

PHILOSOPHICAL STUDIES

Edited by WILFRID SELLARS and HERBERT FEIGL with the advice and
assistance of PAUL MEEHL, JOHN HOSPERS, MAY BRODBECK

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C. Salmon, BROWN UNIVERSITY

Should We Attempt to Justify Induction?

by WESLEY C. SALMON
BROWN UNIVERSITY

IN THE broadest sense, an inductive inference is any non-demonstrative inference to a matter of fact. An inductive rule, then, would be any non-deductive rule of inference for drawing matter of fact conclusions, provided that such a rule does not sanction drawing self-contradictory conclusions from any consistent set of premises (including the null set). I regard the problem of justifying induction as the problem of justifying a choice from among the wide variety of possible inductive rules. The question whether past experience is to be a guide to the future is included in the problem thus formulated, for among the possible rules are some which render evidence about the past irrelevant to predictions of the future.*

In recent years a rather large number of philosophers have argued that the attempt to justify induction ought to be abandoned. They have supported this claim by arguments designed to show that a justification of induction is either impossible or unnecessary or both. Within this paper

* The "dagger-method" described in (3) is an example.

I shall call such philosophers "anti-warrantists"; those who believe it worthwhile to persist in attempting to find a justification of induction will be called "warrantists." The anti-warrantists have frequently charged that there is no genuine problem of justifying induction—if there appears to be a problem it is because of a misconception of the nature of induction or justification, or because of a similar kind of confusion. To whatever extent there is a "problem," it is solved by exposing the confusions which led us to demand a justification in the first place, not by producing a justification. It will be the purpose of this paper to examine the arguments of the anti-warrantists to see whether they have, indeed, disposed of the problem of induction. The arguments of the anti-warrantists fall into two major groups. The first contains arguments designed to show that a justification of induction is impossible; the second contains those designed to show that it is unnecessary.

1.1. The anti-warrantist's most common argument is that Hume has shown the impossibility of proving that inductions will ever again be successful; hence, the warrantist is bound to meet with failure in his attempt to justify induction. However, certain warrantists can reply that they are not attempting this impossible task. They propose a different kind of justification—one not open to the kind of objection Hume raised. In particular, authors like Feigl, Kneale, and Reichenbach—called "practicalists" by Max Black (2a)—have held that, because of certain deductive relations between inductive rules and the aims of inquiry, some rules are superior to others *for the purposes inductive inference is designed to serve*.

But, it might be objected, getting true conclusions from true premises as often as possible is the aim of induction. How could any rule be shown to be suited to this aim without at the same time showing that this rule will, in fact, produce true conclusions? The practicalist attempts to answer this objection by proving that, although we cannot guarantee the success of any method, still, we can demonstrate that some rules are bound to lead to true conclusions if any rules will. If we have a set of possible rules $\{R_i\}$, and if it can be shown that the situations on which any of these rules will produce true conclusions is a subset of the situations in which R_1 will yield true conclusions, then R_1 is justified, for it is as good or better than any of the others for drawing true conclusions. This is precisely the way in which a practicalist such as Reichenbach tries to justify a standard inductive method such as his *Rule of Induction* (7, p. 444).

In his essay "Pragmatic Justifications of Induction" (2a), Max Black presents a detailed criticism of the practicalist theory of induction. He takes issue with four contentions which he associates with practicalism (2a, pp. 157–58):

- i. Some experts have held that inductive policies are bound to be applicable in all possible worlds.*
- ii. Inductive policies have often been praised for being "self-correcting."
- iii. It has been urged that the inductive policies at any rate satisfy the necessary conditions for prediction and generalization; so that anybody following them can be sure of having done everything in his power to discover factual truth, although he can have no guarantee of success.
- iv. Sometimes it is conceded that there are methods alternative to induction (which the last argument in effect denied), but it is added that all such methods ("clairvoyance," etc.) must in turn be tested inductively, and are therefore to be regarded as pseudo-inductive methods.

