Nothing New About Grue

Jeremy Goodman
Brown University, Class of 2010

Hume is the father of the so-called problem of induction. He characterized induction, or as he called it "causal reasoning," as the inference from what is observed (either currently perceived or remembered) to what is unobserved. I will begin by recapitulating his discussion.[1]

Hume distinguished between two kinds of claims: claims about the relations of ideas and claims about matters of fact. These categories correspond to the *a priori*/*a posteriori* distinction. Claims about relations of ideas can be verified (or falsified) on the basis of reasoning alone (a procedure Hume calls "demonstration" or "intuition"), whereas the truth or falsity of claims about matters of fact cannot be demonstrated, since we can neither derive a contradiction from them nor from their negations. Hume also argues that any claim about matters of fact can be conceived to be the case, as can its negation. On the basis of both of these features—non-contradictoriness and conceivability—Hume concludes that no claim about matters of fact can be necessary or impossible.[2] In other words, he concludes that the a *priori*/a *posteriori* distinction tracks the necessary/contingent distinction. In so doing, Hume moves from the arena of epistemology to the arena of metaphysics.

According to Hume, all ideas must derive from impressions in the mind. In the case of our beliefs about the matters of fact that we perceive or remember perceiving, our beliefs derive from our present or past perceptual impressions. But many, indeed perhaps the vast majority of our beliefs concern matters of fact that we have not observed. Since these beliefs must ultimately derive from impressions, which are generally perceptions of matters of fact, Hume argues that any unobserved matters of fact of which we have an idea must be somehow related to observed matters of fact in such a way that our ideas of the latter can facilitate our idea of the former. He calls this relation that must hold "causation." This seems to accord well with our pre-theoretical usage, since, as Hume argues, we generally explain or justify our beliefs in matters of fact we have not observed by appealing to causal relations between them and matters of fact that we have observed—often to chains of such relations.[3]

Hume's next step is to analyze our concept of causation. He proceeds by attempting to discover necessary and sufficient criteria for two *events* to be causally related. He hits on "contiguity" and "priority" (that causes must be spatially adjacent and temporally prior to their effects) but argues that these conditions, though necessary for causation, are not sufficient for it. Hume calls the missing ingredient the "necessary connection" between causes and effects, which he proceeds to investigate. He considers two candidate principles: 1) that *necessarily* every event must have a cause and 2) that any cause must *necessitate* its effect.[4] He then argues that neither (1) nor (2) is true. The former fails because we can conceive of, and thus there might possibly be, matters of fact that come into existence spontaneously, i.e. without causes.[5] Likewise, in the case of the latter we can conceive of any cause occurring without its actual effect occurring.[6] Moreover, and critically for Hume, it appears that there is no impression associated with causes and their effects that could underlie the idea of a necessary connection between them. We are led to conclude that our concept of a necessary connection, and therefore of causation, is specious.

At this point Hume the rationalist despairs and Hume the naturalist takes over. Moving from metaphysics back to epistemology, he asks us to consider what is common to all of our causal *judgments*. He discovers that all cases where we

judge an event to be the cause of some effect are cases where we have previously observed a "constant conjunction" of many events resembling the cause contiguous and prior to events resembling the effect. He claims to have identified a psychologically necessary and sufficient condition for our assent causal *claims*. Furthermore, Hume argues that this is precisely the necessary and sufficient condition required for our assent to inductive claims as well, by which he means the following: For kinds of events A and B, we are willing to infer that there will be a B following the observation of an A just in case we previously observed a constant conjunction of As followed by Bs. This vindicates our pre-theoretical observation that causation and induction go hand in hand.

