and, indeed, welcome as a virtually necessary condition for progress in both science and philosophy (I argue that the two differ, in the main, only in degree).

The paper will now be brought to a close with a discussion of some of its implications for common sense, science, and philosophy. I have written on this subject at some length elsewhere, and shall, therefore, limit myself here to a brief discussion of a few main points. In an appendix to the paper, a few of the other currently popular approaches to confirmation, probability, and induction will be considered briefly.

I shall not dwell at all on the wearisome and depressing matters having to do with restrictive and truncating methodologies such as operationism, behaviorism, and related radically empiricist positions. Although they totally lack any viable rationale and are disastrously impeding progress, especially in psychology and the other "behavioral"

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60 Not to be confused with what is sometimes called "behavioralism" — the view seeking to exclude value judgments from science or, at any rate, to distinguish them sharply from the main body of the science. I do not wish to take sides on this issue here.

61 The almost universal acceptance and usage of this question-begging, pseudo-scientific label (along with facts such as B. F. Skinner's being, according to a recent survey by the popular magazine Psychology Today the most admired psychologist by those in the profession today) indicate how pervasive is the occurrence of these methodological diseases. Fortunately, with a sizable portion of scientists, the infection is relatively superficial. They give lip service to operationism or behaviorism but it does not seem to affect their behavior [!] in their scientific (or everyday) practice. They go on acting, and talking, and theorizing as if they believed that people (and animals) do have ("inner") feelings, thoughts, pains, pleasures, etc. Apparently they
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sciences, their refutation does not require the material developed in this paper. Sufficient to destroy completely any prima facie justification that such views might seem to enjoy are the realizations that (1) hypothetico-inferential procedures are as legitimate as, and indeed, more prevalent and fundamental than, simple inductive ones, a fact now acknowledged by most empiricist philosophers, and (2) we can refer to unobservable things, events, and properties without naming them — such reference being accomplished by means of definite and indefinite descriptions a la Russell and Ramsey.\(^6\) (In most parts of psychology and the social sciences, we do not even need the Russell-Ramsey devices; we can refer directly to the thoughts and feelings of others because we know what our own thoughts and feelings are and, according to theories best accounting for the evidence that we have, these are of the same kinds as those of others.)

Obviously, restrictive bludgeons such as “meaning criteria” based on verifiability or, even, confirmability of any kind acceptable to empiricism must fall by the wayside along with other empiricist nostrums. This also holds for Popper’s “criterion of demarcation,” for we have seen that, in general, interesting and important theories and hypotheses neither are nor ought to be falsifiable. In fact, as far as methodology is concerned, virtually all the important implications are negative ones. I am so strongly tempted that I shall yield and say that the less methodology the better and support something very similar to Feyerabend’s “methodological anarchism.”\(^7\) Whenever and wherever methodological needs do arise, they are almost certain to be due to contingent facts about the area of

are misled into the lip service because they are misled to infer from the truism that (most of) our evidence in these sciences consists of behavior the false conclusion that the (main or only) subject matter must also be behavior rather than emotions, thoughts, pains, pleasures, etc. (Compare the erroneous inference: (most of) the evidence in contemporary physics consists of pointer readings of measuring instruments; therefore the (main or only) subject matter of contemporary physics must be pointer readings rather than electrons, neutrinos, mesons, energy, heat, etc.; or: (most of) our evidence for occurrences in the Vietnam war consisted of words in newsprint and sounds from electronic devices such as radios, T.V. sets, etc.; therefore what happened in Vietnam consisted of words in newsprint and sounds from electronic devices rather than bombed-out villages, dead children, habitual military and administrative lying, etc.)

\(^6\) See, for example, my “Structural Realism and the Meaning of Theoretical Terms,” and “The Later Russell: Philosophical Revolutionary.”

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investigation, and developments in response to them should proceed along with other developments in the science in question.

Does the abandonment of empiricism land us in the arms of its arch-rival, rationalism? No, certainly not in the classical sense. We do accept, at least until something better comes along, the postulates required for our theory of confirmation and the estimates of required prior probabilities even though they are not justified by experience (because they are not justified in any classical sense at all). But though they are prior to experience in this sense, they are not a priori in the classical sense at all. We have seen that they are nonnecessary (contingent) even though they are nonempirical (not justifiable by experience) and may, as far as a priori knowledge (or any kind of certain knowledge) is concerned, be false.

