

Normative Naturalism and Epistemic Relativism

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In previous work, I defended Larry Laudan against the criticism that the axiological component of his normative naturalism lacks a naturalistic justification. I argued that this criticism depends on an equivocation over the term ‘naturalism’ and that it begs the question against what we are entitled to include in our concept of nature. In this paper, I generalize that argument and explore its implications for Laudan and other proponents of epistemic naturalism. Here, I argue that a commitment to naturalism in the methods and aims of science inevitably entails a kind of epistemic relativism. However, I argue that this should not be interpreted as a reductio of naturalism, since the admission of contextually based standards and aims of science does not result in quietism when it comes to important questions concerning scientific rationality.

1. Introduction

Larry Laudan’s normative naturalism has two parts. The first is methodology, which tells us how to interpret methodological rules in science. The second is axiology, or an account of cognitive aims in science, which is multi-faceted; it consists of a historical analysis of aim change in science and provides a model for large-scale change in science. It also provides a criterion for the rational abandonment of cognitive aims (Laudan 1984, 1987a, 1987b, 1988, 1990a, 1990b, 1996). According to Laudan, it is rational to abandon a cognitive aim if we discover that that aim is unrealizable, or utopian. Laudan’s critics have complained that the so-called realizability criterion is *non-naturalistic*; it is, they have rightly noted, a non-empirical, value-laden criterion (Doppelt 1986, 1990; Siegel 1990; Rosenberg 1996). Elsewhere, I have defended Laudan against this criticism (Freedman 1999). I have argued that, in the first place, its success depends on an equivocation over the term ‘naturalism’, and second, that it begs the question against what we are entitled to include in our concept of ‘nature’. If

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we interpret the concept widely enough, as I have argued we should, then we can help ourselves to a naturalistic justification for the realizability criterion. Specifically, it will be *our* criterion for guiding aim change because empirical results show it to be the dominant criterion that guides aim change in science.

Would Laudan be happy with my defence of his realizability criterion? I am not so sure. In this paper, I generalize my previous argument and explore some of its implications. I argue that the move to naturalize the realizability criterion has the consequence of rendering Laudan's normative naturalism vulnerable to a kind of epistemic relativism. If our best naturalistic justification for our various normative commitments, including our axiological constraints, is that said norms be read off the history and current practice of science, then the question of which norms will guide our meta-methodologies will be a contingent one, whose answer will depend on what our empirical investigations into science proper turn up. And this is what raises a problem for Laudan. He could hope to keep relativism at bay only if he could count on the realizability criterion, which states 'abandon any aim which is unrealizable', as a fixed constraint on aim change in science. But as a naturalist, Laudan cannot declare this a priori, and the suggestion that realizability is a contingent but universal feature of science raises a number of serious problems for his view on aim change in the history of science.

In what follows, I show that the significance of this point goes beyond Larry Laudan and his particular brand of naturalism. In general, the commitments we make as epistemic naturalists, namely to contextually based standards of evaluation and desiderata of theory choice, inevitably entail a kind of epistemic relativism. However, I argue that this result should not be interpreted as a *reductio* of naturalism. Although the admission of contextually based standards in science renders the naturalist defenceless against a form of relativism, this relativism does not render the naturalist defenceless against the usual charges of irrationality. Indeed, I hope to show that relativism need not result in *quietism* when it comes to important questions concerning scientific rationality.

2. Normative Naturalism

Laudan's normative naturalism has a good deal in common with the many accounts of naturalism in philosophy of science in the previous decade.¹ Like these other accounts, it is motivated by a kind of *scientism*, captured by the idea that 'science has been successful at producing the epistemic goods' (Laudan 1987a, 28). This idea, at least in Laudan's case, engenders a meta-epistemological thesis. Since science has delivered on the knowledge front, we ought to import the methods of natural science into our philosophies of science. Epistemology and science are thus viewed as 'epistemically of a piece' (Laudan 1990a, 50). This thesis gives rise to a theory of methodology and an account of axiology.