There is a problem with this picture. We make inductive inferences more broadly than we make causal judgments. Suppose every silver coin I have ever handled weighed 10 grams. Upon seeing a silver coin I inductively infer that the coin weighs 10 grams. Yet I do not want to say that the coin's color causes its weight; I certainly don't want to say that its color was prior or adjacent to its weight. The problem is that causation can occur only between events, but properties, in addition to events, can be the objects of inductive inferences, and constant conjunctions seem to play the same role in both cases. Therefore, we ought to allow this kind of constant conjunction to form the psychological basis of necessary connections, too. However, Hume can still distinguish causal from other necessary connections on the basis of contiguity and priority, so this is more of an expansion of his view than a modification of it.

How does Hume explain this psychological tendency? He claims that once we have observed a constant conjunction of As and Bs, an *idea* of an A will naturally lead us to form an *idea* of a B—*this is all it is for the two to be necessarily connected.* Moreover, *impressions* of As naturally lead us to form *beliefs* in Bs. Hume takes the first tendency to be simply a brute fact, to the effect that "Nature" has made our minds such as to engage in certain "customs" or "habits" of thought. The second tendency is explained by his theory of belief. Hume thinks that the difference between a belief and a mere idea is that the former has more "force" or "vivacity" than the latter. According to his theory of ideas, all ideas are faint (less vivacious) copies of impressions. Combining these claims, Hume argues that in cases where a constant conjunction of As and Bs causes an *impression* of an A to produce in us an *idea* of a B (via producing an *idea* of an A) the vivacity of the *impression* of the A is imparted to the resulting *idea* of the B, making it a *belief*.

Why does Hume adopt this strange account of belief? The answer is that he thinks it is introspectively apparent that one has the very same idea of a matter of fact whether one believes it to be the case or merely conceives that it might be the case. Hume anticipates the following objection: To believe that a matter of fact is the case is to have the idea of it *as existing*, whereas to merely conceive it being the case is to have an idea of it but not of it *as existing*. Hume does not think this is a sensible way to speak; to think about something being the case *just is* to think about it as existing. This seems right; that way of talking does seem like a confusion. From these considerations Hume concludes that the difference between belief and mere conception must lie in the *way* the relevant ideas are held in one's mind. Moreover, he thinks that there is a phenomenological difference between belief and mere conception, so he takes this difference to be definitive.

This concludes my very cursory summary of Hume's account of induction. It seems to me that there are broadly four ways in which the account's adequacy might be challenged:

1. Hume's account of the difference between belief and mere conception is wrong.
2. Hume's description of our inductive practice is wrong.
3. Hume does not explain *why* we have the inductive practice that we do.
4. Hume does not explain *why* our inductive practice is *correct*.

Each of these claims, I think, is true. I will spend the remainder of this paper assessing the degree to which they undermine a broadly Humean account of induction.

Let's first consider Objection 1. Hume's account of the difference between belief and mere conception surely can't be the whole story. After all, as Barry Stroud points out, we can have all sorts of attitudes towards propositions other than belief, including disbelief, hope, desire, supposition, dread and many others.[7] These clearly differ in kind rather than degree, so Hume will have to appeal to something other than degree of vivacity to distinguish them. In fact, it is not even clear what the difference between a belief and an impression is supposed to be according to Hume's view, since beliefs are vivid ideas and ideas are less vivid copies of impressions. At the same time, Hume should be applauded for disentangling the content of a proposition from the attitude we have towards it. Moreover, Hume did not think that his talk of vivacity was really satisfactory, merely hoping that it would be enough of a guide to allow the reader to discover the phenomenology of belief for his- or herself. He writes: "[I]n philosophy we can go no further than assert that belief is something f*elt* by the mind, which distinguishes the ideas of the judgment from the fictions of the imagination."[8] Therefore, we can interpret Hume charitably as claiming merely that propositional attitudes are individuated phenomenologically. This seems, p*rima facie,* like at least a defensible view. At any rate, it is not clear that Hume's theory of belief is particularly relevant to the problem of induction. Of course inductions lead to beliefs, but it seems that providing a theory of induction and providing a theory of belief are largely independent projects. This is another area where Hume gets things right. For him, these two e*xplananda*—our inductive thought (moving from i*deas* of the observed to i*deas* of the unobserved) and our inductive belief (moving from i*mpressions* of former to b*eliefs* in the latter)—receive more-or-less separate treatments. Therefore, I don't think that Objection 1 threatens Hume's account of induction.