I have written at length elsewhere about the implications of confirmation theory for questions about the nature of philosophy (and of science and common sense). The issue is so important, however, that a few remarks about it will be included here. My contention is that a great many, though not all, problems that are usually considered to be philosophical, differ, if at all, only in degree and not in kind from problems that are considered to be purely scientific ones. Among these are the mind-body problem, many of the "philosophical" problems about perception and reality, etc., some crucial components of the controversies between realism and instrumentalism, realism and phenomenalism, etc., important aspects of theories of meaning and other parts of "philosophy" of language, and, as we have seen, several vital issues concerning confirmation theory itself. As long as one holds the belief that observations plus logic (including "inductive" logic) are sufficient to confirm or disconfirm our contingent knowledge claims (i.e., as long as one remains a judgment empiricist) it does seem quite natural to consign all these contingent claims to the realm of the "empirical," which, in turn, is held to include only scientific and common-sense knowledge. Such a move leaves for philosophy only those truths (or falsehoods) which are non-contingent and, given the empiricist view of contingency and necessity, only those statements that are logically true (or false) or analytic. Philosophical activity could consist then, only of "logical analysis," or "con-

ceptual analysis," or the “analysis of language,” etc. — certainly a very
natural view given prevailing philosophical fashions but one that collapses
immediately once the false assumption of judgment empiricism is aban-
donned.

Even when I held such a view, I was very uncomfortable at having to
defend the thesis that a question such as whether there existed an ex-
ternal, mind-independent world or whether all of reality was “in our
minds” or consisted of certain arrangements of sense impressions, etc.,
was either a “pseudoquestion” resulting from a “misuse” of language or
a question that, properly interpreted, was about the language we use, or,
perhaps ought to use, or “find more convenient” to use, etc. — and even
more uncomfortable about similarly “explaining away” the question
whether the mind and the body are separate and distinct or whether they
are in, some sense, one. I am sure that I have been as guilty as anyone
else, but the machinations of philosophers supporting these contorted
interpretations are indeed sights to behold; especially remarkable are
the intuitively outrageous, extraordinary interpretations that the “ordi-
nary language” philosophers find it necessary to put upon ordinary lan-
guage itself. I am not concerned to deny that logical, conceptual, and
linguistic “analyses” are sufficient to “solve” some “philosophical” prob-
lems or that they play a role, sometimes a vital one, in the problems just
discussed. Indeed they do, but they also play equally important roles in
many problems generally acknowledged to be scientific ones — special
and general relativity are spectacular examples, but there are many more
less dramatic ones.

Some philosophers, like Quine and Goodman, found this view of the
function of philosophy and the resulting certainty which a correct “philo-
sophical analysis” should be able to bestow upon those few significant
conclusions left in its province difficult to accept. They also recognized
some of the implications of some of the difficulties in confirmation
theory. Their (notorious) solution was to blur the distinctions between
contingent and necessary, between analytic and synthetic, etc. This move
also deemphasized the importance of analyses of meanings and creates
a continuum containing philosophical, scientific, and common-sense
problems (or statements). I believe that their move, however, is a bad
way of reaching good conclusions. Here I stand with the empiricists
against it. However, I do not want to debate the matter here but,
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rather, to point out that the same "good" conclusions follow from the theory of confirmation advocated in this paper, leaving the analytic-synthetic distinction untouched.

Once we recognize that just the evidence (plus logic) not only does not produce general knowledge but does not even select from among competing knowledge claims, our views about the classification of theories and hypotheses change drastically. We realize that empiricist criteria of meaning based on confirmation classify even all significant scientific problems as pseudoproblems (since confirmation in the empiricist sense is impossible) and that Popper's criterion of demarcation would make them all "metaphysical" (since scientific theories are not, in general, falsifiable). I believe that the most important principle, if one must have a principle for distinguishing science from philosophy, is that questions that seem to be more easily decidable on the basis of the evidence are ones that we are inclined to call "scientific" and those which seem more tenuously related to the evidence are ones that have been, traditionally, classified as philosophical. One or more, or all, of the following usually hold for propositions more commonly classified as philosophical than as scientific: (1) They are conjoined with a greater than usual number of auxiliary theories, hypotheses about unobservable initial conditions, etc. (2) General competing propositions (theories) are usually under consideration none of which has an obvious or widely agreed upon advantage vis-à-vis its prior probability. (3) It is relatively easy to adjust, replace, etc., the auxiliary theories and assumptions about initial conditions to protect, in turn, each of the competing theories against prima facie difficulties with the evidence. (4) They are usually propositions about matters that seem quite basic and fundamental to us for one reason or another. They may be concerned, either pro or con, with our most cherished or most tenaciously held beliefs, sometimes tacit beliefs of which we become aware only after we fall into the company of philosophers, such as our belief in the existence of the external world and of other minds. Note that (1) and (3) have to do with our abilities

