# For Universal Rules, Against Induction

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This essay criticizes John Norton's 2010 defense of the thesis that "all induction is local." Norton's local inductions are bound, if cogent, to involve general principles, and the need to accredit these general principles threatens to lead to all the usual problems associated with the 'problem of induction'. Norton, in fact, recognizes this threat, but his responses are inadequate. The right response involves not induction but a sophisticated version of hypothetico-deduction. Norton's secondary thesis—that if there is a general account of cogent scientific reasoning, then it is certainly not the one supported by personalist Bayesians—is also criticized.

**1. Introduction.** There are many scientific inferences that, while ampliative, seem clearly justified. Philosophers have generally held that the justification of such inferences is via appeal to some universal principles of 'inductive logic'—just as valid, nonampliative inferences are justified by appeal to the universal principles of deductive logic. John Norton has long argued (2003, 2005) that this approach was always bound to fail because no such universal principles are to be found; instead, as his favored slogan has it, "all induction is local": all cogent 'inductive' inferences depend essentially on facts that are contingent and domain specific.

Norton cites (2010, 766) two inferences:

1. This sample of bismuth melts at 271°C.
2. Therefore, all samples of bismuth do.

1. The temperature of the first day of the new millennium was 8°C at noon in Pittsburgh.
2. Therefore, all first days of new millennia in Pittsburgh will be so.

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The two inferences have exactly the same logical form, and yet number 1 is clearly cogent, number 2 clearly not. No universal principle will, Norton alleges, explain this difference—instead, the difference lies in contingent domain-specific facts about bismuth (and chemical elements more generally), on the one hand, and weather patterns, on the other.

There are two obvious worries—or rather, two routes into the same worry. First, how do the contingent domain-specific 'facts' themselves—about bismuth or chemical elements, more generally, in the case of the cogent inference 1—get to be accredited as 'facts'? Second, isn't Norton's thesis based on a non sequitur? It is surely true that inferences such as number 1 (and many much more sophisticated inferences in science as he and others [Glymour 1980; Zahar 1980; Harper 1991; Norton 1993, 1995] have shown) do depend on domain-specific principles of background knowledge and also true that articulating and defending those inferences are important tasks. However, it does not follow that a full justification of the inferences can be achieved without invoking universal rules. After all, we seldom 'in real life' spell out obviously cogent deductive inferences in the full, gory detail required to exhibit their validity in accordance with some formal system of logic: instead, in order to avoid taxing our audience's patience, we leave implicit required background premises ('enthymemes') that we believe will also be background knowledge for that audience, and we make leaps that we think will be obvious to that audience rather than doggedly following the rules of such a logic. Nonetheless, those inferences are cogent precisely because, once those enthymemes are added as explicit premises, the inferences become demonstrably valid according to such a formal system using its permitted, general rules.

So like Peter Achinstein (2010) and Thomas Kelly (2010), I will develop a version of what is, I suppose, the obvious response to John Norton's localism. The (admittedly less catchy) slogan for my view might be 'all (or at any rate, many) cogent inferences depend interestingly on local facts, but the justification of such inferences cannot be entirely local: appeal to some universal principles is ultimately inevitable'.

John Norton is not likely to miss obvious responses to his theses, and in earlier writings he has produced a rejoinder—one made more explicit in his essay (2010). The main part of my argument will aim to show that this rejoinder cannot work. And I will aim to redirect the debate toward an approach that does work. The alternative approach I endorse does not eliminate the need for some universal principles governing scientific inference if we are to avoid collapse into relativism—so I still do end up disagreeing with John Norton—but it does shed a different light on those universal principles than do the interesting criticisms of Achinstein (2010) and Kelly (2010). Indeed, my approach suggests that those general principles are not 'inductive' in any serious sense.

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**2. 'Deduction from the phenomena': Its Scope and Limits.** Consider again Norton's simple example of an apparently cogent (and of course local) 'inductive' inference—the inference 1 from "this (pure) sample of the element bismuth melts at 271°C" to "all such samples do." The inference is indeed ampliative: the conclusion has content that far outstrips that of the premise, and, hence, the inference is not deductively valid. It is 'local' because there is an underlying contingent and domain specific 'fact' that is the key to the 'licitness' of the inference. Norton now (2010, 766) states this as "the fact that elements like Bismuth are generally, but not assuredly, uniform in their properties." But (*a*) we clearly need to ask questions about how this general 'fact' is accredited as such on the basis of the (of course always finite) available evidence, and, even more obviously, (*b*) once we articulate this additional 'fact' and add it as an extra premise, then the local nature of the inference seems not to be underlined but rather to evaporate. It then, arguably, becomes an inference of something like the following form:

1. It is highly probable that a chemical element, one (pure) sample of which turns out to have some particular value of a well-defined parameter, such as a melting point, is such that all (pure) samples have that same value.
2. This pure sample of bismuth has a melting point of 271°C.
3. The melting point is a well-defined parameter.
4. So, it is highly probable that all pure samples of bismuth have a melting point of 271°C.