i. The first thesis—that induction must be applicable in every possible world—is, I believe, mistaken, but it is no necessary part of the practicalist position. The practicalist does not hold that induction must work. He holds that if any method works, induction does. With Black, we may reject the first thesis as incorrect, but this does no damage to practicalism. We might add that Black in his essay "How Difficult Might Induction Be?" has pointed out that there are possible worlds in which prediction is impossible (2c). In order to refute the practicalist, however, it would be necessary to describe a possible world in which induction would not work but some other method would.

ii. According to the second thesis, induction is self-correcting. Black objects that the only sense in which induction is self-correcting is that additional data bring about revisions in the conclusions. But there is no guarantee that these revisions bring one closer to the correct conclusions; they may take us farther from the truth. There are any number of methods which merely revise their conclusions as more data accumulate, but there is no particular virtue in this characteristic unless the revision constitutes an improvement. Black illustrates this point by contrasting two widely different methods. The first is a standard inductive method which directs us to use the relative frequency in the observed sample as our estimate of the limit of the relative frequency. Black describes a second method which he calls the "counter-inductive method." According to this method, the smaller the observed relative frequency of an event, the greater is our estimate of the limit of the relative frequency. Unfortunately, Black's method leads to contradiction (8); however, there are consistent methods which have the same general feature and which will serve our purposes as well

* In his discussions, Black uses "inductive policies" to mean standard inductive methods such as Reichenbach's *Rule of Induction*.

as Black's counter-inductive method.* Hereinafter, when we speak of a counter-inductive method we will be referring to a consistent analogue of Black's method. Black points out that each of these methods, the standard method and the counter-inductive method, leads to revised estimates of the limit of the relative frequency as it is applied to new samples with different constitutions. Hence, Black concludes, the two methods are self-correcting in exactly the same sense.

However, Black has overlooked an important sense in which the standard method is self-correcting and in which the counter-inductive method is not. If the relative frequency in a sequence converges to some limit, the standard method will eventually lead to estimates of that limit which are accurate within any desired degree of approximation, however small. This statement is demonstrably true. Hence, persistent use of the standard method in such a sequence must sooner or later yield estimates which are close to the correct value of the limit. On the other hand, the counter-inductive method leads, in general, to estimates which always differ by at least some positive quantity from the observed relative frequency. The counter-inductive method may produce some accurate estimates of the limit, but there is no guarantee that it will ever do so. Furthermore, it can be shown that, if the sequence has a limit (other than one half), persistent use of the counter-inductive method will lead to estimates of the limit which are consistently inaccurate. This is the sense in which the standard inductive method is self-corrective and the counter-inductive method is not.

iii. The third thesis—that inductive policies satisfy the necessary conditions for prediction and generalization—is a crucial one. Unfortunately, Black's discussion of this thesis involves a major misinterpretation and a related major oversight. As Black construes the thesis it means that the use of induction is a necessary condition of successful prediction of the future—that the standard inductive method is the only method that will produce correct predictions of the future. This is not what the practicalist maintains. The practicalist holds that the ability of the standard inductive method to predict the future is a necessary condition of the predictability of the future. The practicalist does not claim that the inductive method is the only method which can correctly predict; he claims that the inductive method can predict successfully if any other method can. Hence, the demonstration that some other method might predict correctly is no refutation of the practicalist position. The practicalist maintains that, if the inductive method fails, so will every other method. In order to refute the

* The "diamond-method" described in (3) is one such.

practicalist, therefore, it is necessary to show that some other method could succeed where the inductive method would fail.

As a result of this misunderstanding, Black claims that the practicalists have unduly restricted their definition of the aims of cognitive inquiry in order to prove that the use of induction is a necessary condition of successful prediction. In particular, he criticizes Reichenbach for characterizing these aims as the ascertainment of limits of relative frequencies. He quotes the following statement from Reichenbach (7, p. 474): "Scientific method pursues the aim of predicting the future; in order to construct a precise formulation for this aim we interpret it as meaning that scientific method is intended to find limits to the frequency."

Then Black comments (2a, p. 175): "He is narrowing the aim, not just formulating it more precisely. This narrowing of the aim makes it possible for Reichenbach to view the search for limits of the frequency as a necessary condition for the success of inductive method. But anybody who says he wants to predict the future but is not interested in finding the limits of relative frequencies of occurrence of characters in infinite series is not contradicting himself. Reichenbach is not analyzing scientific method but redefining it for his own purposes."