65 I have done so at length, with a fervor that is now incomprehensible to me, in my "Meaning Postulates in Scientific Theories," in H. Feigl and G. Maxwell, eds., Current Issues in Philosophy of Science (New York: Holt, Rinehart, and Winston, 1961), and "The Necessary and the Contingent," in Feigl and Maxwell, eds., Minnesota Studies in the Philosophy of Science, vol. 3. I am now much more sympathetic with the view of Hilary Putnam ("The Analytic and the Synthetic," in ibid.,) that not as much hangs on the issue, one way or the other, as most philosophers seem to think.
in these special cases to think of auxiliary theories and possible (unob-
servable) initial conditions that seem not too unreasonable to us (to
which we assign not too low prior probabilities), while (2) is totally
and explicitly concerned with our estimates of prior probabilities of the
competing theories of interest. The anthropocentrism involved in (4)
requires no comment. It begins to appear that even the difference in
degree between philosophy and science or common sense depends to a
large extent on our (subjective) estimates, interests, and beliefs. Again,
I am not claiming that this is the whole story. Perhaps, for example,
relative to the main body of our knowledge (whatever this may be) there
are some logical differences, at least in degree, between philosophical
problems and scientific ones, but they are not the ones that have been
proposed by contemporary philosophy. As far as logic (including “induc-
tive logic,” if only it existed) relative to actual and possible evidence is
concerned, scientific and philosophical problems are on a par. By skillful
manipulation of auxiliary theories and assumptions about unobservable
initial conditions, not only philosophical theories but also the most mun-
dane scientific theory not only can be “saved” no matter what the evi-
dence, but can also be made to account for any possible (relevant)
evidence. In practice, of course, we soon balk and refuse to consider
what we judge to be silly, contorted, unnatural, etc., auxiliary theories
and assumptions. However, silliness, contortion, unnaturalness, and even
extremely low prior probability are not logical notions. It is mainly be-
cause of the constitutional, perceptual, and mental capabilities we happen
to have (plus, perhaps, some historical accidents) that we consider the
competition between, say, realism and phenomenalism to be philosophi-
ical and that between relativity and Newtonian mechanics to be scientific.
In each case each competitor accounts for all the evidence and can be
made to do so come what evidence there may be. Some of us may feel
that realism has a much higher prior probability than does phenomenal-
ism and that it accounts for the disappearance of the burning logs in the
fireplace in an observer-empty room in a “more natural,” less convoluted
manner than does phenomenalism — that the auxiliary theories and as-
sumptions used by realism in this case are more “reasonable” or have a
higher prior probability than those that a phenomenalist would have to
employ. But exactly the same sorts of considerations could be applied to
the comparison of relativity’s explanation of the advance of the peri-
helion of Mercury’s orbit with the one that could be (and, indeed, has
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been)\textsuperscript{66} advanced by the Newtonian. In these, as in all cases, we cannot make decisions without tacitly or explicitly making estimates of the prior probabilities that are involved.

To bring the main body of this paper to a close, I repeat a perhaps somewhat oversimplified but elegant and succinct explanation by Bertrand Russell of the fact, if it is a fact, that we have any general knowledge and are sometimes able to make successful predictions: “Owing to the world being such as it is, certain occurrences are sometimes, in fact, evidence for certain others; and owing to animals being adapted to their environment, certain occurrences which are, in fact, evidence of others tend to arouse expectation of those others”\textsuperscript{67} (italics not in the original).

Appendix

A few more of the more popular approaches to confirmation theory will be briefly considered in this appendix, although no attempt is made to provide anything like a complete survey.

