Hume, Norton, and Induction without Rules

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Does Hume's skeptical critique of inductive reasoning depend for its force on the assumption that such reasoning is a matter of following general rules? I explore this question in connection with John Norton's "Material Theory of Induction."

1. Introduction. With respect to inductive reasoning, there are at least two broad projects that have been of interest to philosophers. The first project is that of accurately describing paradigmatic instances of inductive reasoning in the sciences and in everyday life. Thus, we might ask of some particular historical episode how exactly Newton, or Darwin, or Einstein arrived at some conclusion on the basis of the evidence that was before him. The second project is one of justification. The task here is that of showing why paradigmatic inductive reasoning is good reasoning, or why skeptical challenges that purport to undermine the legitimacy of such reasoning fail to do so. As I understand it, this second, justificatory project prominently includes but is not limited to the task of either solving or dissolving Hume's skeptical critique of induction.[1]

Neither of these projects is trivial. Indeed, many think that we are currently far from successfully completing either one. But in any case, it would seem that the descriptive project has a certain priority over the justification project. After all, how could one justify our inductive prac-

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1. On my conception, the second project also includes the task of justifying less global aspects of our inductive practice, e.g., the well-known tendency of both scientists and ordinary people to give more weight to predicted data than to merely accommodated data in evaluating theories. For illuminating discussion of the two projects and their relationship, see Lipton (1991).

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tices, or defend them against skeptical challenge, in the absence of some reasonably detailed and accurate description of those practices?

Given this apparent priority, it is a striking fact that the justification project has often been pursued in the absence of any significant attention to the project of accurate description. In particular, a great deal of the traditional discussion of Hume's problem of induction has proceeded against the background of hopelessly crude models of our actual inductive practice—a fact sometimes acknowledged by participants in such discussions themselves. Plausibly, the thought that underlies this procedure is the following: whatever force Hume's critique ultimately possesses, that force is not tied to the specific content of the general rules or principles that we follow in reasoning inductively. Rather, the force of Hume's critique depends on our following any general rules or principles at all.

Consider in this connection the apparent contrast between induction and deduction. When one reasons in accordance with modus ponens, one reasons in accordance with a rule of inference whose reliability is not contingent on the character of the world that we inhabit. In contrast, the reliability of an inductive rule or principle seems to be a much more contingent matter, something that *does* depend on the specific character of the possible world that we happen to be in. For any inductive rule that we might choose to employ, there are some possible worlds in which that rule is reliable but other possible worlds (including some whose past history is identical to our own) in which the rule will be unreliable in its future applications. In some possible worlds—the induction-friendly worlds—reasoning inductively, in the way that we ordinarily do it (however exactly that is), works very well. But in other possible worlds—the induction-unfriendly worlds—it does not. (By contrast, there are no "deduction-unfriendly" worlds.) Because of this, our having justified confidence in the reliability of the inductive rules that we actually employ would seem to require our having justified confidence that we are in one of the induction-friendly worlds as opposed to one of the induction-unfriendly worlds. Showing that the rules in question are reliable would thus seem to require one to discharge the heroic task of vindicating some large-scale, sweeping assumption about the character of our world. I will not review here the long history of attempts to vindicate such an assumption, a history which would include, among other noteworthy episodes, Kant's appeal to a synthetic a priori "principle of universal causation," Mill's "axiom of the uniformity of nature," and Russell's "postulates of scientific inference." Suffice it to say that once this general picture is in place, Hume's dilemma—according to which neither an a priori nor an a posteriori justification of such an assumption can be had—strikes many of us as quite formidable.

However, this way of viewing things makes the following possibility

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salient: if the assumption that we reason in accordance with general rules is jettisoned, Hume's skeptical critique loses whatever force it might originally have enjoyed. That is, even if Hume's critique does not depend on our following general rules with some specific content, perhaps it does depend on our employing some general rules or other. According to this line of thought, if it turns out that inductive reasoning is not a matter of following general rules or principles, then there is no corresponding optimistic assumption about the world that stands in need of justification—and, thus, nothing to serve as grist for Hume's mill (cf. Okasha 2001). In this way, Hume's skeptical challenge is defused, having been revealed to rest on a mistaken presupposition about how inductive reasoning actually works. An intriguing possibility, perhaps. But is it so?

2. The Material Theory of Induction. Rather than pursuing this question in the abstract, I want to explore it in connection with John Norton's "material theory of induction" (2003, 2005). The denial that induction is a matter of reasoning in accordance with general rules is central to Norton's account. Moreover, he explicitly suggests that it is a virtue of the account that on it, but not on more traditional accounts of induction, Hume's critique fails to gain traction, for reasons very similar to those sketched above (Norton 2003, 666–69; cf. 2005, sec. 8). Thus, Norton's theory affords an excellent context for consideration of the issue at hand.