This is surely a less than perfect example exactly because of the fuzzinesses involved here, both with the probabilities and the notion of a well-defined (theoretically significant?) parameter—and I will turn soon to clearer examples (following earlier work by Norton himself, as well as myself and others). But, laying this aside, the inference now seems to be revealed as an arguably generally valid probabilistic inference.

Although in earlier papers Norton talked of the method he advocates as that of 'demonstrative' or 'eliminative induction', I always, in fact, took him to be one of several recent philosophers who, in response to some obvious defects in naive versions of hypothetico-deductivism, had rediscovered Newton's method of 'deduction from the phenomena'. This fell into disrepute in the heyday of unsophisticated hypothetico-deductivism largely because of the obvious question: how could anyone possibly deduce 'from the phenomena' a theory (such as Newton's principle of universal gravitation) that clearly transcends the phenomena in both generality and depth? The answer to the question, provided by the method's new defenders, was naturally that no one, not even Newton, can defy the laws of logic. Theories cannot be inferred 'from the phenomena'—except

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with the implicit help of some further background principles. For all his talk of "induction," the best way to construe Newton's method, as I have argued (2000), and in sharp contrast to Peter Achinstein's interpretation (2010), is as follows:

1. Scientists always work within a framework of various theories, results, and underlying general principles that are already accepted parts of 'background knowledge'—some of these principles are of extreme generality governing the whole process of science (such as the principle that 'Nature does not affect the pomp of superfluous causes'), others are still general but less so and subject to (relatively slow) historical variability (the basic principles of the mechanical philosophy, conservation of momentum, etc.).
2. The 'discovery' of a new specific theory often consists in (or, better, is readily reconstructible as) the result of finding new evidence ('phenomena') that, when taken together with those accepted principles of background knowledge, deductively entails that new theory.
3. This is, in turn, an important part of that new theory's accreditation: the new theory is shown by the deduction (given the 'phenomena') to be the only possible more specific representative of the already entrenched more general principles and theories supplied by 'background knowledge'.

One set of particularly simple examples of (relatively localized) deduction from the phenomena is formed by cases where some general theory is already accepted (of course, "on what grounds" is an important question) but involves a free parameter. We would like a more specific theory with this parameter fixed. The general theory specifies that the parameter value is a function of one or more observable quantities. Should the scientist sit on her couch and make a series of bold Popperian conjectures about the value of the parameter and then test it in the hope of eventually hitting on a specific theory that survives testing? Of course not: instead, she ascertains the relevant values of the observables, deduces the value of the parameter, and, hence, deduces the more specific theory 'from the phenomena'.

John Norton's bismuth case is almost, but not quite, an example. (It would be an example if we were ready to take as a background premise the claim that all pure samples of bismuth have the same melting point, but, in fact, we are only given the more general but not fully deterministic assertion that "elements like Bismuth are generally, but not assuredly, uniform in their [significant] properties" [Norton 2010, 766].) The following is an example that, because simpler, does fully conform to the above pattern.

The general classical wave theory of light characterizes what constitutes

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a monochromatic light source (no prismatic dispersion of the light from it), and makes various predictions about experiments involving such light, but was silent on the issue of the particular value of the wavelength of light from some particular monochromatic source. This general theory was very well supported by evidence and, hence, in the mid-nineteenth century, at least, would have been an acceptable background premise for a 'deduction from the phenomena'. Again, we would like a more specific theory that specifies the wavelength of light from particular monochromatic sources, say, light from a sodium arc. And, again, it would be madness to start making bold conjectures. Instead, we note that the general theory specifies that whatever light source is used, its wavelength will be related to measurable quantities in particular experiments.