Many philosophers seem to find appeals to simplicity very attractive. “Simply select the simplest theory,” they say; “what simpler way could one desire to eliminate all but one of whatever competing theories there may happen to be around?” Perhaps. But such a procedure does not seem to move one whit toward answering the problem posed by Hume nor does it fare any better with what we have argued is the prime question of epistemology: What are the conditions to be fulfilled if knowledge (or true belief) is to exist? Unless it is contingently postulated to be the case, what reason under the sun is there to suppose that the simplest theory is any more likely to be true than any of the other infinite number of competitors? A contingent postulate that asserts such a higher prior probability for simpler theories may be true—or may be well advised, etc. But it is contingent, and if we fall back upon it to support our inferential procedures, we have certainly abandoned empiricism. On the other hand, if we disallow such a postulate, but claim that

\textsuperscript{66} See, e.g., Feyerabend, “Against Method.”
\textsuperscript{67} Human Knowledge: Its Scope and Limits. This quotation appeared as part of the epigraph of my “Theories, Perception, and Structural Realism.” I emphasized the adaptive and (what is not necessarily the same thing) the evolutionary aspects of epistemology in “Corroboration without Demarcation” but, for reasons of brevity (believe it or not), have omitted consideration of them from this paper.
the likelihood of truth of simpler hypotheses is supported to some extent by decades of evidence from the history of science, this is at least reasonably arguable. However, then it cannot be used, on pain of vicious circularity as a basis for confirmation theory. Simplicity may be beautiful, and it certainly makes things easier. Moreover, a certain minimum amount of it in nature is no doubt a necessary condition for the existence of knowledge. But none of this provides a justification for making it the basis of a decision procedure for selecting among competing theories. For example, the existence of the bare minimum of simplicity necessary for the existence of knowledge is perfectly consistent with the simplest theory's always being false. If a congress of scientists or philosophers (or even elected politicians) legislated that such a decision procedure was henceforth to be employed, I would practice civil disobedience and politely but firmly demur, preferring to use, rather, my feeble hunches in each case about which competing theory had the highest prior probability, and I would also want to learn about the estimates of prior probabilities by other investigators in the area instead of seeing them muzzled by simplicity requirements. To resort to banalities, I would much rather trust the results of the free competition of diverse ideas than to trust any uniformity that might result from slavishly following a "rule of simplicity." By this time it should be abundantly clear that the search for "the scientific method" whereby a given body of evidence univocally selects the best theory or hypothesis is a search for a chimera at least as elusive as the philosopher's stone or the fountain of youth.

Be all this as it may, however, I do not believe that, in most cases, there is any satisfactory, univocal way of deciding which theory should be called the simplest. It may be easy to decide, when we are dealing with points on a graph, that a straight line is simpler than a very "curvaceous" Fourier curve. However, neither theories and hypotheses nor the evidence for them comes neatly packaged, very often, as curves or series of points on a graph.

If someone proposed that, when we have proposed before us two or more viable competing hypotheses or theories and our estimates of their prior probabilities are all equal, we should then select the simplest one (provided we can decide which is simplest), I would probably agree for

68 Though as so stated, false, I believe. Simple theories usually require subsequent qualifications and, thus, complications. Moreover, they are usually applicable only to "ideal" and, strictly speaking, nonexistent systems.
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reasons of convenience or of laziness, but, as far as deciding which to bet on as being more likely to be true or, even more likely to stand up best under further testing, I would just as soon toss a coin.

Regarding the approach of the personalists, I have already discussed some of the similarities and some of the differences between their approach and the one defended in this paper; and I should like to emphasize and gratefully acknowledge the importance and precedence of their work. As explained earlier, taking a relative frequency interpretation of the probability of propositions to be the fundamental one, I give a (contingent!) rationale for supposing (or hoping) that our estimates of prior probabilities bear some, not-too-hopeless relationships to reality. To put it in another way, I try to remove the ontological, though not the epistemic, subjectivity from confirmation theory. We may think of knowing subjects (human beings and at least some other animals) as being very imperfect, though not completely hopeless, measuring instruments for determining prior probabilities. Inaccurate though they may be, the relative frequencies (or, perhaps better, propensities to realize relative frequencies) that they attempt to measure are just as objective and mind-independent as is tomorrow's weather, whatever it may turn out to be.

As strange as it may seem, I do not feel that a detailed consideration of Carnap's celebrated attempts to systematize "inductive logic" has a place in this paper. The reason is that I believe that his approach is a nonempiricist one. The axioms for his system are admittedly and avowedly not logically valid in the sense of deductive logic and are, thus, in this sense synthetic. They are, moreover, according to him, a priori. This paper has just assumed without discussion that classical rationalism is untenable, so I shall not bother with the obvious implications of these two points. Carnap's move is to claim that these basic principles of inductive logic are validated and their use made possible by our inductive intuitions just as, he claims, we must have deductive intuitions in order to employ successfully (and to see the validity of) deductive principles. My position is, of course, that at least some of the axioms (or tacit assumptions) of his system are not only synthetic but also contingent.