Black might be right in maintaining that not every problem of prediction can be analyzed in terms of the problem of finding limits or relative frequencies, but he offers very little reason for his contention. Reichenbach, on the other hand, has argued at great length that all problems of prediction can be reduced to this form. Since the argument is long and technical we can only briefly indicate a few of its main points. First, Reichenbach argues that the theorems of the calculus of probability are tautological when the single non-logical primitive, the probability implication, is interpreted as the limit of the relative frequency. From this it follows that all probability derivations can be construed as derivations from premises about the limits of relative frequencies. In addition, Reichenbach, following Venn, gives an interpretation of probability of single events in which this concept is defined in terms of probability referred to infinite sequences. And finally, the probability of scientific theories is interpreted in terms of Bayes's Theorem, showing that the assignment of a probability to a theory may be regarded as a derivation from premises which deal with limits of relative frequencies. Reichenbach's argument may not be completely adequate, but in the light of this argument it is not as obvious as Black seems to feel that Reichenbach has merely redefined scientific method for his own purposes.

iv. The fourth thesis which Black attributes to the practicalists—the thesis that methods other than the standard inductive methods must be

tested by induction—is again a case of a misunderstanding on Black’s part. The practicalist need not, and does not, assert that alternative methods must be based upon or tested by induction. The practicalist has two theses about alternative methods. First, he requires that a method of inference be justified if it is to be considered methodologically sound. Assuming induction to be justified, inductive testing of these alternative methods would be reasonable. But this thesis has nothing in particular to do with the justification of induction. Second, the practicalist maintains that alternative methods *can*, not *must*, be tested inductively. This seems to be a correct doctrine, for we can investigate inductively the causal or statistical relations between predictions made according to a given method and the events predicted. But the significance of our ability to test alternative methods inductively is this. If someone says, “I can conceive of a world in which clairvoyance is a consistently successful method of prediction, but in which induction cannot predict successfully,” we can deny this possibility on the ground that we could learn inductively about the success of clairvoyance; hence the inductive method *would* be successful in such a world. This point is usually introduced in support of the contention discussed above, that induction will work if any method will.

Black makes the point that the alternative methods may just as well be utilized to evaluate the inductive method as vice versa. Someone might use clairvoyance to determine whether induction is going to be successful in the future. But if induction is independently justifiable, as the practicalist tries to show, while clairvoyance is not, then, although one *could* use clairvoyance to test the standard inductive method, it would be utterly pointless. It would be a case of testing a justifiable method by one which was unjustified.

This examination of Black’s arguments against the practicalists shows, I believe, that his objections do not hold. He has failed to show that the practicalist approach cannot succeed; indeed, he has not even shown that all practicalist attempts thus far are insufficient. I do not wish to maintain that an adequate justification of induction has yet been given (9). My point is that we have not been given reason for giving up the search.

1.2. Let us now examine a second argument of the anti-warrantist for the impossibility of justifying induction. According to this argument, justification consists in showing that whatever is to be justified conforms to certain already accepted principles or rules. In particular, an inference is justified if it can be shown to conform to the relevant rules of inference. Sometimes these rules can, in turn, be justified by reference to other rules or principles. But to ask for a justification of *all* rules of inference is without sense, for no rules or principles are available in terms of which a justifi-

cation could possibly be given. When we have called into question so much that there no longer remain any rules of principles to which a justification could be referred, then we have reached the limits of justifiability. Thus, to question any particular inductive inference is legitimate, for it can be justified or refuted in terms of the general canons of induction, whereas, to question induction in general leaves no canons in terms of which the justification can occur. This view is held by Strawson (10).

If the foregoing theory is correct, empirical knowledge is, at bottom, a matter of convention. We choose, quite arbitrarily it would seem, some basic canons of induction; there is no possibility of justifying the choice. They are arbitrary in the sense that cognitive considerations do not force their acceptance. It is perfectly conceivable that someone else might select a different set of inductive canons, and if so, there would be no way of showing that one set was better than another for purposes of gaining factual knowledge. Yet, such a person would regard certain inferences as justified which we would regard as unjustified. He would hold certain conclusions to be well established while we would hold the same conclusions to be disconfirmed. This is the sense in which conventionalism follows from the Strawson theory.

