

The Old Problem of Induction and the New Reflective Equilibrium

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ABSTRACT

In 1955, Goodman set out to ‘dissolve’ the problem of induction, that is, to argue that the old problem of induction is a mere pseudo-problem not worthy of serious philosophical attention. This dissolution, which has enjoyed tremendous acceptance, essentially involved an application of what has since been called the method of reflective equilibrium. Largely in connection with naturalism in epistemology, the reflective equilibrium method has lately been the subject of considerable attention (Goldman and Pust 1998, Graham and Horgan 1998, Jackson 1998, Kornblith 1998/2002). I will argue that, under naturalistic views of the reflective equilibrium method, it cannot provide a basis for a dissolution of the problem of induction. This is because naturalized reflective equilibrium is – in a way to be explained – itself an inductive method, and thus renders Goodman’s dissolution viciously circular. This paper, then, examines how the old problem of induction crept back in while nobody was looking.

Hume’s problem of induction is surely one of our clearest examples of a philosophical problem – if it *is* a problem. Its claims need little by way of motivation, its logic is very simple, and the rational conflict it brings up is unbearable. At the same time, however, it is a problem that many have found easy to dismiss as a ‘pseudo-problem’, a would-be problem whose tension is relieved when certain confusions are cleared away. In 1955, Goodman set out to ‘dissolve’ the problem of induction, that is, to argue that the old problem of induction is a mere pseudo-problem not worthy of serious philosophical attention (1955, 65–8). This dissolution, which has enjoyed tremendous acceptance, essentially involved an application of what has since been called the method of reflective equilibrium.¹ Largely in connection with naturalism in epistemology, the reflective equilibrium method has lately been the subject of considerable attention. I will argue that, under current views of the nature and status of the reflective equilibrium method, it cannot provide a basis for a dissolution of the problem of induction, because it has become itself an essentially inductive method. This paper, then, examines how the old problem of induction crept back in while nobody was looking.

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¹ Reflective equilibrium is of course most closely associated with Rawls (1971). However, Rawls, who gave the method its name, cites Goodman’s dissolution of the problem of induction as an earlier application of the method Rawls was working out (Rawls 1971, 20 n7).

1. *The old problem of induction*

The problem of induction was presented in its most urgent form by Hume, who also gave it its inevitable, skeptical solution.² He exhorts us to provide the ground for our inductive inferences, our 'reasoning concerning matters of fact'. The problem involves the logical tension between three individually compelling statements:

- (1) Inductive inference is not justified a priori.
- (2) Inductive inference is not justified a posteriori.
- (3) Inductive inference is (at least sometimes) justified.

Assuming that all justification is either a priori or a posteriori, (1), (2), and (3) form an inconsistent triad. Any two of them entails the denial of the other.

The tension between claims (1), (2), and (3) is problematic only if we have some inclination to accept them all. And we do. (3) is the most obvious of them. I cannot write this sentence without making countless inductive inferences. You cannot understand this sentence without making countless inductive inferences. We're strongly inclined to think that most of those inferences are justified. Yet reasons for (1) and (2) are hard to deny. Hume identifies the principle of the uniformity of nature as the principle underlying our inductive inferences. When I have observed sufficiently many green emeralds, and no non-green ones, I may infer that the next emerald I see will be green, or that all emeralds are green. But underlying this inference is the assumption that nature is uniform, that it is subject to immutable laws, that the future will resemble the past. So the question what grounds our inductive inferences reduces to the simpler question what grounds our acceptance of the uniformity principle. And here is where Hume digs in his heels.

The uniformity principle is not justified a priori. It is logically possible that regularities that have held up to now will suddenly cease to hold; laws of nature are not after all laws of logic. Nor is the uniformity principle justified a posteriori. We cannot reason from experience that regularities that have held up to now will continue to hold into the future. For such reasoning itself assumes that the future will resemble the past, and so begs the question. Thus, despite our unshakable impulse toward (3), there are good arguments for (1) and (2). For Hume, the solution was clear: The arguments for (1) and (2) are unassailable; he recognized that (1) and (2) together entail the denial of (3); so he was forced to abandon (3), and concluded that inductive inference is irrational.

² What follows is a brief sketch of the logic of Hume's problem as found in Hume 1975, Sect. IV, pt. II.

Needless to say, Hume's solution has not caught on. And so radical is his skepticism, so contrary to commonsense, it is easy to see why so many philosophers have suspected that something in the setup of the problem must have gone awry. Under the right analysis of induction and justification, it is thought, Hume's problem simply would not arise. Goodman sought to deliver such an analysis.

