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Social Knowledge and the Role of Inductive Inference: An Appraisal of Two Contemporary Approaches

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Abstract – Part of the intellectual legacy left behind by David Hume is a powerful skeptical argument which casts doubts on the validity (or, more appropriately, justification) of a basic form of inductive inference. Brian Skyrms and Laurence Bonjour have outlined several possible defenses of what they call the inductive principle (IP), in response to the broader Humean challenge. In this paper I elaborate Skyrms' inductive justification and pragmatic defense of IP, as well as Bonjour's novel a priori argument for IP. In the course of critically assessing the cogency of these three strategies, I argue that each one is problematic and fails to provide an adequate defense of IP. I conclude by briefly considering what would beminimally required for a serious rebuttal to the skeptical argument.

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I. Introduction: The Humean Challenge

Part of the intellectual legacy left behind by David Hume is a powerful skeptical argument which casts doubts on the validity (or, more appropriately, justification) of one our basic forms of reasoning. In science as well as in every day affairs, we find it both necessary and useful to make predictions, or to draw inferences based upon observation and experience. For instance, I believe that another morning will be followed by another night, or that the food which has nourished me in times past will also nourish me when I partake of my next meal. In general, the process which underlies such reasoning goes something like this: all other things being equal, given that m/n observed instances or events of type A are or have been B, we can infer that m/n A's are (or have) B. Lets call this the Inductive Principle (IP). Very roughly put, unobserved cases or instances of a certain type will resemble observed cases or instances of the same or similar type. The basic inferential structure of inductive reasoning will be captured by a principle such as (IP) (or, maybe more accurately, a family of related principles). Systems of inductive logic formulate rules which assign inductive probabilities to arguments based on the strength of the evidence or degree of support that the premise(s) provide for the conclusion. Arguments which are assigned a high inductive probability will yield true conclusions from true premises most of the time.

Now Hume claims that making inductive inferences is a habit or custom which is part and parcel of the way we reason. Whether self-consciously or unreflectively, we utilize this process in acquiring, maintaining, revising, and discarding beliefs about what is either unobserved or as yet future to us. But what justifies us in reasoning this way? Is there any rationale for thinking that drawing inferences on the basis of (IP), or assigning high inductive probabilities to arguments from some system of inductive logic S having rules R, will give us conclusions that are likely to be true given that the premise(s) are true (or that the probability of a certain conclusion, given that the premise(s) are true, is at least greater than it would be otherwise)? This in a nutshell is the traditional problem of induction. In section IV of An Enquiry Concerning Human understanding, Hume offers an argument (maybe more than one) which purports to show that there is no rational justification for inductive reasoning. One key passage goes as follows:

"[Past experience] can be allowed to give direct and certain information of those precise objects only, and that precise period of time, which fell under its cognizance; but why this experience should be extended to future times, and to other objects, which for aught we know, may be only in appearance similar; this is the main question on which I would insist... The consequence seems nowise necessary... I shall allow, if you please, that the one proposition may be justly inferred from the other; I know, in fact, that it always is inferred. But if you insist that the inference is made by a chain of reasoning, I desire you to produce that reasoning. The connexion between these propositions is not intuitive."

Relations among matters of fact are not necessary. It is always possible that the future be unlike the past, or that unobserved cases of a certain sort not resemble observed cases of a similar sort. So it is clear that no piece of demonstrative reasoning can establish either (IP) or any inductive conclusion. Similarly,

"When a man says, 'I have found, in all past instances, such sensible qualities conjoined with such secret powers; [thus] similar sensible qualities will always be conjoined with similarly secret powers', he is not guilty of a tautology, nor are these propositions in any respect the same. You say that the one proposition is an inference from the other. But you must confess that the inference is not intuitive; neither is it demonstrative. Of what nature is it, then? To say it is experimental, is begging the question. For all inferences from experience suppose, as their foundation, that the future will

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resemble the past, and that similar powers will be conjoined with similar sensible qualities. If there be any suspicion that the course of nature may change, and that the past may be no rule for the future, all experience becomes useless, and can give rise to no inference or conclusion. It is impossible, therefore, that any arguments from experience can prove this resemblance of the past to the future; since all these arguments are founded on the supposition of that resemblance."

Although Hume's focus is narrower in the sense that he is primarily concerned with inferences from cause to effect, his argument can be construed more broadly. On the one hand, Hume says that we cannot justify (IP) or inductive reasoning by means of any a-priori demonstration. In a valid deductive argument, the conclusion can make no factual claim that is not already implicitly contained in the premises. But the premises of an inductive argument contain only information about past and present events or states of affairs which have been observed. Thus a deductively derived conclusion cannot go beyond this to make claims about future or unobserved happenings. But it is precisely these sorts of claims that characterize (IP) and inductive reasoning; so it looks like there is no way (IP) or any inductive system S can be justified deductively.