Herbert Feigl has given an answer to this contention of Strawson, and it consists in providing a clear sense for the question of the justification of induction in general (4, 5, 6). Feigl distinguishes two kinds of justification. He calls the first of these "validation"; it is the kind of justification Strawson describes. An inference is validated by showing that it is governed by an accepted rule. A rule of inference is validated by showing that it can be derived from other accepted rules or principles. There is, however, a second form of justification called "vindication." This kind of justification consists in showing that a given decision, policy, or act is well adapted to achieving a certain end. Translated into Feigl's terminology, Strawson's thesis becomes the innocuous claim that it is impossible to validate induction in general; only particular inductive rules and inferences can be validated. However, the warrantist is not attempting to validate the basic inductive canons; he seeks to vindicate them. The warrantist intentionally goes beyond the limits of validation, but he does not go beyond the limits of justification. To maintain that he transgresses the limits of justification would be tantamount to a denial that vindication is a kind of justification. It is difficult to imagine any argument that could possibly support such a denial.

The appeal to vindication requires, obviously, some aims or goals in terms of which a vindication can be given. It is at this point that one of the main controversies in the whole philosophy of induction occurs. The

practicalist wants to vindicate induction by reference to the aim of attaining correct predictions and true conclusions. The critic will immediately point out that it is impossible to prove that induction will ever achieve this goal. It might therefore be concluded that there is no possibility of ever vindicating induction.

There are two major alternatives at this point. On the one hand, we may revise our conception of the aim of induction in an attempt to escape the necessity of proving that induction is well suited to the aforementioned purpose of arriving at true conclusions. On the other hand, we may hold, as the practicalist does, that it is possible to show that some inductive rules are better suited than others to the purpose of arriving at true results, even though it is impossible to prove that one will be successful while another will not. Let us consider the first of these alternatives. A large number of authors have suggested that we might justify induction as a tool for establishing *reasonable* beliefs, since it is impossible to show that induction will lead to *true* beliefs. According to this view, induction could be vindicated as leading, not necessarily to true conclusions, but rather to reasonable ones. Strawson, not really content with a view which implies sheer conventionalism, argues for this kind of justification when he is not busy arguing that no justification is needed. The argument is based chiefly upon an analysis of the meaning of "reasonable" which purports to establish that reasonable beliefs, by definition, are beliefs which have good inductive support. Strawson says (10, p. 249), "to call a particular belief reasonable or unreasonable is to apply inductive standards . . ." A little later, he further comments (10, p. 257): "to ask whether it is reasonable to place reliance on inductive procedures is like asking whether it is reasonable to proportion the degree of one's convictions to the strength of the evidence. Doing this is what 'being reasonable' means in such a context."

It seems to me that there are fatal objections to this approach. The term "reasonable" is, after all, virtually a synonym of "justifiable." To have reasonable beliefs is to have beliefs that are well grounded by justifiable methods. "Reasonable," then, partakes of the same ambiguity as "justifiable"—one sense referring to validation, the other to vindication. Thus, believing reasonably in one sense means holding beliefs which are sanctioned by inductive and deductive canons. In this sense, reasonable beliefs are beliefs which have been arrived at by methods which can be *validated* by reference to the accepted principles of inductive and deductive inference. In the second sense, "reasonable" means the adoption of methods and techniques which will most efficiently bring about one's ends and goals. This sense of "reasonable" corresponds to *vindication*. It is clear that using inductive methods is reasonable in the sense of "reasonable"

which corresponds to *validation*. Now the problem of the justification of induction assumes the form "Is there any justification for being reasonable?" It will not do to reply that this question has the obvious tautological answer "It is reasonable to be reasonable." In view of the two distinct meanings of "reasonable" this answer may be no tautology at all, for it may contain an equivocation on the term "reasonable." Therefore, we must not lightly dismiss the question about a justification for being reasonable.