2.1. Methodology

Methodological rules in science, according to Laudan, are prescriptions whose surface structure is misleading, for while they often look like categorical imperatives, their

hidden structure is more similar to hypothetical imperatives. Thus, rules which appear to have the form ‘one ought to do x ’ in fact have the form ‘if one’s goal is y , then one ought to do x ’. As an example of this, Laudan cites Popper’s well-known dictum ‘avoid ad hoc hypotheses’, which he claims would be better formulated as the rule ‘if one wants to develop theories which are very risky, then one ought to avoid ad hoc hypotheses’ (Laudan 1987a, 24). If our theories about x and y tell us that x is the most effective way to achieve y , then we ought to adopt x (to achieve y). This means—ends or instrumental rationality is the essence of the methodology component of normative naturalism, and as long as we are somewhat clear on how to go about testing theory claims, we should have no difficulties testing rival methodologies.²

2.2. Axiology

Because methodological rules are warranted hypothetically, axiology becomes the pivotal feature of Laudan’s normative naturalism. His axiology has three distinguishable components. It consists of a historical analysis of aim change in science, it gives a model for large-scale change in science, and it provides a criterion for the rational abandonment of aims. The historical component is straightforward. According to Laudan, the fundamental aims of science have changed throughout the history of science. He argues that ‘the aims of science in particular and of enquiry in general have exhibited certain significant shifts through time’ (Laudan 1990a, 48), a claim which he illustrates with case studies from the history of science.³ This historical view has implications for Laudan’s account of methodological rules in science. Since rationality is instrumental rationality *and* the aims of science have changed over time, we cannot determine the rationality of science if we evaluate past theory and methodological choices with respect to *our* goals. We need instead to judge whether certain methodological rules have helped scientists to achieve *their* goals. The rationality of changing goals in science is captured by the ‘the reticulated model of scientific rationality’.

2.3. Reticulated Model of Scientific Rationality

The reticulated model of scientific rationality is designed to show what is wrong with the idea that aim change in science is non-rational. The hierarchical model of scientific change, which Laudan attributes to Kuhn, paints the following picture (Laudan 1984, 23). Factual disagreements in science happen at the lowest level of the hierarchy and are resolved by appeal to methodological rules. Methodological disagreements, in turn, are resolved by appeal to the level of shared aims. Disagreements here, however, are not rationally resolvable, at least not on this view; they are instead matters of taste (here we encounter a Kuhnian gestalt shift). The reticulated model, on the other hand, is represented as a triad consisting of theory, methodology, and axiology. On this model, justification flows both upward and downward, with no one level privileged over another.⁴ Instead, there is a reticulation or triangulation between theories, methods and aims, with one or two aspects of the triad remaining (temporarily) fixed, while another is being challenged for revision. This reticulation is how we arrive at a rational evaluation

of aims. Specifically, aims are evaluated on the basis of information supplied by theories and methods following one general mode of criticism, namely the utopianism, or unrealizability of aims.⁵

2.4. Utopianism/Unrealizability of Aims

In Laudan's (1984) *Science and Values*, he identifies three different 'utopian strategies', although in his later writings, these are subordinated to 'realizability in general'.⁶ In general, an aim is utopian or unrealizable if there is no ground for believing that aim can be actualized. Our beliefs about the world and about available methods of enquiry tell us when an aim is unrealizable. They tell us, for example, that someone whose aim is to be in two places at the same time has unrealizable aim (Laudan 1984, 51). According to Laudan, if an aim is thought to be unrealizable, then it is only rational to abandon it. As he states, 'if an agent comes to believe that a goal which he formerly espoused is *in principle unrealizable*, then continuing to hold that goal makes nonsense of the notion of rational action' (Laudan 1987b, 227).

As I noted earlier, Laudan's critics have argued that the realizability criterion is *non-naturalistic*. The various objections on this point have been aptly summarized in a one-liner by Harvey Siegel, who said 'That an aim is utopian (e.g.) may be established naturalistically; that a utopian aim ought not to be pursued is not' (Siegel 1990, 311). Siegel's point is that although picking through aims and selecting ones that are realizable is an empirical process, this criterion is itself normative. While Laudan has never adequately responded to this objection, there is, as I have argued elsewhere, a good response available to him. I will briefly summarize that response before generalizing its implications for Laudan's normative naturalism, and more broadly for naturalism in the philosophy of science.