So, for example, the general theory (modulo a couple of 'natural idealizations') entails that in the celebrated two-slit experiment,

$$\lambda = \frac{dx}{\sqrt{x^2 + D^2}},$$

where $\lambda$ is, of course, the wavelength, and all the quantities on the right-hand side are measurable—$d$ being the distance between the centers of the two slits, D the distance from the double-slit screen to the observation screen, and $x$ the distance from the middle of the central bright fringe to the middle of the next bright fringe (on either side). Once the experiment is performed using light from whatever monochromatic source is chosen, particular values of $d$, D, and $x$ can be measured and, hence, a value for the theoretical parameter $\lambda$ deduced. Once we have deduced a value for $\lambda$ in this way, then we have also deduced a more specific version of the general wave theory with this initially free parameter now fixed. This specific theory is then the uniquely experimentally determined version of the already accepted general theory, and, hence, the empirical support for that general theory is all inherited by the specific one (Worrall 2006).

Of course this is only one exceptionally clear, if scarcely exceptionally interesting, example of this methodological phenomenon. More interesting cases involve more deeply underlying principles and often a certain amount of 'generalizing' from restricted models to more general cases—as in the most famous instance: Newton's own 'deduction from the phenomena' of his principle of universal gravitation (Glymour 1980; Zahar 1980; Harper 1991; et al.).

I believe that John Norton and I are at one that deduction from the phenomena/demonstrative induction forms an important corrective to naive hypothetico-deductivism—both in terms of showing how the path to new theories can often be rationally reconstructed rather than being left in the darkness of alleged Popperian bold conjecture and in terms of supplying a more adequate account of empirical support. The fact that

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many, at least, of our scientific theories can be argued for in this way also explains, as John Norton has insightfully pointed out (1993), why scientists fail to see the 'problem' much loved by philosophers of (indefinite) 'underdetermination of theory by data'. New specific theories are very strongly (in clear-cut cases, maximally) constrained by data plus already accepted general theories and principles of background knowledge.

But deduction from the phenomena quite plainly cannot be the whole story of theory-accreditation. The obvious, and already flagged, question inevitably arises: how did the general principles involved as extra premises in deductions from the phenomena/demonstrative inductions themselves get to be accepted? What accredits the more general claim about the uniformity of chemical elements or the general claims involved in the basic (free parameter) version of the wave theory? After all, if we were to presuppose certain theories about weather uniformities—most straightforwardly that the noon temperature at any point on the planet is always the same on the first day of any millennium—then Norton's intuitively noncogent (2010) inference 2 can readily be justified. But we do not make such a presupposition because, unlike the one about the chemical uniformities, it is not rationally acceptable—indeed, it is presumably massively refuted, on the basis of the known evidence. The lack of cogency of inference 2 is a direct reflection of the fact that any additional theory that would make the inference valid is plainly rationally unacceptable.

Norton's talk of background 'facts' (such as the 'fact' that chemical elements are similar in their [significant] properties) rather than well-entrenched theories might lure the unwary into thinking that this problem does not arise. And it does not arise descriptively in those situations that those of us with sensible proscience intuitions would intuitively take to be cogent reasoning: of course it was reasonable, for example, for Newton to take the conversation of momentum as a background premise when inferring to his principle of universal gravitation. But as philosophers it is surely our duty, whatever some fashionable 'externalist', 'non-foundationalist' trends may suggest, to face up to the justificatory issue of exactly why it was reasonable. If we concede that anyone is allowed to start from whatever background knowledge they claim to have and make "deductions from the phenomena" or "cogent Nortonian inferences" from that vantage point, then we have surely conceded directly to relativism. If, as some suggestions from Norton suggest (personal correspondence; see also van Fraassen 1981), we take the view that we just have to swallow the fact that we always must start from somewhere and that the attempt to ground our starting point is based on a hopeless 'foundationalist' delusion, then we still surely have conceded to relativism. The fact is, for instance, that a large number of people's "background knowledge," to which they see no serious option as a starting point for further inference,

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includes the theory that everything in Genesis is literally true. It is not difficult within that framework to 'deduce from the phenomena' of the fossil record (or rather the so-called fossil record—really, just the indentations in the rocks and the apparent bonelike structures in desert sands) Philip Gosse's well-known (and surely paradigmatically scientifically unacceptable) theory (1857) that God created the world in the relatively recent past with those indentations already in the rocks, those bonelike structures already in the sands, and so forth. Unless we are ready to say that science is just 'one way of knowing' and the recent Earth creationist another such way, then we had better take on the task of saying why Newton was justified in using the background premises that he did in his inference, while Gosse was not justified.