On the other hand, as the second half of the passage quoted above contends, induction cannot be justified by means of an inductive argument either. Any inductive argument will have to assume in advance that (IP) or S is reliable in order to prove that (IP) or induction is justified, and this amounts to circular reasoning. One might argue that (IP) or S is justified because it has a good track record, and since it has worked in the past it will continue to work in the future. But this argument is an inductive argument which itself either incorporates some form of (IP), or has been assigned a high inductive probability by certain rules of S, the system whose justification is the very point at issue. It might be said that the argument does not appeal to (IP) but rather to the principle of the uniformity of nature (UN); however, even assuming that an adequate version of this principle could be formulated, the only way (UN) itself could be justified is by prior appeal to (IP) or S.

So the Humean challenge can be viewed as a kind of dilemma which runs roughly as follows:

P1: If inductive reasoning is to be rationally justified, such justification must take the form of either a valid deductive argument or else a strong inductive argument.

P2: It is impossible to justify induction by means of a valid deductive argument.

P3: It is impossible to justify induction by means of a valid inductive argument.

C: Therefore, inductive reasoning is not rationally justified.

What are we to make of the skeptical argument? In a lecture delivered in1926, C. D. Broad described the failure of philosophers to solve the problem of induction as "the scandal of Philosophy." In more recent times, there have been various attempts to block the argument which have focused on denying one or more of the above premises. In general, there have been four very different sorts of strategies proposed as solutions to the problem of how to justify inductive reasoning: the inductive justification, the pragmatic justification, the ordinary language justification, and the a-priori justification. We will now take a closer look at each of these in turn.

II. The Inductive Justification

a) Skyrms' proposal

An inductive strategy attacks P3 head on and purports to show that an inductive justification for inductive reasoning can be given which is non-question begging. The claim is that such a justification only appears to beg the question, because it is easy to overlook a distinction between different levels of inductive argument. At the first level inductive arguments are applied to things and events in the world, at the second level inductive argumentation is applied to arguments on the first level, at the third level induction is applied to arguments on the second level, and so on. Each of these levels constitutes a "distinct, logically autonomous mode of argument" employing its own distinct inductive principles, so that the arguments on each level can be justified in a non-circular way by appealing to arguments at the next higher level. What this amounts to is that whereas level 1 will consist of inductive arguments about phenomena in the world, every level k greater than 1 will consist of inductive arguments about other inductive arguments on level k-1 (that is, arguments justifying the "success" of the rules used on level k-1), plus rules for assigning inductive probabilities to arguments on its own level, k. So a system S of inductive logic is rationally justified if for every level k of rules of S, there is an argument on level k+1 which i) is adjudged inductively strong by the rules of level k+1, and ii) has as its conclusion the statement that S's rules at level k will continue to assign high inductive probabilities to arguments whose conclusions turn out to be true. In justifying inferences made at any level, appeal is made to inductive rules at the next higher level which are not numerically the same as the rules used on the original level. For instance, in justifying the rules of level 1, the proponent of the present argument does not presuppose that these particular rules R1 will continue to work, but rather she advances an argument on level 2, together with its corresponding rules R2, to show that rules R1 will continue to work. Thus none of the arguments employed in the inductive justification of induction presuppose what they are trying to prove, and so this method of justification does not technically beg the question.

b) *Problems with the inductive justification*

Does the inductive strategy successfully rebut the charge of circularity? It seems clear that there is no circular argument in the technical or formal sense. Let's say that, roughly, an argument is circular when R, S, T, etc. are statements which (allegedly) jointly support some conclusion C (one or more of R, S, T may be explicit premises, or they may be suppressed premises or assumptions which are necessary for the argument to go through), and at least one member of R, S, T... just is C (expresses the same proposition as C). Given the way in which the above strategy is deployed, the inductive rules or principles used on the various levels of argumentation are numerically distinct. An argument at some level k would beg the question (technically) only if it employed the exact same rule or principle it was trying to justify. Yet notice that in the present strategy, the rules are distinct only in a trivial way. For any level k greater than 1, the inductive arguments on that level will all have the following form:

Level k argument:

Arguments on level k-1, which according to rules Rk-1 are inductively strong, have yielded true conclusions from true premises most of the time.

Therefore, arguments on level k-1 will yield true conclusions next time.

The sole difference between arguments on any two levels is their reference to arguments of the exact same type on the level right below them. Apart from the purely trivial difference that the arguments are assigned a certain "level", the arguments are exactly alike. They differ not one iota in form or content. And since the "rules" are strictly about the arguments themselves, they too differ only trivially. Any two levels of rules Rk and Rk-1 differ only in that they are assigned a unique number corresponding to the "level of argument" on which they are employed; there is no intrinsic or qualitative difference between the arguments or rules themselves at the various levels. In fact, the "rules" at each level are merely instances of a more general rule, e.g. 'for any inductive rule R which assigns inductive probabilities to arguments on some level k, if R worked well in the past, then R will work well next time.' All of the "level-specific" rules are instances of this general rule and presuppose it; and the only way to justify this general rule is by appealing to the very rule itself. If the general rule stated above is not epistemically justified, then none of its specific instances are justified either. Perhaps I follow a "rule" about bicycle riding which tells me 'When you're on Maple street and want to veer left, gradually turn your handle bars to the left and lean left,' and another "rule" which says 'When you're on Elm street and want to veer left, gradually turn your handle bars to the left and lean left.' But in the end aren't they either the same rule or else instances of a more general rule, such as 'When you're on a level, well-paved road and want to veer in a certain direction, gradually turn your handle bars in that direction and lean in that direction'? And if the more general bicycle rule is not "justified", then how can the two specific instances be "justified"? So it still appears that the inductive justification of induction is circular in a way that undermines its cogency.