It is central to the material theory of induction that inductive reasoning is not governed by general rules (or "universal schemas" or "formal templates"). Indeed, this seems to be its defining feature. Rather, particular inductive inferences are underwritten by domain-specific facts or "material postulates." As Norton puts it, "all induction is local" (2003, 648).

Consider one of his paradigm examples (Norton 2003, 649–50). A scientist observes that a small number of samples of bismuth melt at 271 degrees Celsius; on that basis, he infers that all samples of bismuth melt at 271 degrees. The scientist's inference is justified despite the fact that he has observed only a small number of samples, the combined content of which exhausts only a vanishingly small fraction of the content of the inferred generalization. By contrast, the scientist is not justified in inferring that all samples of wax melt at such-and-such a temperature on the basis of observing a few samples that melt at that temperature. What is the difference? In the case of bismuth, the relevant inference is underwritten by the known empirical fact that "samples of elements are generally uniform in their physical properties" (Norton 2003, 650). In contrast, there is no analogous fact in the case of the wax.

Of course, few if any of us would deny that this is the correct thing to say about the bismuth wax example. Compare a similarly straightforward case from outside of science: I give more credence to things that I read

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in the *Wall Street Journal* than to things that I read in the *National Enquirer* because of my domain-specific background knowledge that the former is more reliable than the latter. If my background knowledge about newspapers and tabloids were different or less substantial than it is, I would distribute my credences differently, in the obvious ways. I take it that the difference between the proponent of the material theory and a more traditional philosopher of induction is something like this: the proponent of the material theory thinks that, once one understands how induction works in these and similar examples, one understands (at least, more or less) how induction works. By contrast, the traditional philosopher of induction will think that, precisely because such examples involve appeal to crucial bodies of domain-specific background knowledge that are obviously relevant to how one should proceed inductively, they fail to engage with (or at least, tend to obscure) the deep philosophical issues about induction. Thus, many philosophers will think that the deep questions about inductive inference are better raised by cases in which one lacks such domain-specific background information, yet, intuitively, some inductive conclusions are better supported than others by one's observations. It would be a mistake to simply dismiss such cases as unrealistic philosophers' fictions or as ones that never arise in actual science.[2]

No doubt, such cases are not paradigmatic of inductive reasoning in science. After all, skepticism aside, science has by now succeeded in amassing a great deal of domain-specific knowledge, and everyone agrees that such knowledge makes a difference wherever and whenever it is possessed. Nevertheless, many traditional philosophers of induction will hold that such cases are (so to speak) where the philosophical action is. Compare: such a philosopher might very well agree that there is a genuine norm to the effect that one should give higher credence to what one reads in the *Wall Street Journal* than to what one reads in the *National Enquirer* and that the existence of this norm depends on various known facts specific to the domain of newspapers/tabloids. But she will think that this norm is a highly derivative one, which is ultimately underwritten by much more general norms that do not depend for their status as such on domain-specific facts about newspapers and tabloids. While the traditional philosopher of induction will agree with Norton that much, even most, actual inductive reasoning is "local" in his sense, inasmuch as it proceeds in accordance with domain-specific norms underwritten by domain-specific

2. Lange provides a number of examples from the history of science, focusing in particular on cases from astronomy and macroecology. He concludes that "the history of science is filled with cases where scientists, despite operating in contexts that were remarkably theoretically barren . . . took uniformities among their observations and justly projected them onto unexamined cases" (2002, 229).

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facts, she will deny that induction is or could be local all the way down (as it were). As we might put it: although everyone thinks that some induction is local, not everyone thinks that all induction is local.

I find the material theory of induction an attractive account, in several respects. First, the associated picture of induction does seem agreeably close to much actual scientific practice. Wesley Salmon once remarked that the "the idea of a philosopher discussing inductive inference is apt to arouse grotesque images in many minds" (Salmon 1967, 17). The charge of gross distortion is not one that is likely to be made against the material theory, I think. Moreover, in its firm rejection of the idea that there are universal rules of induction that it would be reasonable to employ regardless of the prevailing facts, it immediately avoids an embarrassment that looms for various competing accounts. Above, I noted the familiar point that the reliability of an inductive rule, unlike the reliability of a deductive rule, seems to vary depending on the character of the world in which it is employed: even if a given inductive rule is reliable in our world, or in a world which is, in fact, as we believe our world to be, there are presumably other possible worlds in which the same rule is unreliable. Moreover, it is natural to suppose that not only are there at least some possible worlds in which the rule is unreliable but also that there are some possible worlds in which one acquires evidence that the rule in question is unreliable. But once one acquires evidence that the rule is unreliable when used in the world in which one is located, surely it would be unreasonable to continue to employ that rule. Although there are no doubt moves that could be made here, on the face of it this line of thought seems to tell against the idea that there could be any truly universal rules of induction, that is, rules that it would be reasonable to employ no matter what one's view of the "material facts," in the way that (plausibly) it would always be reasonable to reason in accordance with modus ponens.[3]