So what justifies the general supraphenomenal premises involved in legitimate deductions from the phenomena/demonstrative inductions/cogent scientific inferences? Sometimes the answer will be 'same again': the general principles involved in some initially identified deduction from the phenomena/demonstrative induction are themselves derivable by the same process from other, even deeper-lying principles also available in background knowledge together with previously discovered phenomena. However, if we follow this backward direction, we clearly meet what seems to be an insuperable problem: the accreditational buck has to stop somewhere: it cannot be an infinite chain (or rather, tree, since more than one nonphenomenal premise will usually be involved in any 'demonstrative induction', and perhaps there will be more than one way of accrediting a given theory by this method). Even if we were to think, following Hume's thought experiment, of the starting point being Adam making some initial observation, we know that nodes in the tree must contain, at some stage, universal claims—and so we would still have to account for some initial act (or acts) of generalization. And given that we want each node to be justified, we would seem to be back at the same old problem.

The worry, then, just as Thomas Kelly suggests (2010), is that the so-called material, local theory of induction is 'just' a more detailed variant of the old story. It is, no doubt, much more accurate as a description of (some of) the 'inductive' practices in science than Hume's scientifically untutored talk about straight enumerative inductions, but in the end the "local" theory needs some sort of general ampliative principles that sanction steps from, say, data to accepted generalizations (and that sanction other more typical and interesting ampliative inferences in science), and the same (or essentially the same) justificatory issues arise concerning these principles as we have become used to through the voluminous literature on 'Hume's problem'.

It is not clear to me exactly what John Norton's view is on this. He seems to accept—at least in an earlier paper—that a reconstruction of a

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scientific inference as a legitimate "demonstrative induction" requires a justification of the background principles that are inevitably involved (2003, 666): "It must be stressed that the flight to demonstrative induction does not and cannot free us of the need to employ ampliative inference. Typically ampliative inference will be needed to justify the [background] principles of greater generality."

And he seems to accept that if those ampliative inferences themselves are reconstructed as "demonstrative inductions," then we are off down a chain of inferences leading into the distant past and, finally, to take the view that such a chain of inferences must, on pain of infinite regress, terminate. However Norton believes (as also noted by Kelly 2010) that this is not a problem for his local 'material' theory (2003, 668):

> [It] remains an open question . . . exactly how the resulting chains (or, more likely branching trees) [obtained from tracing the justifications backward] 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. . . . 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.

This is a very revealing passage. On one central point, I am in complete agreement—namely, that it is unclear how the trees will terminate (indeed, I would suggest terminally unclear). I will explain in a moment why I think that this terminal lack of clarity is nothing to worry about but is instead just an artifact of viewing the problem in the wrong way. But staying within Norton's approach for the moment, problems surely face this idea of 'benign termination'.

I am unsure what a 'brute fact' of experience is. But presumably brute facts for Norton here had better be singular: if so, then the problem has not been solved since the tree needs to go universal at some point; if not, then the problem is simply being presumed solved (à la Hume when outside his study). As for possible termination via 'prosaic facts' such as the "fact" that we are not consistently being deceived by a Cartesian demon (and notice the slip again from theory to "fact"): there is a danger that the problem is being 'solved' not just by fiat but by clandestine fiat; moreover, as again Kelly points out (2010), this is, anyway, not 'prosaic fact' enough,

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since Hume's problem arises even when we are given a mind-independent world (indeed, it is posed in such a way as to presuppose that world).

Notice also the hint (maybe more than a hint) of another approach in this most recently cited passage from Norton. The suggestion is that some terminations may be benignly achieved by invoking 'prosaic facts whose acceptance *lies outside the concerns of philosophy of science*' (emphasis supplied). Caricaturing: No doubt Achinstein, Kelly, and now this tiresome Worrall are right that some universal principles (or in Achinstein's case, universal 'forms of argument') need to be presupposed, so that induction is not really all local; but why grub around with this basic logico-philosophical stuff when you could be concerning yourself with the fascinating details of Newton's demonstration of universal gravity or Planck's demonstration of the quantum theory—details that are demanding enough without asking questions about some of the underlying premises? Maybe not all induction is local, but the really interesting inductive steps—those that "lie (or ought to lie) within the concerns of [real] philosophers of science" *are* local.

Whether or not this is Norton's view, I have some sympathy with it—largely because what one can say about the universal principles is so little (and, I fear, so dogmatic—Worrall 1999). But (*a*) let, if not a thousand, then at least two flowers bloom (even if the primary interest is in the local and often technically challenging detail, this does not mean that more basic, more 'philosophical' and universal issues do not arise that are of a different kind of interest); (*b*) whatever counts as 'interesting', the fact is that the problem has not been solved without a solution of the problem of the accreditation of these universal principles; and finally (*c*), as Peter Achinstein reminds us (2010), these general principles (notably again the claim—again claim, not fact—that 'Nature does not affect the pomp of superfluous causes') do play an explicit 'detailed' role in at least one deduction from the phenomena/demonstrative induction: Newton's 'demonstration' of universal gravitation.