If we ask, "Why be reasonable?" construing "reasonable" in the sense related to vindication, the answer is easy to find. Being reasonable, in this sense, means adopting methods which are best suited to the attainment of our ends. Since we are motivated to achieve our ends, the realization that a method is reasonable constitutes a sufficient reason for adopting that method. To be unreasonable, in this sense, is to invite frustration. If, however, we shift to the sense of "reasonable" which is associated with validation, the answer to the question "Why be reasonable?" is much less clear. Presumably, the answer would be that to be reasonable is to be scientific and to use methods which have worked well for us. To be unreasonable would be to hold beliefs which are ill grounded and which run great danger of being false. But in so saying, have we not begged the very question which is at issue in the problem of induction? Surely there is no particular intrinsic value in being scientific or proceeding in accord with the standard inductive methods. We adopt these methods because we regard them as the best methods for establishing matter of fact conclusions. But when the problem of induction is raised, the question at issue is whether the standard inductive methods are, in fact, well suited to the purpose of establishing these factual conclusions.

It may be that the two senses of "reasonable" which we have distinguished are extensionally equivalent—that procedures are reasonable in the one sense if and only if they are reasonable in the other. But it would be a mistake merely to assume that this is the case. When a term has two distinct definitions it is not permissible to assume that the two definitions are equivalent; if there is such equivalence it must be shown. This is especially true when there are arguments which indicate that the supposed equivalence may not hold. Hume's arguments are just such arguments. If we try to show that such equivalence does hold—if we try to show that the standard inductive methods are those best suited to the purpose of arriving at correct beliefs—we are undertaking the task of the warrantist.

In accord with the philosophic fashion of the times one may be tempted to ask what is the ordinary meaning of "reasonable." Perhaps the ordinary sense of "reasonable" ensures that proceeding according to the standard

inductive rules is reasonable. This is probably true of the ordinary sense. But this only shows that ordinary usage is established by people who are unaware of Hume's arguments. To say that ordinary people are untroubled by Humean doubts about induction may simply mean that ordinary people are philosophically ignorant. They assume that the two senses of reasonable distinguished above are equivalent partly because they have never thought of the distinction and partly because, had they thought of the distinction, they would have been unaware of any considerations which would lead to the conclusion that possibly the two senses are not equivalent. To cite ordinary use in this context, then, does not solve the philosophic question. It sanctions neglect of the philosophic question by virtue of an equivocation.

The attempt to vindicate inductive methods by showing that they lead to reasonable belief is a failure. If we assume that inductive beliefs are reasonable in the sense of being based on justifiable methods of inference, we are begging the question. If we regard beliefs as reasonable simply because they are arrived at inductively, we still have the problem of showing that reasonable beliefs are valuable. This is the problem of induction stated in new words. If we regard beliefs as reasonable simply because they are arrived at inductively and we hold that reasonable beliefs are valuable for their own sake, it appears that we have elevated inductive method to the place of an intrinsic good. On this latter alternative it would seem that we use inductive methods, not because they enable us to make correct predictions or arrive at true explanations, but simply because we like to use them. It sounds very much as if the whole argument (that reasonable beliefs are, by definition, beliefs which are inductively supported) has the function of transferring to the word "inductive" all of the honorific connotations of the word "reasonable," quite apart from whether induction is good for anything. The resulting justification of induction amounts to this: If you use inductive procedures you can call yourself "reasonable"—*and isn't that nice!*

1.3. The third argument which might be used to prove the impossibility of carrying out the warrantist program amounts, essentially, to a denial that there are any fundamental inductive rules or principles with which to begin the process of justification. Black seems to hold this view (2b, pp. 195, 208). The absence of fundamental inductive principles might be accounted for in either of two ways:

i. Suppose we raise a question about the correctness of some inductive rule R_1 . Suppose further that R_1 can be validated by reference to an accepted rule R_2 . Suppose in addition that R_2 can be validated by reference to R_3 , etc. In short, suppose we can continue indefinitely justifying

one inductive method by a different inductive method without ever exhausting the supply. In that case there would be an infinite sequence of inductive rules which does not end in any supreme rule or principle; hence, there would be no place to start in the attempt to give a justification of induction in general. This state of affairs is suggested by Black when he says (1, p. 88), "Every inductive principle can be justified—but not all at the same time."