Skipping Objection 2 for the time being, I would like to briefly consider Objection 3. Although Hume treats it as a brute fact that "Nature" has made us such that we engage in inductive thought, post-Darwin we are in a position to appeal to natural selection.[9] Clearly, the ability to make accurate, or at least mostly accurate, inductive inferences greatly increases an organism's biological viability by allowing it to anticipate events in its environment and engage in goal directed behavior. In fact, the ability to generalize past regularities is plausibly a n*ecessary* condition for the biological viability of even marginally complex organisms. Since natural selection serves to maximize a species' biological viability, it would produce organisms that engage in reliable inductive practices. Assuming our inductive practices are in fact generally reliable, natural selection provides an explanation of them. Hume would likely be quite sympathetic to these considerations, given his naturalistic orientation.[10]

It is one thing to ask what makes a practice reliable and another to ask what makes it justified.[11] This is the essence of Objection 4. Perhaps the most notable feature of Hume's theory is that he does not think that our inductive practice has any justification; that is to say, there is no sense in which an inductive inference can be said to be "correct." Hume argues as follows: Clearly any instance of induction cannot be justified by a *priori* reasoning since, as argued above, the falsity of the inference's conclusion is logically compatible with the truth of its premises. This is merely to note that no deductively valid inference can guarantee that there will be a B on the basis of there being an A and all prior observed As being followed by Bs. For such a move to be valid we must add the premise, as Hume puts it, "t*hat instances, of which we have no experience, must resemble those of which we have had experience, and that the course of nature continues always uniformly the same.*"[12] Stroud calls this the "uniformity principle," and Hume thinks it is needed to justify any inductive inference. So, the question becomes, how can we justify the uniformity principle? The principle is not a necessary truth, as it is conceivably false, so Hume argues that it cannot be demonstrated by reason alone. Therefore, since the uniformity principle goes

beyond what can be observed, we must arrive at it on the basis of induction. But all inductions presuppose the uniformity principle! Therefore, on the assumption that justification cannot run in a circle, Hume concludes that the uniformity principle, and thus our inductive practice, is not justified.[13]

Is this a fatal flaw of Hume's account? Does our inductive practice need some special justification? It is not clear that it does. Consider Nelson Goodman's interpretation of Hume. Goodman provides a version of the so-called "analytic justification" of induction.[14] According to Goodman, what it is to be a c*orrect* (or r*easonable)* inductive practice is to license all and only c*orrect* inductive inferences, and what it is to be a c*orrect* inductive inference is just to conform to c*orrect* inductive practices, all subject to the constraint that we find a*cceptable* everything the theory stipulates to be correct. Goodman acknowledges that the circularity is blatant, but argues that it is virtuous. For one, he notes, the circle, whether vicious or virtuous, is not vacuous: It is a non-trivial feature of a conceptual scheme that the rules of induction affirmed describe the inductive inferences assented to. Moreover, we place different normative constraints on generalizations (e.g. simplicity) than we do on individual inferences (e.g. accuracy), so bringing the two into accord substantively constrains our theorizing about induction. More importantly, however, Goodman claims that we are in precisely the same situation with respect to d*eductive* reasoning. Any justification of our practice of deductive reasoning would be an exercise in deduction, and thus circular.[15] It seems intuitive that we evaluate the correctness of the rules of deductive logic on the basis of their agreement with acceptable deductions, that we evaluate the correctness of deductions on the basis of their conforming to acceptable rules of deductive logic, and that we deem a deduction or logical rule acceptable only if we judge it to be correct. Goodman admits that the rules governing induction are nowhere near as simple or well established as those of deduction, but this does nothing to undermine the claim that justification works the same way in both cases. Goodman sees this theory as Humean; he writes: "And we owe belated apologies to Hume. For in dealing with the question of how normally accepted inductive judgments are made, he is in fact dealing with the question of inductive validity."[16] This is never explicit in Hume, and Goodman gives no textual support, but Hume does suggestively write that "Perhaps 'twill appear in the end, that the necessary connexion depends on the inference, instead of the inference's depending on the necessary connexion." At any rate, I'll take the view to be Humean, at least in a broad sense (one Goodman following another).