Hume's strategy was to identify the uniformity principle as the principle governing inductive inference, and then to argue that that principle can in no way be justified. To effect his dissolution, Goodman identified a different rule of induction, and then demonstrated how it can be justified. Goodman had in mind something akin to Hempel's hypothetico-deductivism, roughly, the model that states that empirical hypotheses are confirmed when their observable predictions hold forth and disconfirmed when their observable predictions fail.³ Hypothetico-deductivism gives us a formal rule for deciding evidence, as opposed to the uniformity principle, which makes the substantive assumption that the universe is lawlike. Hume showed us what happens when one tries to justify the uniformity principle. What is novel in Goodman's dissolution is his account of how the rules of induction get to be justified:

But how is the validity of rules to be determined? . . . Principles of deductive inference are justified by their conformity with accepted deductive practice. Their validity depends upon accordance with the particular deductive inferences we actually make and sanction. If a rule yields unacceptable inferences, we drop it as invalid . . . The point is that rules and particular inferences alike are justified by being brought into agreement with each other. *A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend.* The process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either. All this applies equally well to induction. An inductive inference, too, is justified by conformity to general rules, and a general rule by conformity to accepted inductive inferences. Predictions are justified if they conform to valid canons of induction; and the canons are valid if they accurately codify accepted inductive practice. A result of such analysis is that we can stop plaguing ourselves with certain spurious questions about induction (1955, 67–8, emphasis Goodman's).

Nowadays, we would put Goodman's point this way: Inductive rules are justified when they are in reflective equilibrium with our particular inductive intuitions. That is to say, general principles of inductive inference are justified when their consequences, their classifications of particular inferences as valid or invalid,

³ Goodman 1955, 69–70. Hempel's model is not completely adequate because, as Goodman goes on to explain, it gives rise to the 'new riddle of induction', the famous 'grue' paradox. Still, he seems to think that an appropriately shored-up version of hypothetico-deductivism can be achieved, once the grue paradox is got around, and it is that version that gives the lie to Hume's 'problem'.

accord with our intuitive assessments. Reaching this state of agreement may require adjustment at both ends. In this respect, Goodman acknowledges that the process of justifying rules is akin to the process of defining concepts:

The task of formulating rules that define the difference between valid and invalid inductive inferences is much like the task of defining any term with an established usage . . . [W]e may decide to deny the term 'valid induction' to some inductive inferences that are commonly considered valid, or apply the term to others not usually so considered. A definition may modify as well as extend ordinary usage (Goodman 1955, 68–9).

Thus, there is a further reduction in the offing here. As we've already seen, the problem of induction is the problem of justifying inductive inferences, and this was reduced by Hume to the problem of justifying an appropriate inductive rule. Now, Goodman reduces the problem of justifying inductive rules to that of defining our concept of valid inductive inference.

This all works out very well for induction, of course. Since according to Goodman induction and deduction are justified in the same way, induction presents no special problem, and 'we can stop plaguing ourselves with certain spurious questions about induction'. That inductive rules are justified the same way deductive rules are justified means only so much more prestige for inductive rules.

2. *The new reflective equilibrium*

Just as the problem of induction was being crossed off the list of problems we have to worry about, sweeping changes were taking place in philosophy regarding the nature and status of philosophical theory. Old-fashioned, a priori analytic philosophy fell on hard times. Quine 1953 argued that analytic philosophy is not possible. He argued that, from a plausible holistic view of semantics, there couldn't be a privileged set of statements or beliefs true by virtue of the meanings of their constituent concepts alone. Later, Kripke 1972 and Putnam 1975 argued that analytic philosophy is not necessary. They showed that some necessarily true statements are knowable only a posteriori, for instance, identities between natural kinds, like 'water is H₂O'. In all of this, the view that philosophy should be regarded as continuous with natural science was gaining momentum, and the momentum crested when Quine 1969 called for the naturalization of epistemology. He argued that the traditional a priori investigation of knowledge was futile, and that the theory of knowledge should be relegated to a branch of descriptive psychology. The net effect of these views has been a shift toward philosophical naturalism.