If inductive reasoning as it is understood on the material theory is less susceptible to Hume's critique than it is on more traditional accounts, then this would be an additional virtue of the material theory, and a significant one at that. On the other hand, if—as I will suggest—the material theory does not ultimately put us in a stronger position with respect to Hume's critique, this is not necessarily a good reason to reject the theory. While a satisfying answer to Hume would no doubt be a nice thing to have, it would be a mistake to treat the provision of such an answer as a condition of adequacy on an account of induction, just as it would be a mistake to require of a theory of perception that it provide

3. For a development of this line of thought, see Okasha (2001, 321).

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a satisfying answer to skepticism about our knowledge of the external world.[4]

3. The Material Theory and the Problem of Induction. According to Norton,

> When we transport the argumentation used to set up the problem of induction to the material theory, it no longer forces the same sort of difficulty. . . . In it, we induce a fact with an induction that is grounded by the facts of a material postulate; these latter facts are justified by inductions that are in turn grounded in the facts of other material postulates; and those facts are justified by inductions grounded in other facts; and so on. The regress here . . . merely describes the routine inductive explorations in science. Facts are inductively grounded in other facts; and those in yet other facts; and so on. As we trace back the justifications of justifications of inductions, we are simply engaged in the repeated exercise of displaying the reasons for why we believe this or that fact within our sciences. (2003, 668)

In what sense are inductive inferences "grounded in" material facts? I take it that it is not sufficient either that such grounding facts obtain or that they are believed to obtain. Rather, what is required is that the person drawing the inference *knows* (or at least, reasonably believes) that they obtain. After all, if the rationality of induction could be guaranteed either by (i) certain facts obtaining (regardless of whether we had any reason to believe that they obtain) or (ii) our simply believing that such-and-such facts obtain, then it is hard to see how the problem of induction could pose much of a problem for anyone, including proponents of traditional, "formal" accounts of induction. On the first possibility (i), the rationality of induction would be guaranteed simply by our living in what is in fact an induction-friendly world (again, regardless of whether we had any reason to think that it is induction friendly as opposed to induction unfriendly). On the second possibility (ii), it would be enough if we simply believe, optimistically, that our world is induction friendly—as we surely do. And indeed, Norton's view is that knowledge of the underlying ma-

4. Compare: many Bayesians readily concede that Bayesianism does not provide a satisfying answer to Hume. On a Bayesian account, the optimistic assumption that we inhabit an induction-friendly as opposed to an induction-unfriendly world is, in effect, built into our prior probability distribution; a satisfying answer to Hume would thus involve showing that those priors are rationally superior to the alternatives, a task for which Bayesians tend to have famously little appetite. Mindful of this, many Bayesians frankly admit that their view offers little against the Humean skeptic—but they are no less committed to their Bayesianism on that account.

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terial postulate is what is required: "In order to learn a fact by induction, the material theory says that we must already know a fact, the material postulate that licenses the induction" (2003, 666).

Let us call this commitment of the material theory

> **Prior Knowledge**: in order to learn a fact by induction, one must have prior knowledge of the material fact that licenses the induction.

Thus, in order for the scientist to come to know that "all samples of bismuth melt at 271 degrees" via induction, he must already know the material postulate that "samples of elements are generally uniform in their physical properties." Of course, this material postulate is itself a generalization. We can then ask, how did the scientist acquire his knowledge of it? Presumably, knowledge of the generalization depends on earlier inductive inferences (whether performed by the scientist himself or by others), inferences that were themselves grounded in yet prior knowledge.