However, let us not pursue these 'internal' criticisms too far, because the main point is surely that it is not just John Norton who is going to struggle to defend this general line. The whole idea of reconstructing our knowledge from bottom up in this way is surely a chimera. Surely these justificatory trees grow back into the mists of time to the emergence of homo sapiens and beyond.

It is tempting to believe 'no sure beginning, then no sure (current) ending'—and, hence, that if we accept what I just said, then we must surrender to relativism. This is presumably the thought underlying John Norton's pursuit of "benign terminations." But it just is not so.

The key is to recognize that reports of the death of hypothetico-deductivism (H-D) are greatly exaggerated (though, of course, it can only

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survive in a sophisticated form). Indeed, Newton's own method is surely a sophisticated version of H-D. It is sometimes forgotten that this method consists of two parts, not one—analysis (in which, as Newton put it, we derive the 'causes' from the phenomena) and synthesis (in which we take the 'causes' as given and test them by deriving further consequences). Peter Achinstein (2010) concentrates on the first part, but the second is nontrivial for Newton: in particular, 'synthesis' is not just a method of checking that your analysis was correct, by making sure that, having deduced T from phenomena P, T gives you back P by entailment. The nontriviality is established by Newton's admission that synthesis may lead you to modify the theory you arrived at by analysis: remember Newton's phrase, 'until yet other phenomena make such propositions either more exact or liable to exceptions.'

But, whatever Newton may have thought, the fact is that he starts his derivation of the principle of universal gravitation with the assumption of strict Keplerian ellipses (there is funny stuff going on, remember, in the move from the two-body model—one sun, one planet—to the general case) and finishes up with a theory that predicts the observed deviations from Keplerian ellipses, not to mention predicting other phenomena such as the known—but still in the proper sense predicted (Worrall 2006)—precession of the equinoxes and, of course, much later, the hitherto unknown existence of Jupiter.

It is these stunning predictive successes that give at least a large part of the credence to the premises from which demonstrative inductions begin—not some stepwise demonstration drifting back into the mists of time. 'Deduction from the phenomena' is an adjunct to H-D (when properly construed), not a replacement for it. "Deductions from the phenomena" in general guarantee that the new deduced theory will share the empirical success of the prior, accepted general principles. But the new theories arrived at in this way have in the really successful areas of science gone on to make often stunning predictions, and it is these successes that play a major role in underwriting the whole process. All that generally happens when you pull on your own bootstraps is that you either fall over or the bootstraps break (or both), but the development of science in the above sense does give us a real demonstration of bootstrapping—of the conclusions of deductions from the phenomena exhibiting independent success that, in turn, give greater justified credibility to the premises (as—at least in the full story—approximate truths).

Suppose this is correct. Does it mean that John Norton was (serendipitously) right all along—we do not need any universal principles to accredit science (except, of course, for deductive logic, which he gives us)? No. In giving a fundamental accreditational role to independent testability/predictive success, we are assuming what might be called the 'no miracles

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intuition', which itself centrally involves a notion of unity (Worrall 2011). Hence, we are presupposing some general truths about the world—among them, our old friend 'Nature does not affect the pomp of superfluous causes'. We are assuming that nature does not conspire against us by systematically hiding 'mechanisms' via systematic compensation (what Glymour 1980 called 'de-occamisation'). Without this assumption there would be no basis for the all-important distinction in confirmational weight between a theory's genuinely predicting a phenomenon and its merely accommodating it.

Hence, my main conclusion is that what is perhaps the obvious response to John Norton's localism is—unsurprisingly—correct (though I hope that the details of the response are not so obvious). There is no doubt that at least many cogent ampliative inferences are interestingly dependent on local 'facts'—that is, already established principles that are at least somewhat domain-specific. However, a full endorsement of the cogency of these inferences cannot be purely local but must instead rely on some universal principles. The most important of these is deduction (which John Norton cunningly concedes before starting and so its universality does not count against his 'local' approach), but other universal principles, such as unity, are involved too.

**3. Sliding Down John Norton's Dome.** There is a second part to John Norton's paper. Just in case you have not been convinced by the main argument and are still searching for general principles governing cogent inferences in science, then, Norton proceeds to argue, you will certainly not find them within personalist Bayesianism. Despite being far from a card-carrying Bayesian, I find these arguments unconvincing too.