Goldman emerged as an early proponent of naturalism in epistemology with his reliabilist account of epistemic justification. For Goldman 1986, 63, the theory

of justification primarily involved theorizing at two levels, the level of the criterion and the level of the rule system. At the level of the criterion, a theory specifies the substantive, non-normative properties on which epistemic justification supervenes. At the level of the rule system, a theory specifies what justificational-rules, rules governing the formation and updating of belief, satisfy the earlier specified criterion. For instance, Goldman's own theory specifies reliability as the criterion on justification, and then identifies rules describing such processes as normal vision and memory as rules that satisfy the reliability criterion. What is interesting at this stage is what Goldman has to say about the nature of the theorizing that goes on at these two levels:

In trying to identify an acceptable criterion, what method should be used? The strategy I endorse is best expressed by the Goodman-Rawls conception of 'considered judgments in reflective equilibrium'. . . . A criterion is supported to the extent that implied judgments accord with [our] intuitions, and weakened to the extent that they do not. But our judgments are not final. They can be pruned and adjusted by reflection on candidate rule systems . . . This procedure need not involve empirical psychology or social science. I do not claim that psychology plays a role in selecting a criterion [on justification]. This is not the level at which psychology enters the epistemological enterprise. It enters the picture only if and when a criterion is selected that makes reference to cognitive processes. In other words, psychology is relevant to . . . the choice of particular [justificational]-rule systems, not . . . the choice of a criterion (Goldman 1986, 66).

Goldman seems to have reserved a place for a priori theorizing in naturalized epistemology; namely, he appears to think that the deliverances of reflective equilibrium in identifying a criterion on justification are uninfluenced by empirical findings. The only part of epistemology that gets naturalized is the selection of justificational-rules, that is, the identification of rules that satisfy a proposed criterion.

Whatever reasons Goldman had when he made these remarks for thinking that the deliverances of reflective equilibrium are non-empirical, he seems to have since changed his mind. Nowadays, he clearly takes our intuitions about cases to provide defeasible, empirical evidence for (or against) proposed conceptual analyses. This is because Goldman now takes concepts to be psychological structures that play a causal role in our linguistic behavior (Goldman and Pust 1998, 187–8). Thus, the manner in which evidence about linguistic dispositions supports (or undermines) a conceptual analysis is said to be just the way other observational evidence supports (or undermines) other empirical theories on the hypothetico-deductive model. According to Goldman and Pust, we derive an analysis of the concept, F, from a set of intuitions about cases of F-ness and non-F-ness through 'a straightforward kind of explanatory inference of the kind familiar from the sciences. Thus, using intuition as evidence would not carry any mysterious, nonscientific baggage . . . Its evidential claims are no more mysterious, from a

scientific or naturalistic point of view, than those of perception or memory' (190). Thus, in response to post-Quinean worries about the legitimacy of a priori conceptual analysis, Goldman and Pust have elaborated a way to legitimate reflective equilibrium-style philosophical theorizing: treat philosophical intuitions as empirical data, and treat reflective equilibrium as a case of hypothetico-deductivism. A corpus of intuitions illustrating ordinary usage of a concept is amassed; then a general hypothesis is introduced to classify cases according to ordinary usage. To the degree that it accounts for our intuitions, it is confirmed; and to the degree that it cannot account for them, it is disconfirmed. All of this goes with the usual rider about the defeasibility of intuition. Some intuitions may have been registered under unfavorable circumstances (Goldman and Pust, 189) or may have to be smoothed over for the sake of theoretical utility (190–1).

In his project to naturalize reflective equilibrium, Goldman could hardly be in better company. For starters, Jackson's (1998) defense of conceptual analysis also treats proposed conceptual analyses as empirical hypothesis to be tested against intuitions common to the folk. As he puts it, identifying criteria for some philosophically interesting concept 'is an exercise in hypothetico-deduction. We are seeking the hypothesis that best makes sense of [our intuitions] taking into account all the evidence' (36). Jackson then goes on:

I am sometimes asked – in a tone that suggests the question is a major objection – why, if conceptual analysis is concerned to elucidate what governs our classificatory practice, don't I advocate doing serious opinion polls on people's responses to various cases? My answer is that I do – when it is necessary . . . But it is also true that we often know that our own case is typical and so can generalize from it to others (Jackson 1998, 36–7).

Clearly, then, Jackson considers 'folk intuitions', i.e. the linguistic intuitions of a language community, to be empirical evidence: You can collect your data through opinion surveys or, if you are sure that your linguistic intuitions are representative of the folk, you can collect your data from the comfort of your own armchair.