To avoid confusion on this point it is important to distinguish carefully between particular inductive inferences and rules of inference. It is probably true that one can go on indefinitely supporting the conclusion of one inductive inference by another inductive inference, but frequently all these inferences will be governed by one or a very few rules. It is a very different matter to maintain that there are inexhaustible supplies of rules which make it possible to justify one by another without end. If we are going to test an inductive rule inductively we will ask what its frequency of success has been—this is what an inductive test amounts to. But in so doing we soon arrive at some form of induction by enumeration, and it is hard to imagine what other inductive rule we could bring into play to criticize it. The claim that there could be an infinite sequence of different rules as well as different inferences seems implausible in the extreme.

ii. Black seems to feel that the search for fundamental rules of inductive inference takes us so far in the direction of abstractness that, when we have found any rule which looks as though it is basic, it turns out to be so hopelessly abstract that it is useless. Take the rule of simple enumeration. According to Black, from the mere fact that all A's so far have been B, without any additional information, it is impossible to make any reasonable judgment as to whether the remaining A's will be B. Only if we know what kind of things A's are and in what conditions they have been observed can we decide whether the fact that all of them have been B is evidence that the others will also be B. Black points out that from the time we first learn how to draw conclusions we are taught the circumstances in which we can safely generalize and those in which we cannot. Whenever we make inferences we do so in the light of a good deal of auxiliary knowledge.

This is probably a true factual statement about all our inductive inferences, but it ought not to be confused with logical analysis. When we judge that it is safe to generalize in some circumstances and not in others we are making use of inductive inferences. To be sure, we may have been taught, as infants, when to apply a certain rule and when not to. But in so doing we are applying criteria which have been established inductively, if not by ourselves, then by others. There seems to be no reason to sup-

pose we cannot investigate the evidence which supports such criteria and the inductive methods by which they can be established. Again, it would seem probable that the inductive methods involved soon reduce to some form of induction by enumeration.

It is strange that Black calls it an "assumption" to suppose that there might be a "single supreme principle of induction" (2b, p. 208). Reichenbach, for example, has offered extended arguments to show that induction by enumeration is such a principle. We took note of this argument in another connection when we considered whether the aims of prediction could be correctly characterized as the search for limits of frequencies. Since induction by enumeration is the method Reichenbach proposes for the determination of limits, the same argument supports the contention that every induction can be analyzed in terms of induction by enumeration. This argument, incidentally, precedes Reichenbach's attempt to justify induction and does not presuppose it. It seems that Black is remiss in not giving more serious attention to this important argument against the view he supports.

This concludes our consideration of arguments designed to prove that it is impossible to justify induction. We shall now discuss the group of arguments designed to show that no justification of induction is necessary. Arguments of this second type ordinarily follow arguments of the first type. After we have been told we cannot have a justification of induction the attempt is made to convince us that we never really wanted one in the first place.

2.1. Anti-warrantists have sometimes held that arguments such as Hume's only prove that induction and deduction are distinct and different forms of inference and that inductive inferences do not possess deductive validity. When some of the more skeptical writers have said that inductive conclusions are unjustified and without support, the anti-warrantists continue, all they are really showing is that such conclusions do not have deductive support. But, they do have inductive support. There are two kinds of support, inductive and deductive. There is no reason to cast aspersions on one of these kinds of support and deplore the fact that it is different from the other. When we see this point, the apparent need for a justification of induction disappears. It is recognized as an irrational demand that inductive inferences be transformed into deductive inferences (1, pp. 61-88; 11).

We agree that it would be useless to demand that inductive support be transformed into deductive support, but we must protest that the warrantist—at any rate, the practicalist—is not attempting any such feat. Rather, the warrantist proceeds on the principle that justified modes of

inference may be used to justify other modes. Deductive systems are regarded as purely formal and a priori; hence, in principle they can be established independent of and prior to the establishment of any empirical knowledge. Deductive inference is the tool which is available at the beginning of the task of attempting a rational reconstruction of empirical knowledge. A consistent logic may be utilized because it cannot produce false conclusions from true premises; its justification consists in just this. Hence, when the warrantist attempts to find a deductive justification of inductive inference, he is simply using a justified system of inference in his attempt to justify a type of inference which is as yet—within his philosophical investigation—unjustified. He is not relegating induction to an inferior position; he is simply taking up problems of justification in a logical order.