Norton (2010) reintroduces an example that he earlier used for other purposes—"the dome." He argues that the (classical Newtonian) physics of the dome provides no physical chances for 'futures' of the system (more exactly, what will happen to a point mass placed at the exact apex of the dome). Instead, the physics specifies only that certain 'futures' are possible. John Norton makes a number of claims about the dome and its import, but the main one for current purposes is that the Bayesian "cannot responsibly analyze" it (2010, 765), and, hence, Bayesianism cannot be the sought-for universal system of 'induction'.

One immediate reaction is along the lines of 'hard cases make bad law'. Of course sometimes idealizations can be interesting and revealing, but there is just so much idealization going on in setting up the dome example that we should be very wary (to say the least) of drawing any general conclusion from it. Not only are we dealing with a point mass on a perfectly symmetrical dome but we have to assume the initial condition that the point mass is perfectly at rest at the exact point that is the apex. It is, therefore, unclear

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to me that it would be irresponsible for a Bayesian (even one with objectivist leanings) to hold that her probability for the dome's being really instantiated in the world is zero. Nor would it seem 'irresponsible' to say, alternatively, that although she is ready to assign degrees of belief to all propositions 'about the world' that can plausibly be taken to possess a truth value, this Bayesian feels no obligation to assign degrees of belief to nonphysical claims from applied mathematical fantasies (no matter how interesting they may be as applied mathematics). Hence, she would just refuse to assign a probability to statements like (S) 'the point mass sits at the exact apex of the dome and will spontaneously start to roll down the dome in direction $d$ at time $t$'. And she could do that without giving up the claim that personalist Bayesianism provides a universal logic for assessing the credentials of any claim from real physics or any real science.

But even laying this possible reaction aside, that is, even assuming that the Bayesian feels obliged to assign a probability to statements like S above, it is not clear that John Norton has raised a problem that a Bayesian will recognize as being even remotely troubling. What he is suggesting is, in fact, a sort of inverse 'Principal Principle' (at least as this is sometimes interpreted). The leading idea behind Lewis's (1980) formulation of this principle was that a rational agent should conform her personal degrees of belief (credences) to the (objective) chances. How to translate this leading idea into a precise principle has been a matter of some debate: the principle has sometimes been interpreted as requiring that, where an agent knows (it is never quite clear exactly how) that the objective probability of some event is $p$, then, provided that the rest of her evidence is 'permissible', her degree of belief in that event's occurring should be $p$; others argue that the principle is ineliminably conditional (e.g., Levi [1980] argued this exactly on the grounds that we have no independent access to objective chances except as things that satisfy the Principal Principle). Without taking a stand on this latter issue, we can see Norton as proposing an elaboration of the leading idea behind the principle: in cases where the objective chance of an event is nonexistent, then the rational agent must not (or cannot 'responsibly') assign a degree of belief to the claim that the event will occur. Since, according to his analysis, the physics of the dome delivers the theorem that there are no objective chances for any of the possible future histories of the ball sitting atop the dome, the Bayesian is in trouble.

The Bayesians, as already noted, may respond that they are perfectly happy to fail to analyze this case while not regarding that 'failure' as any sort of defect in their position because of the extremely idealized nature of the case. But in any event I cannot see how a Bayesian can be regarded as committed to John Norton's 'inverse Principal Principle'. The whole

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thrust of personalist Bayesianism, after all, is that you are allowed to have degrees of belief in anything you like and in any epistemic circumstances so long as those degrees of belief are coherent. It does not seem prima facie "irresponsible" to have a degree of belief in the horse 'Norton's Dome' winning the Kentucky Derby, even while accepting that not only do you not know the objective chance of its winning but there may well be no such thing as the objective chance of its winning.

John Norton replies that if an agent were to assign a degree of belief to S, then she would be "express[ing] mere opinion" (2010, 776), to which the personalist Bayesian will reply (assuming she has not taken the above line that she is under no obligation for principled reasons to assign degrees of belief to such super-idealized statements) "yes, of course, and the pope is a Catholic." 'Expressions of opinion' (constrained by overall coherence) are exactly what Bayesian degrees of belief are. While I share John Norton's evident lack of sympathy with personalist Bayesianism—largely on the well-known grounds that it is too personalist, too subjectivist to supply an adequate rationale for scientific reasoning—his latest argument seems to supply no new reason for challenging that position.

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