Maybe the foregoing discussion is really much ado about nothing; for there is another reply which many take to be a decisive refutation of the inductive strategy. Recall how the inductivist posits a distinction between various "levels" of argument and and the unique rules of inference which operate at each particular level. A system of inductive logic is justified if there is an argument on each level which is adjudged inductively strong by rule(s) on the same level, and has as its conclusion the statement that the rule(s) which are employed on the level directly below it will continue to assign high inductive probabilities to arguments whose conclusions turn out to be true. But couldn't a completely different (and even incompatible) system of inductive logic utilize this same procedure in justifying its system? There might be a system which presupposes the denial of (UN); call this a system of counterinductive logic. Such a system will assign high inductive probabilities to level 1 arguments which instantiate the following argument form: Many A's have been observed and they have all been B; therefore, the next A will not be B. Then an inductive justification could be given for the rules of level 1 by offering this level 2 argument: level 1 rules of counterinductive logic have not worked well in the past; therefore, level 1 rules will work well next time. Based on the counterinductivist's own level 2 rules, this level 2 argument is inductively strong and can in turn be justified by a similar argument on level 3, and so on. In general, for each level of argument k, there will be counterinductive rules on level k which assign high inductive probabilities to arguments of the following type: Rules of level k-1 of counter- inductive logic have not worked well in the past; therefore, level k-1 rules will work next time. Thus, an inductive justification of a counterinductive system S' of rules and arguments can be carried out in parallel fashion, and will meet the same criteria laid out for the system S of (scientific) inductive logic. Yet the fact that S and S' are inconsistent with one another shows that this method of justification is sorely inadequate. The inductive justification of induction is an example of a self-validating procedure, and while such procedures may not always be suspect, the case of induction shows that it can validate something illigitimate. Thus the inductive justification of induction fails.

One final objection to the inductive approach is that appealing to various levels of rules and arguments leads to an infinite regress of inferences, so that there is ultimately no justification for induction. The ready reply to this is that if every level of rules is justified, then the whole system is justified, and it makes no sense to demand justification for the system over and above

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each of its parts. I find this sort of answer, in spite of its "Russellian" ring, to be quite implausible. One reason might be that justification is an epistemological notion, involving one's actual beliefs and noetic structure. Justification is always for someone. Now no one, except God perhaps, is capable of holding an infinite chain of beliefs. Returning to the inductive justification of induction, since a person can only hold a finite number of inferences, his beliefs can only stretch back to some level, say the nth level of argument for induction. So the nth level and thus every level below it will fail to be justified, in virtue of the fact that he doesn't hold the n+1th level of argument, which would be required to justify his believing n. But at second glance this reasoning seems somewhat dubious. For although one could not actually, psychologically form or hold a chain of beliefs proceeding to infinity, could not one simply claim that there is such a series of inferences, and that since he understands how the reasoning at each step goes, he is thus able to grasp the chain of argument itself as a whole, and is thereby justified in believing its conclusion? I don't see why not.

Maybe the following line of argument is a bit more tenable. In the inductive justification of induction, epistemic justification is transferred via the inferences from beliefs about arguments on one level to beliefs about arguments on the next level below it. The transfer of justification is a transitive relation. But justification itself is not generated or increased by virtue of this linear transfer of warrant or justification. Each belief must already possess a certain amount of warrant or justification in order to transfer that justification. But what then is the ultimate source of this warrant? It must be a basic belief or set of beliefs which are already epistemically justified and which form the starting point of the inferential chain. It follows from this that an infinite (linear) chain of beliefs and inferences can never generate the warrant or justification that is allegedly transmitted along that chain. Now it is not difficult to see that the inductive justification approach accounts only for warrant transfer, in as much as it posits an infinite linear chain of inferences in order to justify induction at each level, but has no way to account for the initial generation of the warrant that is transfered between beliefs within the chain. Thus none of the inferences in the overall argument are epistemically justified, and so the inductive strategy fails.

Another way of highlighting the same basic point is to see that an infinite epistemic chain could be imagined which provides justification for any proposition or belief B whatsoever, no matter how absurdly false the belief might be. Let B be the belief that Chris can run 50 miles per hour. It would be easy enough to imagine a linear series of beliefs to support B. I could do this, for instance, by claiming to hold the following belief C: If Chris can run 51 miles per hour, then he can run 50 miles per hour. Then I could affirm the antecedent of C and go on to back that belief up with yet another belief D: If Chris can run 52 miles per hour, then he can run 51 miles per hour. And so on, the argument would go. The point is that if my series of beliefs could be infinite, there would be no way to "catch" me with a claim that I couldn't back up. So I would be justified in believing the original B, namely that Chris can run 50 miles per hour. Thus it seems prima facie unreasonable to count as rationally justified any argument or inferential process that contains an infinite, linear series of beliefs or inferences which provide the sole justification for that argument.