If we refer back to the statement of the problem of justification as it was set out at the beginning of this paper, we realize how inappropriate are the charges of the anti-warrantists on this count. We might perfectly well agree that there is such a thing as inductive support and that this is quite distinct from deductive support. But the problem is, which of the vast range of possible inductive rules is it that defines this kind of support? This kind of question does not involve any confusion of induction with deduction or any desire to transform induction into deduction.

2.2. Finally, let us examine Max Black's contention that it is possible to provide inductive support for inductive rules (2b). This argument is classed as an argument against the necessity of justification of induction because Black uses it to show that no general justification of induction is needed. A given inductive rule can be established by a self-supporting argument, according to Black. He offers two examples of self-supporting inductive arguments. But, since he admits that one of these examples has an obviously false premise, we shall confine our attention to the other. It runs (2b, p. 197):

In most instances of the use of R_2 in arguments with true premises examined in a wide variety of conditions, R_2 has been successful.

Hence (probably):

In the next instance to be encountered of the use of R_2 in an argument with a true premise, R_2 will be successful.

The rule R_2 itself reads:

To argue from *Most instances of A's examined in a wide variety of conditions have been B* to (probably) *The next A to be encountered will be B*.

This is clearly a case of an argument which conforms to the Rule R_2 and has as its conclusion that R_2 will be successful.

Black holds that this kind of self-supporting argument is neither circular nor trivial. Suppose someone had noticed that in several cases in which R_2 had been used it had led to a true prediction. His immediate reaction might be to regard R_2 as a correct inductive rule. But, wanting to be cautious and not accept rules uncritically, he could look around for many other instances of the application of R_2 . If he does so and finds that R_2 has produced a true conclusion in the majority of newly examined situations in which it has been applied, Black says, is he not justified in regarding R_2 as better substantiated than before? If there is any doubt, consider what would have happened if the new instances he examined had all turned out to be cases of unsuccessful inference. This would have tended to show R_2 as unreliable. Now, if negative cases can tend to disconfirm, positive cases must tend to confirm. Hence, the further investigation of R_2 does, in fact, help to support the reliability of R_2 .

To evaluate Black's analysis, let us examine another "self-supporting" argument. Earlier in the discussion we introduced a counter-inductive method. Let us (somewhat inexactly) formulate a rule which corresponds to this method. We will call this rule " R_3 "; it will read: To argue from *Most instances of A's examined in a wide variety of conditions have not been B* to (probably) *The next A to be encountered will be B*. The "self-supporting" argument which is to correspond to R_3 will be:

In most instances of the use of R_3 in arguments with true premises examined in a wide variety of conditions, R_3 has been unsuccessful.

Hence (probably):

In the next instance to be encountered of the use of R_3 in an argument with a true premise, R_3 will be successful.

It is to be noted, first, that this argument is governed by R_3 and, second, that if R_2 and R_3 are both applied in the same situations, the premises of both "self-supporting" arguments will be true but the conclusion of at least one must be false. R_2 and R_3 are conflicting rules in the sense that from the same premises they will almost always produce contrary conclusions. But, to a person who holds R_2 exactly the same evidence will support R_2 as will support R_3 for a person who holds R_3 . This indicates that neither argument is genuinely self-supporting. For if one were to raise seriously the question "Which is the better of these two rules for making predictions?" he would be unable to get any answer from such "self-supporting" arguments.