Put this way, I think the analytic justification is reasonably persuasive. To be clear, this account does *not* claim that deductions are valid, whereas inductions are merely reasonable. Reasonableness and validity are not comparable notions: The former is normative and the latter is semantic. Both deduction and induction are reasonable in exactly the same sense; deduction is valid and induction is not (since it is not necessarily truth preserving). If induction is in the same kind of boat as deduction with respect to justification, then the situation can't be all *that* bad.

However, those who insist that induction must be justified on the basis of inference rather than definition can make a superficially similar move by allowing for circular justification.[17] The idea is that since observed cases of induction have generated true conclusions most of the time, we can inductively generalize that all inductions, including this very one, yield true conclusions, at least with high probability. This way of putting things seems somewhat plausible. Yet the theory has at least two problems. The first is that it is intuitively wrong to think that inductions are justified on the basis of other inductions. The world being only contingently uniform, it is clear that any p*articular inductive inference* can only be justified a *posteriori.* But the justification of good inductive p*ractices,* practices that surely rely on a *posteriori* input, seems like it should be a *priori.* To see this, imagine a sorry possible being, forever bombarded with completely erratic and arbitrary sense impressions, with the exception of his gustatory sensations, which alternate between sweetness and saltiness every five

seconds. After a minute or two, and with a salty taste in his mouth, it seems reasonable for this being to infer that within the next five seconds he will experience a sweet taste. But although this being has an i*ntuitive* justification for his inductive inference, he has no basis for an i*nductive* justification of it. This is because, by construction, the gustatory induction is the only reliable one this being can make, which by itself is an insufficient induction base to inductively justify it. A second, related objection appeals to the necessary/probable distinction in a way analogous to the appeal to the *a priori/a posteriori* distinction just mentioned. Although past regularities do not necessitate future ones, they do justify those who observe them to infer the truth of, or at least assign a high probability to, the proposition that the regularity will persist. At least on an internalist conception of justification, it seems that the observation of regularities must necessitate the j*ustification* of these associated inferences, if not their r*eliability,* since the observation of the regularity is the reason for the belief in the proposition inferred. But if this justification can only be established by inductive means it must hold only contingently, since (or at least so thinks Hume) all comprehensible necessities can be demonstrated. For these reasons, I think inductive justifications of induction fail.

Both analytic and inductive justifications of induction, however, leave the same project unfinished: describing our inductive practice. Goodman argues that our inductive practice is much more complicated than Hume supposed, endorsing Objection 2. The project, as Goodman describes it, it to develop a logic of confirmation, analogous to our logic of deduction.[18] He considers, amongst other difficulties, Hempel's famous raven paradox and Hempel's solution to it, using them to illustrate how we should discover the rules that govern our inductive practice by examining the inductive inferences we find reasonable. The paradox is the following: I look around the room and see a number of objects, none of which are black and none of which are ravens. According to simple theories of confirmation, this helps confirm the hypothesis that all non-black objects are not ravens. But that hypothesis is logically equivalent to the claim that all ravens are black. Something has gone wrong: How could I have learned anything about ravens from just looking around my room? Hempel's solution is to require that we take the totality of our evidence into account when doing inductive reasoning. Since we already know that the world is full of non-black non-ravens, the observation of a few more of them does not change our relevant evidence and thus cannot confirm a generalization about the color of ravens. Had we not known that there are many non-black non-ravens, perhaps the observation of the objects in my room would have provided a *little* confirmation of the claim that all ravens are black. Whatever one thinks of this solution, Goodman takes it to be a model of how we refine our theory of induction to bring it into line with our intuitions about the reasonableness of certain inferences. As Goodman puts it, "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."[19]