III. The Pragmatic And Ordinary Language Justifications Of Induction

a) *Skyrms and Bonjour's defense of the pragmatic argument*

Another way to disable the Humean argument is to deny the second premise. This is the tact taken by proponents of the pragmatic justification of induction. Both Bonjour and Skyrms focus their discussion on a version of the solution originally developed by Hans Reichenbach. Many of our beliefs, decisions, and predictions can be likened to a bet made in a gambling situation. We do not know that our inductive inferences lead to conclusions that are likely to be true. (IP) presupposes that the proportion of A's that are B's will converge in the long run on some mathmatical limit m/n as the number of observed instances of A approaches infinity. The problem is that no one knows whether such a limit really exists, or whether the proportion will simply vary at random and not approach m/n (because we don't know that nature is uniform). But what we can know, according to the proponent of PJI, is that if there is such a limit, then the inductive method will discover it. In other words, we can give a kind of "conditional" justification of a (scientific) system of induction S by showing that if any method of induction will be successful, then S will also be successful. Suppose that some inductive method X were successful in a chaotic universe. Then the universe would exhibit uniformity in this one way (i.e. the uniformity of X's success), so that sooner or later S would discover X's reliability and "license" X as a method of induction. Thus if any inductive method will be successful, then S will. So S is rationally justified because it seems rational to bet on the method that will work if any method will.

b) *Problems with the pragmatic justification*

What should we say about PJI? First, as its proponents are willing to grant, it only shows that scientific induction is at best conditionally justified. But what is the justification for, or the likelihood that the antecedent of the conditional conclusion is true? If there is no reason for first believing that some inductive method will succeed, then there can be no justification for thinking that S will. Thus even if the above argument for PJI is sound, it does nothing to answer the original

Humean worry about induction; P2 of the skeptical challenge emerges unscathed. As Bonjour argues, the conclusion of PJI is fully compatible with the "deepest degree of skepticism" concerning matters of reasoning and scientific inquiry. This strategy yields absolutely no reason at all for thinking that inductive conclusions are to any degree likely to be true; thus it does not even begin to address the basic skeptical worry about induction.

Second, it appears that the argument advanced by the proponents of PJI fails to establish its conclusion after all. Suppose that method X assigns high inductive probabilities to level 1 arguments whose conclusions are usually true when the premises are true. Then in the longer run, as its premise comes to be verified as true, S will produce the following argument on level 2: 'Level 1 rules of X have been reliable in the past; therefore, level 1 rules of X will be reliable in the future.' So what the argument for PJI shows is that if X has rules that work well on level 1, then S can provide justification for those rules on level 2. But this falls drastically short of the conclusion of the present argument, which is that if X works well on level 1, then S will also work on level 1. More generally, what the supporter of PJI needs to demonstrate is 'For every level k, if any method of induction will be successful at level k, then scientific induction will be successful at level k.' But what the pragmatist has succeeded in showing is only the weaker claim that 'For every level k, if any method of induction is successful at level k, then scientific induction will license an argument at level k+1 which justifies the method used on level k.' And the former is clearly not entailed by the latter. It is still possible that scientific induction work on one level and yet fail to work on the level below it. Thus the argument offered for the pragmatic justification of induction (PJI) fails to demonstrate its conclusion.

c) The "ordinary language" defense of Induction

Another type of strategy attempts to refute P1 of Hume's argument by "dissolving" the problem of induction, claiming that no argument is needed to justify inductive reasoning. According to this view, the traditional problem of induction is a "pseudo-problem" that goes away once it is realized that it makes no sense to demand a justification for induction. One reason sometimes given is that such a demand tacitly requires the defender of induction to provide some logical guarantee that inductively strong arguments will give true conclusions from true premises all the time. However, demanding this type of proof or certainty is outrageous and unreasonable, because inductive logic by its very nature falls short of deductive validity. Inductive arguments are measured in terms of inductive strength or probability, a type of standard which is legitimate in its own right and capable of conferring positive epistemic status on arguments which conform to it to a high enough degree. Once this is seen, the demand for a justification of induction is ridiculous.

Now this type of dissolution of the problem of induction exhibits a considerable amount of confusion and blatantly misrepresents the Humean challenge. In order for some account to qualify as a rational justification of the inductive method, the skeptic is in no way demanding that arguments which are judged to be inductively strong by some inductive system should always produce true conclusions. Rather, the skeptic only claims that what is needed for justification is that arguments with high inductive probability produce true conclusions from true premises most of the time. What he wants is a sound reason for thinking that inductively strong arguments will not often lead to false conclusions. And this does seem like a reasonable request on the part of the skeptic, and one which accepts at face value the legitimacy of autonomous standards for evaluating arguments that do not satisfy the conditions for deductive certainty.

Another type of linguistic approach argues that it is senseless to ask for a justification of induction, either because part of the meaning of 'being rational' just is accepting inductive reasoning, or because inductive reasoning is an essential part of the machinery for rational discussion. Suppose that a person were to base his inferences and decisions on counterinductive logic, or on visions of the future that come upon him while asleep. We would certainly judge that person to be irrational, and our assessment of him would be at least partially based on the fact that he does not form his expectations and decisions in accordance with the inductive method. These examples show that inductive reasoning is a standard of rationality, part of what we mean by being rational. To ask the question "Why is it rational to accept inductive reasoning?" is a lot like asking why someone's father is male; anyone who really understands what is involved would never pose the question.