70 For more detailed discussion of some of the weaknesses of the personalists' approach and of the desirability of a frequency interpretation, see Salmon, *The Foundations of Scientific Inference*. 

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For example, I believe that this is clearly true of the principle that "structure-descriptions" have equal "measures." "Measure," here, is an obvious euphemism for prior probability, and my earlier arguments concerning these and any attempted application of anything like a principle of indifference hold, I believe, with full force here.\(^7\) Carnap's system, then, seems to emerge as a contingent theory of confirmation and to be, moreover, in competition with the one developed in this article. Of my many objections to it, I shall discuss only one here—one that seems to me decisive. In order to apply Carnap's system, given any instance of evidence to be used, we must already know what are the relevant individuals and properties in the universe (as well as the cardinality of the relevant classes of individuals). In other words, we must already have the universe "carved at the joints" and spread out before us (otherwise we could not perform the necessary counting of state-descriptions, structure-descriptions, etc.). But just how to "carve at the joints," just what are the important relevant properties, and just which segments of the world are to be designated as individuals are all contingent questions—or, at least, they have crucial contingent components—and, moreover, are often the most important questions that arise in scientific inquiry. Before any application of Carnap's system can begin, we must assume that a great deal—perhaps most—of that for which a confirmation theory is needed has already been accomplished. We may, as might have been expected, put the objection in a form that, by now, is familiar ad nauseam: Given any body of evidence, there will be an infinite number of mutually incompatible ways of "carving the world" (setting up state-descriptions, structure-descriptions, etc.) each of which will give different results for predictions, "instance confirmations," etc. I do not believe that any set of Carnapian axioms can select a unique method without tacitly relying on the same kinds of estimates of prior probabilities that are explicitly recognized in my theory. Carnap's system cannot circumvent our result that, if the (hypothetico-inferential [or simple inductive]) search for knowledge is to meet with any significant success, then we must be able by extralogical means to select theories that have a relatively high prior probability.

The preceding statement applies with full force to the "ordinary language solution" or "dissolution" of Hume's problem. Even if one ac-

\(^7\) For more detailed and forceful objections to Carnap's approach, see, again, Salmon, The Foundations of Scientific Inference.
cepted completely its "analysis" of rationality — which I, for one, do not — the results and implications of the paper would stand unaffected. If "being rational" just means using those kinds of reasoning that we, in fact, usually do employ, then, if I am correct in my contention that, both in science and in everyday life, we do do something very much like applying the frequency version of Bayes's theorem, estimating appropriate prior probabilities, etc., it follows that my theory provides the standard of (nondeductive) rationality and that to apply it is, by definition, to be rational. But I would take scant comfort from this, even if I accepted this definition of 'rational'. A rationale, or a justification, or a solution or "dissolution" provided by a definition is bound to be pretty trivial. How can the practice of labeling our commonly practiced inferential procedures with the (soporific?) word 'rational' do anything toward answering the searching questions of Hume? Linguistic philosophers are inclined to retort that Hume merely showed that induction is not deduction and that we should have known this all along. Well and good, provided they go ahead and recognize the consequences of this. If they recognize that these consequences mean giving up strict inductivism and strict confirmationism, in short, giving up empiricism and acknowledging the irreducibly subjective epistemic components that must be present in whatever remains of the "context of justification" and if they recognize that our "ordinary" modes of reasoning involve, at least tacitly, something very much like the (frequentist) application of Bayes's theorem, then the only dispute that will remain will be the relatively minor one about what the word "rational" means in ordinary speech.

This does suggest, however, a question that is rather fervently debated.

72 Once again, Wesley Salmon has provided a devastating critique of this popular "solution." See his "Should We Attempt to Justify Induction," Philosophical Studies, 8 (1957), 33-44.

73 Strawson, on the other hand, seems to hold that our ordinary methods of (nondeductive) reasoning are what I have called "simple inductive" ones (see his Introduction to Logical Theory (New York: John Wiley, 1952), ch. 9). (He does not even mention hypothetico-deductive reasoning, but this does not significantly affect the main point at issue here.) In view of what we have seen earlier, this would mean that the "rational" way to reason is totally impotent or, rather, that if it is not to be so, it must be bolstered by the powerful contingent assumptions that, in effect, convert it into the set of confirmation procedures advocated in this paper. In other words, if Strawson is right, then what he would call "rational behavior" would be, for me, irrational in the extreme unless my assumptions — which he does not consider, of course — are added.