It is interesting that not only is this not the kind of induction that Hume considers, but his theory is not vulnerable to Hempel's ravens. Hume is concerned with inferences from a constant conjunction of past As and Bs and a new A, to a new B. He does not try to draw the conclusion that *all* As will be (conjoined with) Bs. Note that the following inductive inference *is* reasonable: Every non-black thing in my room I have observed is not a raven, and I am observing a new non-black thing in my room, thus, this thing is not a raven.[20] The problem for Hume is that Hempelian inductive generalizations, rather than Humean predictions, are arguably the content of scientific theories, so if Hume is to accommodate our scientific practice he must allow us to infer generalizations from constant conjunctions.

At any rate, it is easy to show that Hume's account of our inductive practice is oversimplified. As Stroud notes, constant conjunctions are only *defeasible* conditions for inductive inferences. Suppose my family has steak every Friday for dinner. (If only!) On this particular Friday I read in the newspaper that there has been a mad cow disease scare, and that all beef has been recalled from the

supermarkets. In this case, the constant conjunction of Fridays and steak and knowing that today is Friday will not cause me to infer that we will be having steak for dinner. Moreover, it may not even be the case that my mind has the tendency to think of steak when I consider that it is Friday. Perhaps that consideration reminds me first of the newspaper, which reminds me of the mad cow scare, which grosses me out, suppressing any thoughts of food. Thus, it appears that Hume should add something like Hempel's "totality of evidence" condition, but couched in terms of the habits of the mind rather than conscious deliberation. Hume should be commended for trying to describe induction at the level of psychology, but psychology is much more complicated than he conceived.

Goodman sees a serious problem with Hempel's project. Consider the predicate "grue." Something is grue at time *t* just in case it is green and *t* is before 2010 AD or it is blue and *t* is after 2010 AD.[21] Every emerald we have ever seen was green, and on this basis it seems reasonable to conclude that every emerald we observe will be green. The problem is that every emerald we have ever seen has also been grue, but it seems highly unreasonable to infer that all emeralds we observe will be grue; in fact, after 2010 none will be! But the latter inference has the same logical form as the former, so we cannot distinguish reasonable from unreasonable inferences on the basis of their logical form alone, as Hempel had hoped. Therefore, we need a way to distinguish properties that are p*rojectable* from those that are not, where a property is projectable just in case regularities in its observed instances support reasonable inductive inferences to law-like generalizations of those regularities. One might claim that grue is not projectable for the purely syntactic reason that its definition makes explicit reference to time. But this reply fails, since blue and green can likewise be defined in terms of time using grue and its obverse bleen. The problem of providing a non-circular account of projectability is what Goodman called the "new riddle of induction," and it daunted him.

That's ironic, since Goodman's analytic solution to Hume's old problem of induction applies equally well to his new one. The solution, recall, was that the correctness criteria for inductive inferences are simply whatever reasonable-seeming criteria apply to all and only the inductive inferences we regard as reasonable. Goodman, following Hempel, took these criteria to be syntactic rules, and this is why he despaired at grue: It showed that no purely logical rules could decide between reasonable and unreasonable inductions. But the restriction to purely syntactic criteria was arbitrary: Grue shows that both predicate and *properties* matter when describing our inductive practice. Goodman was right to acknowledge the circle formed by confirmable law-like generalizations and projectable properties, but failed to realize that it is the same as the circle formed by reasonable rules of induction and reasonable inferences, which he considered so virtuous. Grue brings nothing *new*. And just as he was able to bracket off considerations of normativity to investigate the syntax of induction, we can bracket off considerations of the "naturalness" of properties and laws to investigate the semantics of induction. In fact, we can go in the other direction: We can take our inductive practice for granted and see what it tells us about natural kinds.