Bonjour examines a version of this type of argument originally put forth by Strawson:

1) Believing in accordance with strong evidence is believing reasonably.
2) Believing in accordance with inductive standards is believing in accordance with strong evidence.
3) Therefore, believing in accordance with inductive standards is believing reasonably.

Strawson claims that the two premises are analytic in virtue of the ordinary usage of the expressions in question. As Bonjour points out, however, the conclusion can't be analytic if it is to have any force. If the conclusion is not analytic, then the phrase 'believing reasonably' might have the epistemically strong sense of 'good reason to think the belief likely to be true' (lets call this epistemically strong sense 'being S-rational'); but taking it that way would beg the question. On the other hand, if the conclusion is analytic, then 'believing reasonably' cannot be construed in the strong sense above, and therefore does not offer a real reply to the skeptic. So either one of the premises is not an analytic truth, or else the argument is guilty of equivocation. Thus there is a serious flaw in the argument. But just where exactly has the argument gone wrong?

One possibility, suggested by Bonjour, is that the argument equivocates on the phrase "believing in accordance with strong evidence". For (1) to be analytic (given Bonjour's epistemically "strong" ("S-rational") sense of "believing reasonably"), the phrase must be construed as "believing in accordance with evidence that actually establishes a strong likelihood that the belief is true" (lets call this "strong" sense 'believing in accordance with S-evidence'); whereas for (2) to be analytic the phrase must mean "believing when the evidence is strong according to generally accepted standards" (lets call this the "weak" sense). Taken this way, (3) does not validly follow from (1) and (2). And if the second phrase in (2) is given the same sense as suggested for (1), the argument is made valid but ceases to be analytic and ends up begging the question; for the point at issue in the whole debate is whether or not following the inductive method provides evidence strong enough to establish the likelihood of the conclusion given that the premises are true.

d) Assessment of the "ordinary language" approach

Whether the skeptical question is "meaningless" depends in part on what is meant by the multifarious term "rational". Bonjour concedes that if the phrases "believing in accordance with strong evidence" and "believing reasonably" are taken in their "weak" senses throughout the argument, then the whole argument will go through. In this case, though, the argument loses it appeal, because this "weaker" conclusion is compatible with a thorough-going skepticism about induction. But why can't the argument go through if we retain the "strong" senses of the phrases given above throughout the argument? Remember, this is an argument about ordinary language. And our usage of the phrase "believing in accordance with inductive standards" means "believing in accordance with inductive standards which make a belief likely to be true" (or, we shall say, 'believing in accordance with S-inductive standards'). Who would take the phrase any other way? With this modification, and retaining the "strong" senses of each of the other phrases as construed above, we get the following argument:

(1*) Believing in accordance with S-evidence is believing S-rationally.

(2*) Believing in accordance with S- inductive standards is believing in accordance with S-evidence

(3*) Therefore, believing in accordance with S-inductive standards is believing S-rationally.

The revised argument is valid and all three statements are analytic. It might be objected that (2*) begs the question by defining inductive method in terms of yielding beliefs which are likely to be true. But that is how we use the term. Our association of the "strong" sense of 'evidence' with 'S-inductive standards' (and the reason why (2*) is analytic) is due to the fact that our notions of evidence and induction already imply the notion of truth conduciveness. We don't question or raise doubts in ordinary contexts about whether inductive standards yield true beliefs most of the time. The wording of (2*) accurately reflects the ordinary usage of 'inductive standards' and 'evidence'. Thus, following inductive standards or inductive reasoning is part of what we mean by rational belief (in the strong sense) after all, and so it is meaningless to ask for a rational justification of induction. So it looks like there is a "philosophically interesting" sense in which the linguistic argument is correct.

But someone might raise the query as to what justifies us, or how we know, that our use of the term 'inductive standards' corresponds to what actually is the case. Question 1: How do you know that inductive standards really are truth-conducive? Answer: because they have been in those cases which we can confirm by experience. Question 2: But how do you know that inductive standards will continue to be truth-conducive? Here we are right back to the original worry raised by Hume. We can simply leave out the term 'rational', and formulate the Humean challenge as a related question which can be meaningfully raised, and which highlights the central issue of the classical problem of induction: does inductive reasoning which assigns high probability to certain arguments actually yield true conclusions from true premises most of the time-- past, present and future? The linguistic argument does not provide an answer to this meaningful question; it stares us in the face regardless of how we proceed to define 'rational'.

A legitimate question can still be raised as to whether or not I am in fact obligated to demonstrate how I know that the inductive method is truth-conducive, that is, whether or not I must prove that it is in order for me to be S-rational. Perhaps I am epistemically obligated in some sense to provide an answer to Question 1 without being obligated in the same way to answer Question 2. Isn't it enough that it simply be true that strong inductive arguments will continue to yield true conclusions most of the time? Why do I need to produce any argument at all for this thesis if I am to be S-rational? What obligates me to do so? We shall return to this question at the end of the paper.