The reason these arguments are not genuinely self-supporting is that the main question is begged by the use of the term "support" or its cognates such as "evidence." For an argument to establish its conclusion, either inductively or deductively, two conditions must be fulfilled. First,

the premises must be true, and second, the rules of inference used by the argument must be correct. To be sure, the rule of inference does not have to be stated explicitly as part of the argument, nor does a principle which corresponds to the rule have to be incorporated in the argument as a premise. But unless we are justified in accepting the premises as true and in accepting the rules of inference as correct, the argument is inconclusive. The so-called self-supporting arguments are therefore circular in the following precise sense: the conclusiveness of the argument cannot be established without assuming the truth of the conclusion. It happens, in this case, that the assumption of the truth of the conclusion is required to establish the correctness of the rules of inference used rather than the truth of the premises, but that makes the argument no less viciously circular. The circularity lies in regarding the facts stated in the premises as evidence for the conclusion, rather than as evidence against the conclusion or as no evidence either positive or negative. To regard the facts in the premises as evidence for the conclusion is to assume that the rule of inference used in the argument is a correct one. And this is precisely what is to be proved. If the conclusion is denied, then the facts stated in the premises are no longer evidence for the conclusion. Someone who had adopted a skeptical view of R_2 might regard the fact that R_2 had been successful most of the time in the past as evidence that it won't be successful in the future because, say, it is "running out of luck."

Black concludes his essay "Inductive Support of Inductive Rules" with the statement (2b, p. 208): "any philosopher who seriously questions the admissibility of induction will have equally grave doubts about any induction. Thus an inductive inference to the reliability of some inductive rule will still leave such a sceptical philosopher a prey to his sceptical doubts." Near the beginning of the same essay, Black comments (2b, pp. 193-94): "It is to be presumed that the reader can recognize some instances of inductive correctness; if not, this essay will be of no value to him."

I think it is to be presumed that most readers can recognize some instances of inductive correctness, and when we ask for a justification of induction we do not presume otherwise. We merely ask for the grounds of such recognition. Black seems to feel this request is improper. He has failed to show, however, why it is improper, beyond showing that it is difficult to fulfill. If there is such a thing as inductive correctness and if instances of it can be recognized, it seems likely that there are criteria of inductive correctness which can be found and vindicated. We have, at any rate, failed to find reasons for condemning the search.

The five foregoing arguments are, in my opinion, the most important arguments of the anti-warrantists. Each is inconclusive. Therefore, in an

swer to the major question of this paper—should we attempt to justify induction?—two comments will be sufficient. First, we have not been given any good reason for abandoning the attempt. Second, important questions hang on the justifiability of induction. If induction cannot be justified, inductive beliefs become conventional (as explained under 1.2); if induction can be justified, this conventionalism can be circumvented. Since we need hardly argue the philosophical significance of the doctrine of conventionalism, an affirmative answer to the original question seems inescapable.

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REFERENCES

1. Black, Max. *Language and Philosophy*. Ithaca: Cornell University Press, 1949.
2. Black, Max. *Problems of Analysis*. Ithaca: Cornell University Press, 1954.
 - a. "'Pragmatic' Justifications of Induction," pp. 157-90.
 - b. "Inductive Support of Inductive Rules," pp. 191-208.
 - c. "How Difficult Might Induction Be?" pp. 209-25.
3. Burks, Arthur. "The Presupposition Theory of Induction," *Philosophy of Science*, 20:177-97 (1953).
4. Feigl, Herbert. "De Principiis Non Disputandum . . ." in Max Black, ed., *Philosophical Analysis*. Ithaca: Cornell University Press, 1950.
5. Feigl, Herbert. "Scientific Method without Metaphysical Presuppositions," *Philosophical Studies*, 5:17-28 (1954).
6. Feigl, Herbert. "Some Major Issues and Developments in the Philosophy of Science of Logical Empiricism," in Herbert Feigl and Michael Scriven, eds., *Minnesota Studies in the Philosophy of Science, Vol. I, The Foundations of Science and the Concepts of Psychology and Psychoanalysis*. Minneapolis: University of Minnesota Press, 1956.
7. Reichenbach, Hans. *The Theory of Probability*. Berkeley: University of California Press, 1949.
8. Salmon, Wesley C. "Regular Rules of Induction," *Philosophical Review*, 1956.
9. Salmon, Wesley C. "The Predictive Inference," *Philosophy of Science*, forthcoming.
10. Strawson, P. F. *Introduction to Logical Theory*. New York: John Wiley & Sons, 1952.
11. Will, Frederick L. "Generalization and Evidence," in Max Black, ed., *Philosophical Analysis*. Ithaca: Cornell University Press, 1950.