Kornblith 1998/2002 [reprint], too, likens appeal to intuitions in carrying out philosophical analyses to appeal to observation in applying and testing empirical theories: '[O]n the account I favor, these judgments are no more a priori than the rock collector's judgment that if he were to find a rock meeting certain conditions, it would (or would not) count as a sample of a given kind. All such judgments, however obvious, are a posteriori, and we may view the appeal to intuition in philosophical cases in a similar manner' (Kornblith 1998, 134; 2002, 12).⁴ And finally, Graham and Horgan 1998, noting the demise of a priori conceptual

⁴ It should be noted here that Kornblith is not defending an account of reflective equilibrium for conceptual analysis; rather he is merely giving a naturalistic account of the method of appeal to intuitions. The crucial point is that, for Kornblith, an intuition about whether a term or concept applies to a particular case is at best a piece of defeasible, a posteriori evidence.

analysis, nevertheless defend reflective equilibrium-style philosophical analysis as a naturalistically sound methodology:

[P]hilosophy should regard armchair-obtainable data about ideological questions [i.e., appeal to intuitions] as empirical, and hence defeasible . . . [and it] should regard such data as having *strong* prima facie evidential status – similar to the evidential status, in linguistics, of introspective intuitions about grammaticality and syntactic ambiguity. We will call this the *principle of accommodation*, the idea being that the judgments that constitute such data should in general get accommodated as correct under an adequate ideological account, rather than turning out mistaken (Graham and Horgan 1998, 277).

Thus, bringing a philosophical theory into reflective equilibrium with our intuitions is of a piece with bringing an empirical theory into coherence with our observations, according to the likes of Goldman, Jackson, Kornblith, Graham and Horgan, and others.

I said in the beginning that the reflective equilibrium method, under current views, cannot provide a Goodman-style dissolution of the problem of induction. It is almost time to show why this is the case. But before I do, I want to attend to an apparent ‘way out’ of the conclusion I am about to draw. That is, if, as I will argue, current views of reflective equilibrium lead to the demise of dissolving the problem of induction, then it might be regarded as a cogent strategy to part company with Goldman and his ilk by maintaining that reflective equilibrium is not at all an empirical methodology akin to hypothetico-deductivism, but rather a purely a priori, conceptual methodology. If this strategy is cogent, then the dissolution of the problem of induction remains available to those who endorse the strategy, whatever the consequences for a sufficiently naturalized reflective equilibrium.⁵ In the remainder of this section, I want to offer *some* reason to think that Goldman and company are right that reflective equilibrium is an empirical methodology in virtue of the fact that evidence provided by our linguistic intuitions is straightforwardly empirical evidence, and therefore cannot guarantee a priori results.

Initially, this point is simply made. Consider Goodman again on reaching a point of agreement between deductive rules and our particular judgments about cases of deduction: ‘A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend’ (Goodman 1955, 68). Whether *we* are unwilling to accept an inference and whether *we* are unwilling to amend a rule are both empirical matters. That is, it is an *empirical* fact, if it is a fact at all, that we are unwilling to reject (or revise) *modus ponens* in light of purported counterexamples. There is a fact of the matter

⁵ Two anonymous referees for this journal pressed this objection. Though I doubt I can settle the matter here, I want to apply some further pressure to side with Goldman and company and take reflective equilibrium to be an empirical methodology.

about what are our inclinations or dispositions, and this matter is empirical. Empirical evidence cannot certify a priori results.

To this it may be responded that while it may be an empirical matter whether *anyone in particular* has certain linguistic dispositions – for example, a disposition to call a particular inference ‘valid’ – it is a non-empirical matter whether *a competent speaker of the language* has certain linguistic intuitions. On this view, while it may be an empirical matter whether anyone in particular is disposed to apply ‘bachelor’ to someone she knows to be married, it is not an empirical matter whether a competent speaker of the language is disposed to apply ‘bachelor’ to someone she knows to be married. On this view, we know – and crucially we know non-empirically – that a competent speaker of the language is not inclined to apply ‘bachelor’ to someone she knows to be married. Or perhaps more to the point, competent speakers themselves know, and know non-empirically, that ‘bachelor’ does not apply to someone who is married. Thus, if we return to the Goodman passage quoted above, and substitute ‘competent speaker’ for ‘we’, we get an importantly different view: ‘A rule is amended if it yields an inference [competent speakers] are unwilling to accept; an inference is rejected if it violates a rule [competent speakers] are unwilling to amend’. The reflective equilibrium test described in this version of the passage is non-empirical if it is a non-empirical matter what competent speakers’ linguistic dispositions are – at least, if it is a non-empirical matter from the competent speakers’ perspective.