At this point one might be inclined to give the following objection: "Methodologically, I can understand bracketing off normativity, since it is probably going to ultimately come down to psychological facts about us. I can buy that at some level things bottom out and 'induction just is as induction does.' This doesn't bother me because we have a story to tell about natural selection and reliability. But the naturalness of properties is not something like the reasonableness of an inference: It's a real oomphy metaphysical fact about the world! Something must have gone wrong." This is where Quine comes in. He thinks we can tell the same story about natural properties that we told about reasonable inferences. There are certain features of the world that nature selected us to be able to track perceptually and to reason about. These properties, which we *recognize as natural*, are those that correspond to salient regularities in our environment. This admits a naturalistic

explanation: it is selectively advantageous to track projectable properties since it allows us to bring to bear in our planning and decision-making the lawful regularity in their instances. Just as in the case of our naturalistic account of our inductive reliability, facts about naturalness are ultimately facts about us —what we recognize as natural.

Incidentally, Hume's theory is resilient (though perhaps not immune) to the grue paradox for the same reasons that it is immune to the raven paradox: Hume's emphasis on prediction and his sensory theory of concepts. Also, his couching the uniformity principle in terms of "resemblance" might be evidence that Hume somewhat anticipated grue-type problems (although admittedly only very tenuous evidence)—resemblance, naturalness and projectability being highly interrelated notions, as Quine discusses. At any rate, note how well Hume's theory resembles Quine's. According to Hume, resemblance is a matter of conforming to a common idea, ideas are derived from impressions, and impressions are received more-or-less passively from Nature. On both of these theories, nature determines our conceptual scheme, our conceptual scheme determines what properties we deem to be natural, and there is nothing more to naturalness than this.

If the foregoing considerations are correct then a broadly Humean (*a la* Goodman and Quine) account of induction is in fact quite tenable. However, we think of induction as a much broader enterprise than Hume did. Hume thought that induction was straightforward causal reasoning: inferences about events we do not observe on the basis of events we do observe, by virtue of the causal connections between them. There is no difficulty in extending Hume's framework of necessary connections (reducible to the mental habits that result from the constant conjunction of ideas) to allow us to infer facts about the unobserved *properties* of objects on the basis of their observed properties or to allow for the inference of law-like *generalizations*. However, we also use induction to infer the existence of objects of a kind we can *never* observe (e.g. electrons). Since we can never observe these objects to be constantly conjoined with anything, we cannot infer their existence on the Humean model. Instrumentalism about such objects offers a way to retain the Humean paradigm, but most would consider that an unacceptable cost.

There are two problems here. One is that, since we can never have impressions of electrons, we cannot, on Hume's theory, acquire the idea of them. But even if we help ourselves to the concept of electrons, their unobservability dooms any Humean induction of their existence. It seems to me that this shows not that Humean induction fails, but rather that there are two kinds of induction: one for prediction and generalization and another for building explanatory theories. The latter is inference to the best explanation (or abduction), which is distinguished from Humean induction (among other ways) by not being associated with the uniformity principle. Humean induction and abduction have complimentary goals, the former aiming to characterize the unobserved on the basis of the observed and the later aiming to characterize the observed in terms of the unobserved; they have opposite directions of explanation. There is much to say about the link between abduction and Humean induction, but I will not address these issues here.[22]

NOTE ADDED IN PROOF

It now seems to me that I have not been entirely forthright about the issue of justification in this essay. As I see things, contra Hume, we *do* need to tell some story about what makes induction reasonable; in fact, I think we should see it, in some sense, as rational and justified. However—and this is what I saw as Humean (following Goodman)—I thought the relevant sense of justification could be the cheap one generated by Goodmanian reflective equilibrium. I now think I was unclear about what Goodman's "bracketing off" (as I called it) of justificatory issues amounts to. I see two possible interpretations:

1. Assuming we are rational (after all, Hume isn't asking us to be skeptics about *that*), reflective equilibrium will generate the rational inductive

practice. Of course, this doesn't tell us *why* the resulting practice is the rational one, but things are no different from the case of deduction.