Skyrms seems to argue that in order for one to be "fully" rational, she needs to be able to offer some sort of answer to Question 2. It is not good enough for her to call herself 'rational' just because part of the definition of being rational simply is reasoning inductively. The Omegas have their own form of 'rationality' which they call brationality. He uses this example to show that on the linguistic solution you can define rationality in any way you want and thereby insulate yourself from criticism and rational discussion. And surely it is dubious at best to claim that your inductive policies are "rational" just because of the way they are built into your definition. But, Skyrms says, this is just what the ordinary language approach implies. If you let language define what it means to be rational, then you have no independent criterion by which to convince the Omegas that rationality is superior to brationality. But just how is this relevant to the problem of induction? Skyrms seems to be making two claims here. First, to be ideally rational or fully justified in accepting inductive logic, one should be able to convince others why they should accept induction. Second, one should have some independent criterion, which is not part of the definition of rationality itself, by which to do the convincing. Both of these claims can be plausibly denied. Skyrms' example does not show that I must be able to prove to anyone that rationality is superior to brationality in order for me to be S-rational. One's epistemic position with respect to some belief B appears to be independent of his ability to convince others to embrace B. Suppose that the Omegas are cannibals. Must I be able to articulate a convincing argument for the conclusion that killing and eating human flesh is wrong before I can be S-rational in believing that the practice in question is wrong? I think not. Moreover, I can be fully rational even if I have no neutral or independent criterion by means of which to assess the superiority of one system of "rationality" over the other. Perhaps there is no such criterion which is not already included in or implied by my own conception of rationality; so just how does that prevent me from being S-rational? Also, I do have a way from the inside by which to evaluate brationality-- I can say to the Omegas that induction generally has worked in those cases which can be confirmed from experience, whereas brationality has not (in other words, I have an answer to Question 1) . If I can show them that induction has been right more often than the predictions of their witch doctor, then surely that counts as something that sets rationality over brationality, even if I can't prove that being rational will work better in the future (or, even if I don't have a satisfactory answer to Question 2). Or perhaps we can confront the Omegas and appeal to their own natural propensities; they seem to believe in the future success of their witch doctors in spite of their bout of bad luck. Perhaps we can get the Omegas to see that they would be even more convinced of the success of their witch doctors if they were to consistently make successful predictions rather than be saddled with all that bad luck. Thus we could point out to them that they too have a natural inclination to follow some sort of rational inductive procedure. The upshot is that I don't need to be able to show that rationality will continue to work in the future in order to have good reason for thinking that rationality is superior to brationality, and I am certainly not obligated to convince anyone of this matter in order to be S-rational.

IV. Bonjour's A Priori Justification Of Induction

a) *Bonjour's a priori argument*

We have seen that the inductive and pragmatic strategies of justifying induction fail in accomplishing the task set before them, while the ordinary language argument provides a partial solution, but fails to address a related epistemically significant question, and one which is at the very core of Humean skepticism about induction. Can an a-priori approach to justifying induction fare any better?

In chapter 7 of In Defense of Pure Reason, Bonjour sets out to build a case for an a-priori solution to the problem of induction. He begins the section with some preliminary comments concerning certain misconceptions about the nature of an a-priori justification of induction. First, contrary to what many people think, an a-priori approach need not (and indeed should not) attempt to prove that conclusions of inductive arguments follow from their premises with deductive certainty. Second, such a solution need not involve the implausible claim that some such principle as (IP) or (UN) is itself an a-priori truth (for how can one rule out a-priori the possibility of a chaotic universe?). Third, Bonjour rejects the appeal to the notion of "containment" which says that since inductive conclusions are not "contained" in their premises, they cannot be justified by a-priori reasoning. Bonjour contends that the only intelligible sense in which the conclusion of an a-priori argument must be contained in the premises is that it must genuinely follow from them. Finally, Bonjour notes that the concept of analytic truth, defined as one whose denial is a contradiction, should not be construed so narrowly as to rule out the possibility that the denial of an inductive conclusion which follows probabilistically from its inductive premise(s) might turn out to be necessarily false.

Bonjour begins the next section by outlining the basic ingredients that are required for an a-priori solution to the problem of induction: an a-priori reason for thinking that the conclusion of a standard inductive argument is likely to be true if the premises are true, which consists of two claims, a) there is some explanation for why the proportion of observed A's that are B's converges on some relatively constant value m/n, and b) there is some sort of objective regularity which best accounts for the phenomenon described in (a). Bonjour then goes on to lay out and defend in some detail a three step argument which purports to be an a-priori justification of induction. His first premise is:

(I-1) In a situation in which a standard inductive premise obtains, it is highly likely that there is some explanation (other than mere coincidence or chance) for

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the convergence and constancy of the observed proportion.

Contrary to what many philosophers have assumed, Bonjour finds no compelling reason why such a meta-thesis, about the likelihood of a certain other thesis, cannot be an a-priori truth. Indeed, there might be possible worlds (including the actual world itself) in which a chance explanation could regularly be found for the truth of standard inductive premises. Yet as long as this situation is infrequent within the total class of possible worlds, it would still remain true in every world that it is likely that there is a non-chance explanation for the truth of a standard inductive premise. Hence (I-1) would still hold in every possible world and thereby be true necessarily.