74 In all fairness, one way or the other — or both — I should note that "context of justification" is a term of Reichenbach's rather than one of the British Analysts.
in some circles today: Are there (objective and permanent) “standards of rationality” or is “rational” more like a label for those modes of reasoning that happen to be fashionable in a given era (within, perhaps, a given group such as “the scientific community,” etc.)? As Imre Lakatos has put it: Should we espouse epistemological authoritarianism (or dogmatism, etc.) or epistemological anarchy? If pressed for an immediate or unqualifying answer, I would, of course, have to opt for anarchy. But this would be unfair, for the question is, obviously, loaded. Surely there are positions intermediate between total anarchy and total regimentation. Few of us, I suppose, would be so anarchistic as to advocate disregard of the principles of deductive logic, and most of us, I should hope, would require that proposed theories and hypotheses satisfy or give promise of satisfying the requirements of hypothetico-inferential explanation. How many or how few more requirements or “standards of rationality” I would endorse is not a matter to be discussed here, nor is it a particularly crucial one for the present issue. What must be emphasized at this point is the other side of the ledger: no acceptable set of ‘standards of rationality’ can be strong enough to eliminate the existence, for any given amount of evidence, of an infinite number of viable, mutually incompatible theories: the proposal and choice of theories from this number must be made on an extralogical, nonrational basis. The proposal of theories is, of course, the (subjective) “creative leap” of the intellect of the knower; this much is fairly uncontroversial even among (enlightened) empiricists. We have seen, however, that if knowledge is possible, the frequency of success of such leaps must be surprisingly high given the number of possibilities. Furthermore, to select from among proposed theories or to make even a wild guess to what degree a given theory is confirmed by the evidence at hand, we have seen that we must rely on (subjective) extralogical, nonrational estimates of the appropriate prior probabilities. Thus, the importance, indeed, the indispensability of the

Private communication. Lakatos's widely discussed approach, admittedly and avowedly a development beginning with Popper's views, is based on what he calls “progressive and degenerating research programs.” (See his “Falsification and the Methodology of Scientific Research Programs.”) It is an improvement, at least in some respects, on Popper's system, because it recognizes the impossibility of viable falsifiability in the important and interesting areas of science. Like Popper's, however, it remains within what I have called the “strict confirmationist” and, therefore, within the empiricist fold. It, thus, fails to come to grips with Hume and leaves unanswered the fundamental epistemological question: What are the conditions to be fulfilled in order for knowledge to be possible?
subjective, the extralogical, the nonrational (but not irrational) for the growth of knowledge (both in the "context of discovery" and in the "context of justification") cannot be overly stressed. All attempts to eliminate this element and to establish "standards of rationality" that provide a univocal procedure for selecting theories on the basis of evidence at hand are doomed just as surely as was the specific instance of such an attempt, based on simplicity, that we considered earlier. The search for rationality, then, in this strong sense, must be abandoned. But just as we saw that there are senses, less stringent than those desired by empiricists and others, but still plausible and useful senses, of ‘knowledge’ and ‘justified’, we could also easily adduce a similar sense for the word ‘rational’. However, the concern of this paper has been the fundamental question of epistemology: What are the conditions to be fulfilled in order for significant knowledge (or true belief) to exist? And the answers to this question, which are the fundamental principles of epistemology, are mainly descriptive rather than normative. Therefore, the redefinition of the essentially honorific term ‘rational’ can be left for another occasion.

As a final footnote and as a close to this paper I should like to quote Russell’s wonderful remarks on the nature of knowledge contained in the introduction to his Human Knowledge: Its Scope and Limits: "... ‘knowledge’ in my opinion, is a much less precise concept than is generally thought, and has its roots more deeply embedded in unverbalized animal behavior than most philosophers have been willing to admit. The logically basic assumptions to which our analysis leads us are psychologically the end of a long series of refinements which starts from habits of expectations in animals, such as that what has a certain smell will be good to eat. To ask, therefore, whether we ‘know’ the postulates of scientific inference is not so definite a question as it seems. The answer must be: in one sense, yes, in another sense, no; but in the sense in which ‘no’ is the right answer we know nothing whatever, and ‘knowledge’ in this sense is a delusive vision. The perplexities of philosophers are due, in a large measure, to their unwillingness to awaken from this blissful dream." (Italics added.)