2. Of course we have to tell a story about what makes induction rational. But this project has traditionally been misconstrued. We should not think that there are separate problems of justifying deduction, induction, abduction, etc. Rather, all we need to do is provide norms governing rules of inference (simplicity, perhaps) and norms governing individual inferences (accuracy, perhaps). Once this is done, all inferential practices can be treated equally.

I don't find (A) to be a satisfying strategy, because there *is* something we can say about deduction that we can't say about induction, namely, that it is valid! Validity is incontrovertibly sufficient for rationality, and Hume's challenge is to show that it is not also necessary. (A) does nothing to answer Hume's challenge. Contra Goodman and Quine, this seems to me like a challenge worth answering. After all, reflective equilibrium really is incredibly cheap: it's pretty obvious that one can't rationally accept general claims that conflict with the particular claims he or she accepts. How on earth could anything substantive follow from a principle like *that*?

I do find (B) to be a satisfying strategy, but it is a much more substantial one than I expect Goodman or Quine would be willing to sign on to. Furthermore, it doesn't seem particularly Humean. However, this does not threaten the two other main points in the essay: that to whatever extent (perhaps not at all) we can give a deflationary account of inductive rules and inferences, we can give a deflationary account of laws and projectable properties, and that not all forms of inference from the observed to the unobserved can be assimilated into the Humean model.

BIBLIOGRAPHY

- John, Foster. *Ayer*. London: Routledge, 1985.
- Gendler, Tamar and Hawthorne, John. (Eds.) *Conceivability and Poissibility*. Oxford: Oxford University Press, 2002.
- Goodman, Nelson. *Fact, Fiction, and Forecast*. Indianapolis: Bobbs-Merrill, 1965.
- Harman, Gilbert. 1965. "The Inference to the Best Explanation." *The Philosophical Review*, 74/1: 88–95.
- Hume, David. *A Treatise of Human Nature*. 1739. Available online (retrieved 1/15/09).
- Hume, David. *An Enquiry Concerning Human Understanding*. 1748. Available online (retrieved 1/15/09).
- Lewis, David. 1973. "Causation." *Journal of Philosophy*, 70: 556–67.
- Quine, Willard Van Orman. "Natural Kinds" in *Ontological Relativity and Other Essays*. New York: Columbia University Press, 1969.
- Stroud, Barry. *Hume*. London: Routledge, 1981.
- Swinburne, Richard. (Ed.) *The Justification of Induction*. Oxford: Oxford University Press, 1974.

NOTES

1. I will primarily follow the treatment in the *Treatise on Human Nature* (*THN*) 1.3.1-1.3.8 (especially 1.3.6). A similar line of argument is presented in the *Enquiry Concerning Human Understanding* IV-V.i. My interpretation is highly indebted to Barry Stroud's *Hume* (1981).
2. Hume runs non-contradictoriness and conceivability together. The move from these epistemic features to modal conclusions is contentious, in part because it appears that the only way it could be justified is by defining non-contradictoriness and conceivability in terms of the very notion of possibility they allegedly entail, rendering the link trivial. See Stroud pp. 49-50 for a discussion of these issues in Hume and Tamar Gendler and John Hawthorne's (Eds.)

Conceivability and Possibility (2002) for more on the contemporary debate.