My worry is that once this picture is in place, Humean skepticism about induction will return with a vengeance. For consider that time immediately before we acquired our first piece of inductive knowledge. Let E represent the totality of our knowledge at that moment. Perhaps E is extremely meager, consisting of a small number of propositions. But, if the principle Prior Knowledge is true, E is nonempty.[5] What might E include? In principle, E might include any proposition that we are in a position to know a priori. With respect to empirical knowledge, it will contain anything that we have learned on the basis of direct observation, perhaps including some empirical generalizations, namely, any generalization that we learned on the basis of performing a so-called perfect induction from propositions that we learned on the basis of observation. It will also include anything that we might have learned on the basis of deduction from things that we learned in some other way. In this context, we can view the inductive skeptic as someone who argues that however the various pieces of E are combined, arranged, or ordered with respect to one another, we will never be justified in moving beyond it by reasoning inductively. Of course, someone might think that the inductive skeptic is simply in the grips of some fundamental confusion from the get-go. For example, someone might think that the skeptic is guilty of falsely presupposing that "all good

5. One might very well worry about any appeal to a hypothetical time at which we possessed some genuine knowledge, none of which is inductive. Isn't this just philosophical make-believe, no more respectable than (say) a political philosopher's appeal to a time at which rational bargainers literally faced off against one another in some presocietal state of nature? Because I myself am generally sympathetic to this kind of concern, I want to emphasize that, given his commitment to the principle of Prior Knowledge, it seems that Norton is committed not only to the possibility of such an epistemic state but also to its actuality.

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reasoning is deductive reasoning" or some such thing. It is important, however, that this is not what Norton thinks. For he holds that the traditional problem of induction *is* a genuine problem for alternative accounts of induction. So there is at least that much to be said for it.

As we have seen, on the material theory the gap between particular pieces of observational knowledge and what we are justified in believing on the basis of induction from those observations is closed by some known material postulate. Suppose that we try to take a first, minimal step beyond E. Again, intuitively, this proposition will be our first piece of inductive knowledge. In that case, we must have recourse to at least one known material postulate. Of course, that material postulate has to be a part of E, since it has to be known, and E represents the totality of our knowledge at the time. Thus, the picture must be something like the following: the material postulate is a proper part of E, which is going to underwrite or license the inductive inference from some other proper part of E.

My worry is that, given that the only empirical knowledge that one has at that point is observational knowledge and its deductive consequences, there would not be anything suitable around to play the role of material postulate. Notably, all of the actual examples that Norton provides of material postulates are propositions with respect to which prior uses of induction must have played some role in our coming to know them.[6] I suspect that this is no accident. My suspicion grows out of reflection on the original role that material postulates are introduced to play within the material theory. After all, one might think that the fundamental question in this area is why it is reasonable to take some ampliative leaps beyond one's observational base as opposed to others. What makes the difference between those ampliative leaps that are reasonable and those that are not? As we have seen, Norton's answer to this question is that in those cases in which the leap is reasonable, there is some known material postulate that bridges the gap between observation and inferred conclusion. But if the material postulate itself is just more observational knowledge or deductive extensions of such knowledge, then one is not yet getting beyond the deliverances of observation. Unless it is claimed that the original material postulate is a piece of innate knowledge, or the

6. In addition to "samples of elements are uniform in their physical properties" (which he clearly understands as an empirical generalization), Norton provides the following examples: that the nature and properties of the elements are due to the quantum properties of electrons trapped by atomic nuclei (2003, 663), that gravitational forces act between celestial bodies (663), facts to the effect that certain methods and techniques are generally reliable (658), facts about which physical properties of stochastic systems are responsible for the objective chances of outcomes being what they are (660), and (in Norton 2005) specific facts about quantum processes that license inductions about radioactive decay.

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deliverance of some kind of rational, a priori insight (and it is quite clear that this is not the status that Norton takes material postulates to have), then there seems pressure to think of it as itself the deliverance of some kind of induction, broadly construed.

In order to arrive at our first piece of inductive knowledge then, it seems that we must already possess at least one prior piece of inductive knowledge. But obviously, that is impossible. On the material theory of induction, the ability to arrive at any inductive knowledge seems to presuppose that one already possesses some prior inductive knowledge that one can exploit. Thus, because of the role that it assigns to material postulates, a particularly strong version of the skeptical problem (inductive knowledge can never get off the ground, etc.) does seem to arise for the material theory.