Bonjour's second step in the argument involves articulating what sort of non-chance explanation for the observed proportion is most plausible:

(I-2) [Excluding the possible influence of observation] the most likely explanation for the truth of a standard inductive premise is the straight inductive explanation, namely that the observed proportion m/n accurately reflects a corresponding objective regularity in the world.

Bonjour does not take lightly the possibility that certain factors involving observation itself might affect the proportion that is actually observed, and so turn out not to accurately reflect the overall proportion of A's that are B's in the world. However, as he sees it, that is a different question; and the problem of induction simply does not address the issue. The classical problem of induction is about whether generalization from observed to unobserved cases is justified when such observational influences are absent; and to this problem Bonjour thinks he has a solution.

In defense of his second premise, Bonjour considers what other possible explanations, besides the straight inductive explanation, could account for the inductive evidence in question. He calls such an explanation a normal non-inductive explanation. In the simplest case, the relation between the presence of two objects or properties A and B is still a lawful regularity, but there is some further characteristic or factor C that combines with the A's and B's to produce a situation in which i) m/n of observed A's are B's, but ii) the presence or absence of C affects the proportion of A's that are B's, so iii) it is false that even approximately m/n of all A's are B's. For instance, it might be the case that there is a certain overall proportion of A's that are C's, which leads to a certain overall proportion of A's that are B's; but that the actual observations of A involve a higher (or lower) proportion of C cases as compared to non-C cases, thus resulting in an observed proportion of A's that are B's which is significantly different from the overall true proportion. Or the occurrence of C in relation to A might not be regular overall, with no objectively correct proportion of A's that are B's; nonetheless, observations of A might include a relatively uniform proportion of C's, resulting in a certain observed proportion of A's that are B's. In either case, the observed proportion will fail to reflect the actual overall proportion in such a way as to falsify the standard inductive conclusion. Now Bonjour contends that it is a-priori highly unlikely that either of these two situations be realized through sheer coincidence or chance. So a normal non-inductive explanation is extremely unlikely to be true. It follows, then, that the best explanation for the observed constant proportion of A's that are B's is the straight inductive explanation. Thus (I-2) is established, and the a-priori justification of induction is complete. From the above two theses, Bonjour concludes

(I-C) Therefore, it is likely that if a standard inductive premise is true, then the corresponding standard inductive conclusion is true also.

b) Why Bonjour's a priori defense fails

Bonjour proceeds to address several worries that might be raised about the argument. First, his argument is compatible with Reichenbach's insistence that from an a-priori standpoint, it is neither impossible nor unlikely that the world is chaotic rather than orderly. Where Reichenbach and others were mistaken was in thinking that this insistence is incompatible with there being an a-priori reason to affirm the likelihood of the truth of a standard inductive conclusion given that its empirical standard inductive premise is true. What Bonjour's argument allegedly shows is that the relevant sort of objective order or regularity asserted by an inductive inference is a-priori likely relative to the existence of empirical inductive evidence. A related worry is that Bonjour's argument only demonstrates that an objective regularity of the sort indicated by an inductive argument has existed in the observed past, with no guarantee that the same will be true of the unobserved future. Bonjour claims that an adequate metaphysical theory which explicates a robust conception of objective regularity or necessary connection would have the resources to handle this objection.

In regard to Bonjour's response to the first worry: if it is no more likely a-priori that the world is orderly rather than chaotic, then why should the existence of any inductive evidence make any difference? Why is the sort of objective order that would legitimize drawing a standard inductive inference more likely (a-priori) to obtain given the existence of some standard inductive evidence? To take a well-worn example, why should one's observing flocks of black crows make it more likely a-priori that all crows are black? Bonjour's answer, following (I-2), is that it is an a-priori truth that the most likely explanation for the truth of a standard inductive premise is the straight inductive explanation rather than some normal non-inductive explanation. It is highly improbable that a factor or condition C would by "sheer chance" cause the observed proportion of A's that are B's to differ in any uniform way from the actual overall proportion. But why think that the deviation in question must be attributed to mere chance or coincidence? Maybe the deviation caused by "factor C", along with the presence or absence of the factor itself, has some non-chance explanation which cannot be discovered by the inductive method. The variation could be due to some unknown but built in feature of our world which allows standard inductive explanations to be successful up to a certain limit but no further. In fact, there could be innumerable possible worlds that contain certain features which make it inappropriate to follow the sorts of inductive procedures we follow, that is, worlds in which reasoning by straight inductive explanations would be on the whole unsuccessful, although they would succeed up to a point. (There could be possible worlds in which following certain normal non-inductive practices are in the long run more successful). Now on the one hand, if there are such possible worlds (even if ours is not), then how do we know that there aren't many of them? And if there are many, then it is not a-priori likely that the best explanation for the truth of an inductive premise is the straight inductive explanation. On the other hand, if our world is a "straight inductive" world, then the only way to know this is by empirical investigation. Either way, Bonjour has not established the a-priority of (I-2).