3. Hume never presents this particular line of argument, although its components are present in *THN* 1.3.2 and in the discussion of matters of fact in the *Enquiry* IV.i. I have adopted this formulation because it provides a convenient way of setting up the problem of induction.
4. These may be formalized as:

   1. $\Box\forall x\,(Ex \supset \exists y\,(Cyx))$
   2. $\forall x\forall y\,(Cxy \supset \Box\,(Ex \supset Ey))$

   Where "C" and "E" are causation and existence predicates, respectively, and the quantifiers range over events. As Hume formulates these in *THN* 1.3.2 the modal operator is outside the scope of the quantifier in (1) and inside it in (2). Notice that a strict conditional rather than a subjunctive one appears in (2). It is notable that Hume apparently did not consider analyzing necessary connections in terms of counterfactual dependence, as has been popularized recently by David Lewis (1973), a philosopher with otherwise Humean leanings.
5. *THN* 1.3.3
6. *THN* 1.3.4
7. See Chapter 4 of Stroud (*op. cit.*).
8. *THN* 1.3.7; italics original
9. Here I am following Quine's "Natural Kinds" (1969).
10. See Stroud (*op. cit.*) for more on Hume's naturalism.
11. I will ignore reliabilist accounts of justification for the time being.
12. *THN* 1.3.6, emphasis original
13. An anonymous reviewer argued that it is anachronistic to see Hume as concerned with issues of justification. Although Hume ultimately concludes that induction is not justified, in his discussion of the uniformity principle he certainly seems to be taking the issue of justification seriously, although he uses the term "reasonableness." See Chapter 3 of Stroud (*op. Cit.*).
14. I am appropriating Richard Swinburne's term from the introduction to his anthology *The Justification of Induction* (1974). However, I think the term is somewhat of a misnomer when applied to Goodman. Unlike Peter Strawson, for example, Goodman is *not* trying to show that induction is rational, in some sense, *by definition*. Rather, he thinks that we can analyze our inductive practice in a way that allows us, in some sense, to bracket off issues of justification.
15. One might wonder whether there could be a purely *inductive* justification of deduction. I'm not sure such an idea really makes sense, but even if it did all it would show is that deduction and induction are not separable practices. The same considerations would show that any justification of the combined practice must be circular.
16. He should have said "reasonableness" instead of "validity."
17. See Max Black's "Self-Supporting Inductive Arguments" in Swinburne (*op. cit.*). Black is concerned to draw a principled distinction between vicious and benign circularities, and to show that inductive justifications of induction are benign. I don't think the distinction he draws is successful, yet I doubt philosophers these days would demand it, many having learned to live with conceptual holism, or at least circles of fundamental notions (i.e. possibility, consistency, rational entailment).
18. The confirmation relation, so conceived, is the converse of the rational entailment relation, subject to certain constraints. The goal of a logic of confirmation is to make these constraints explicit.
19. *op. cit.* pp. 64, emphasis original
20. One might worry that Hume faces a different problem: The mind is not in the habit of having non-raven ideas in response to non-black ones. But on Hume's theory of ideas, it is not clear that

we can have non-black or non-raven ideas in the first place, since that would require having impressions of something with merely non-black color or of merely non-raven species. One can see this as a deficiency in Hume's theory of ideas. I prefer to see it as an anticipation of Quine's claim that our perceptual system determines our perceptual kind concepts. More on this soon.

21. This is not Goodman's own definition, but rather a standard modification thereof.
22. For example, there are both weak and strong senses in which the rationality of Humean induction can be understood as parasitic on abduction. In the weak sense, it might be claimed, e.g., that the rationality of inductive generalizations derives from their ability to explain the observed regularities that constitute their induction base. See Gilbert Harman's "The Inference to the Best Explanation" (1965). In the strong sense, it might be claimed, e.g., that the rationality of an inductive generalization must derive from an abduction of an underlying process that provides a causal explanation of the regularity. See Chapter 3, Section 4 of John Foster's *Ayer* (1985). Personally, I am convinced that the rationality of induction is parasitic on abduction in the weak sense, but I have yet to be convinced that it is parasitic on abduction in the strong sense.