Norton anticipates a version of the "regress" worry floated here. At least for the sake of argument, he seems prepared to talk in terms of a quite traditional, linear picture of justification:

> What remains an open question is exactly how the resulting chains (or, more likely, branching trees) will terminate and whether the terminations are troublesome. As long as that remains unclear, these considerations have failed to establish a serious problem in the material theory analogous to Hume's problem. And it does remain unclear. It is possible that serious problems could arise in termination . . . [but] it is also possible that the chains have benign termination. They may just terminate in brute facts of experience that do not need further justification, so that an infinite regress is avoided. Or, more modestly they may terminate in brute facts of experience augmented by prosaic facts whose acceptance lies outside the concerns of philosophy of science—for example, that our experiences are not fabricated by a malicious, deceiving demon.[7] (Norton 2003, 668)

What if the envisaged chains of justification do end in "brute facts of experience that do not need further justification"? Alternatively, could the assumption that our experiences are not fabricated by a malicious, deceiving demon play the role of the ur-material postulate? These suggestions do not speak to the current worry, I think. The kinds of moves gestured at here seem most relevant if what was at issue was our ability to arrive at genuine knowledge of the "external" world on the basis of our expe-

7. In this respect then, Norton's claim on behalf of the material theory is quite modest: he does not claim that the apparent regress of justification that his account generates is unproblematic, only that it is unclear whether it is. But I suspect that the unclarity here is largely a matter of our unclarity as to the details of the relevant kind of rational reconstruction; that is, unclarity about how exactly to cast our scientific knowledge into the envisaged kind of foundationalist structure.

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riences. However, as I understand him, the Humean skeptic about induction does not call into question whether our experience provides reliable information about the world. Indeed, he is perfectly prepared to grant that we possess ever so much observational knowledge. Rather, his challenge begins when we attempt to move (nondeductively) beyond that observational base.

In a note, Norton briefly considers and rejects what he takes to be a bad reason for thinking that skepticism about induction simply re-arises on the material theory. The bad reason is the mistaken view that "brute facts [of experience] are always singular and no collection of singular facts can license a universal" (2003, 668, n. 9). But against this "the problem is that canonical singular facts—'the ball is red'—already presuppose universal knowledge. In this case it resides in the recognition that the thing is a ball and its surface is red, thereby admitting recognition of commonality with potentially infinite classes of objects and colors" (Norton 2003, 668). Given that material postulates are propositions, I assume that here "universal knowledge" includes propositional knowledge that some universal generalization is true and not merely the kind of classificatory "know-how" that one has when one possesses a general capacity to group red things with other red things, distinguish them from nonred things, and so on. It is unclear from what Norton says here which universal generalization or generalizations he thinks one must know in order to know that some particular ball is red. Of course, knowing that "the ball is red" requires one to possess the concepts *red* and *ball*, and on some views of concept possession this will require knowledge of certain general, a priori conceptual truths (e.g., "anything that is red is colored"). But I do not think that the possibility that we have such a priori knowledge is what Norton intends to lean on here. Among other things, he explicitly tells us elsewhere that, "as an empiricist," he expects the relative justificatory chains to ultimately bottom out in experience (Norton 2005), and he certainly is not offering a rationalist response to Hume that depends on our having a priori knowledge of the most fundamental material postulates. (If that is what he was doing, I am sure that he would have said so.)

In any case, I can state my worry in a way that prescinds from this exegetical issue. Even if knowledge of singular facts presupposes "universal knowledge" in some sense, there does not seem to be good reason to suppose that all noninductive knowledge presupposes inductive knowledge. On the contrary, given his commitment to Prior Knowledge, Norton himself seems straightforwardly committed to the view that there was some noninductive knowledge before there was any inductive knowledge. My concern is that (1) given the kind of thing that material postulates are and the role that they play within the material theory, I do not see

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how we could come to know such things in a way that is wholly independent of broadly inductive considerations, but (2) by Prior Knowledge, we have to know at least some material postulates before we have any inductive knowledge at all.

Of course, presumably there is some true story to be told about why we are justified in reasoning inductively and why the Humean reasoning that purports to show otherwise is mistaken. That is just to say that I think that inductive skepticism is false. But that story is not itself provided by the material theory of induction, I think. Nor, as far as I can see, is there any reason to think that the story in question is available to the proponent of the material theory, as opposed to anyone else.

REFERENCES

Lange, Marc. 2002. "Okasha on Inductive Skepticism." *Philosophical Quarterly* 52:226–32.
Lipton, Peter. 1991. *Inference to the Best Explanation*. London: Routledge.
Norton, John. 2003. "A Material Theory of Induction." *Philosophy of Science* 70:647–70.
———. 2005. "A Little Survey of Induction." In *Scientific Evidence*, ed. Peter Achinstein, 9–34. Baltimore: Johns Hopkins University Press.
Okasha, Samir. 2001. "What Did Hume Really Show about Induction?" *Philosophical Quarterly* 51:307–28.
Salmon, Wesley C. 1967. *The Foundations of Scientific Inference*. Pittsburgh: University of Pittsburgh Press.

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