However, I doubt seriously the view that competent speakers’ linguistic dispositions can be known non-empirically, even from the perspective of competent speakers themselves. This view raises the question what constitutes linguistic competence. I take it that competence is determined, at least in part, by the extent to which a speaker’s linguistic dispositions coincide with the linguistic dispositions of others in her linguistic community.⁶ However, it is an entirely empirical matter what the community’s shared linguistic dispositions are and to what extent the speaker shares them. In this way, I know that I am competent with ‘bachelor’, because I know that my linguistic dispositions connected with ‘bachelor’ are similar to the linguistic dispositions of others in my speech community. And although I take this as quite obvious, something I can ascertain upon a moment’s

⁶ An alternative, and more minimalist, account of linguistic competence might define competence in terms of success in referring. Thus, someone is competent with ‘bachelor’ if she can successfully refer to bachelors when using ‘bachelor’ in utterances. However, on the view of reference we get from Kripke 1972 and Putnam 1975, one needn’t have any particular intuitions at all connected with ‘bachelor’ and one can still successfully refer to bachelors. All that is necessary is that a speaker’s use of ‘bachelor’ be appropriately related to a certain reference-fixing event; this can happen without a speaker knowing much of anything about bachelors. Thus, this, admittedly minimal, view of linguistic competence also cannot underwrite the aprioristic view of reflective equilibrium.

reflection, it is nevertheless straightforwardly empirical and not at all a priori. (This is why Jackson can consistently recommend using opinion polls in this step of conceptual analysis.) This is, of course, only one partial view of what makes for linguistic competence; but it seems an essential part.

The conclusion of this section, then, is that on contemporary views reflective equilibrium is treated as just an instance of hypothetico-deductivism, and it is the hypothetico-deductive model itself that gives reflective equilibrium the legitimacy it has needed in the wake of Quine's attacks on a priori conceptual analysis. Moreover, this view of reflective equilibrium as an empirical methodology seems quite plausible, after considering the nature of the evidence brought to bear in applications of reflective equilibrium. However, there is a serious difficulty with treating reflective equilibrium as just an instance of hypothetico-deductivism. It is now time to consider the implications this view of reflective equilibrium has on the status of the problem of induction.

3. *The old problem reconstituted*

Goodman identified hypothetico-deductivism as the principle of induction and then claimed that it can be justified by being brought into reflective equilibrium with our inductive intuitions. Recent philosophers argue that the method of reflective equilibrium is itself an empirical method and that its legitimacy depends on its being an instantiation of hypothetico-deductivism. The implication of this analysis is now clear: Hume's circularity charge retroactively applies to Goodman's justification of induction. Hume argued that the uniformity principle cannot be justified a posteriori because all such reasoning relies on the soundness of the uniformity principle and so begs the question. A precisely parallel point holds against Goodman's dissolution: The hypothetico-deductive model cannot be justified by reflective equilibrium, as conceived by recent philosophers, because that method relies on the soundness of hypothetico-deductivism and so begs the question. If Hume's objection to experimental proofs of the uniformity principle is decisive, then the same reasoning shows that hypothetico-deductivism cannot be justified by reflective equilibrium. This is because, testing the hypothetico-deductive model against our common intuitions is itself an exercise in hypothetico-deduction, as Jackson might have put it. If this assessment is correct, then Goodman's proposed dissolution must be reversed, and the old problem of induction is reconstituted.

It might be thought that one could avoid this undesirable outcome by simply refusing to go along with Goldman and company in taking reflective equilibrium to be an empirical methodology. That is, if we maintain that the deliverances of reflective equilibrium are a priori, then there seems to be no obstacle to this dissolution. Unfortunately, as we have seen, this way out will not work. The basic

problem is that the evidence brought to bear on principles in the reflective equilibrium test is straightforwardly empirical.

I think this means that the problem of induction gets put back on the list of genuine philosophical problems we have to think about. This isn't to say that it cannot be given a non-skeptical solution. Though of course the well of possibilities is drying up. Goodman's dissolution of the problem of induction, or others essentially like it,⁷ have been terrifically persuasive. However, under current views of the nature and status of the reflective equilibrium method, these dissolutions themselves dissolve. And the current views seem correct, at least in their judgment that facts about our common intuitions are straightforwardly empirical facts. So, in conclusion, Hume's problem is not dissolved by reflective equilibrium; it remains completely intact.

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⁷ These other views, which differ from Goodman's in interesting ways, but which are nonetheless analytical dissolutions of the problem of induction, flourished in the 1940s and 1950s. Among them are included Ayer 1952 and Strawson 1952.