Furthermore, it is hard to see how Bonjour's line of response can allay his second worry so easily. Let's see how things stand. Even granting the plausibility of (I-1) along with the claim that the sorts of normal non-inductive explanations Bonjour discusses are a-priori unlikely, the most that one can conclude (a-priori) given the occurrence of certain inductive evidence is that the observed proportion m/n reflects an objective regularity that existed in the observed past. Now let's define a spatio-temporal world segment (STWS) as a certain tightly defined spatial region and segment of the temporal order, whose outer boundaries are demarcated by either (i) the specific events and phenomena referred to by a given standard inductive premise, or (ii) the specific events and phenomena referred to by the corresponding standard inductive conclusion. We shall call an STWS which satisfies specification (i) a P-bounded STWS, and an STWS which satisfies (ii) a C-bounded STWS. In addition, let's say that inductive evidence obtains when certain observations are made and empirical data gathered which come to constitute standard inductive evidence. Now what follows from Bonjour's analysis is not (I-2), but rather this revised thesis:

(I-2*) [Excluding the possible influence of observation] the most likely explanation for the truth of a standard inductive premise is that the observed proportion m/n accurately reflects a corresponding objective regularity in the spatio temporal world segment in which the standard inductive evidence obtained.

Now (I-1) together with (I-2*) clearly do not entail (I-C). What is required to derive (I-C) is the addition of a third premise, such as

(I-3) It is a-priori likely that objective regularities which hold in a P-bounded spatio-temporal world segment will hold in its corresponding C-bounded spatio-temporal world segment.

What good reason do we have for thinking that (I-3) is true? We can't marshall support for this premise by pointing out that objective regularities which have held for P-bounded STWS's in the past have tended to hold for their corresponding C-bounded STWS's, for that would assume the truth of (I-C) and thus beg the question. Bonjour's suggestion is that if we can set forth some plausible metaphysical theory which gives an account of a robust conception of objective regularity in nature, the traditional problem of induction would be solved. It is important to note, however, that in order for the argument to go through, not just any plausible metaphysical theory will do, but one which a) is a-priori likely to be true, b) gives an account of objective regularities that is a-priori likely to be true, and c) entails that these objective regularities hold (for the most part) in the unobserved past, present, and future. In other words, Bonjour needs to make a further revision to his argument by adding the following premise:

(MT) There is some (a-priori likely) metaphysical thesis M which entails that objective regularities which hold in any P-bounded spatio-temporal world segment will probably hold in the corresponding C-bounded spatio-temporal world segment.

In other words, Bonjour needs a theory which both entails that nature is substantially uniform at all times and which is a-priori likely to be true. Where would we find such a metaphysical thesis that could do this incredible amount of foot-work? And how could we know a-priori that the truth of such a theory is even remotely probable? Bonjour contends that the difficulties involved here do not seem to be insurmountable. Now perhaps such faith in the philosophical enterprise is well-placed; regardless, faith is not nearly enough to show that (MT) is to any degree plausible. And without establishing the plausibility of (MT), Bonjour's argument cannot go through and his a-priori justification fails.

V. Concluding Reflections

So all of the standard answers to Hume's query are unsatisfactory. Where does the burden of proof lie in responding to Humean skepticism? Do I need an argument to show that induction is likely to continue to give me true beliefs in the future? The issue is not so much whether or not the question "is induction rational?" meaningless, but whether I am even obligated to give an argument for an affirmative answer to Question 2, i.e. whether I have an epistemic duty to show that induction will continue to be truth-conducive.

Perhaps it is the defenders of Hume who owe us an argument. We don't require the same kind of justificatory proof for basic laws of logic such as non-contradiction or excluded middle; the laws of logic cannot be given any non-circular justification. (But it must also be said that they are a-priori whereas inductive procedures are not). Nor do we require this kind of justification for other types of cognitive processes which we take to be reliable, such as perception or memory, none of which can be justified non-circularly. Why can't I be a reliabilist who holds, roughly, that a belief is justified if and only if it is formed in accordance with certain reliable belief forming processes, and just accept induction as one of those basically reliable processes? After all, the buck has to stop somewhere. And why must I be tagged as 'irrational' if the best I can do in defending some of those basic processes is to make use of the processes themselves and thereby reason circularly?

Perhaps my belief in (IP) can be what Plantinga calls a basic belief, grounded in the overwhelming propensity of all humans to accept it. Inductive reasoning doesn't seem to have arisen out of custom or habit as Hume claimed; for the reduction of the process of induction to habit is not consistent with what we know about the way in which habits become established. What typically occurs when a habit is being formed is that things which at first have to be done consciously and deliberately come gradually to be done effortlessly and almost automatically. In learning to ski, for example, we begin by conciously applying certain rules or principles. But when the operations in question have become a matter of habit, we are hardly aware of (or maybe not aware at all) of applying the rules. Nothing comparable to this seems to occur in the case of induction. I don't at first induct deliberately and with much effort, and then gradually come to do it with ease and little effort. The propensity to draw inductive inferences does not seem to be a habit established by repitition. I don't learn induction in the same way I learn skiing. I simply find myself applying inductive procedures instinctively, although I may at a later time reflect on them or study the processes and learn more about them.

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