3. Naturalisms

Essentially, the criticism against the realizability criterion gets its punch by equivocating on two different senses of the term 'natural'. We can contrast natural, i.e. empirical, with transcendental or supernatural. We can also contrast natural, i.e. fact, with normative, i.e. value. Naturalists, at least in the philosophy of science, are naturalists in the first sense given; they are opposed to supernatural or transcendental methods, i.e. a priori epistemology. They see knowledge as a part of the natural world and thus urge that our epistemological investigations be conducted as we would conduct any other scientific investigation, namely using empirical methods. These are naturalists cum committed empiricists, following Quine. With these distinctions in mind, we begin to get some idea of what is wrong with the criticism against the realizability criterion. It starts with the claim that realizability is a value-laden criterion, then goes on to claim that naturalists are anti-transcendentalists. From this, the conclusion is drawn that realizability is non-naturalistic. The premises are true, but the argument is invalid. It is missing a third premise that links the other two, namely the claim that naturalists in the anti-supernatural or anti-transcendental sense must also be anti-normativists. But this

claim, unlike the other two, is not obviously true. Indeed, it begs the very issue at stake for contemporary naturalists, that is, can we have a normative naturalism? As far as I can see, the answer to this question depends, in large part, on what we are entitled to include in our concept of 'nature'. This is an important issue for a naturalized epistemology and one that is presupposed by Laudan's critics who assume that no matter how extensive our empirical investigations are, we will not find the realizability criterion 'in the world', so to speak. Yet while realizability is indeed a value criterion, the question of what our values and epistemic norms are is an empirical one and thus can be answered naturalistically, that is, using empirical methods. And as it turns out, Laudan's endorsement of the realizability criterion appears to be based precisely on these grounds; so he states: '[Realizability] is a criticism which one regularly finds in scientific controversies' (Laudan 1984, 53).

4. Norms and Nature

As it stands thus far, what we have is a naturalistic justification for the realizability criterion. More generally, we have what amounts to a naturalistic justification for normativity. Just, as naturalists, we borrow the empirical methods of natural science, we are also entitled to import into our epistemologies the norms, aims, and values that we find prevalent in scientific practice. But it is at this stage that we can begin to anticipate the problems that lie ahead for resolute anti-relativist-naturalists like Laudan. For, as critics of naturalism will point out, however broadly you want to interpret the concept of nature, nature cannot tell us how to carve nature up. Once we have done our empirical investigations and we have a multitude of scientific norms on the table, there is the question of how to proceed. The naturalist has a limited arsenal with which to answer this question, for they must answer it in a way that does not violate their anti-transcendental commitments. In other words, it cannot be that once those norms are on the table, we use some a priori standard of appraisal that tells us which ones to follow, what to prioritize. The only guide we have, as naturalists, is what is already on the table; our prescriptions must come directly from our descriptions. Although he did not always put it quite this way, when pressed on the matter this was precisely Kuhn's answer to our present query:

Are Kuhn's remarks about scientific development, [Feyerabend] asks, to be read as descriptions or prescriptions? The answer, of course, is that they should be read in both ways at once. If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish. The structure of my argument is simple and, I think, unexceptionable: scientists behave in the following ways; those modes of behaviour have (here theory enters) the following essential functions; in the absence of an alternate mode *that would serve similar functions*, scientists should behave essentially as they do if their concern is to improve scientific knowledge. (Kuhn 1970, 237)

Following Kuhn's lead, then, our prescriptive 'oughts' should be culled directly from our descriptive 'is's'. And as naturalists we might wonder where *else* we could get our normative guidance from, if not from our current and past practices.

On the face of it, this move seems problematic, since it appears to be the naturalistic fallacy in its epistemic form. But the naturalist is in a unique position to make this move and just about get away with it. The requirement that we do not derive our prescriptions from our descriptions rests on the belief that statements of fact are logically distinct from statements of value, and as such no set of descriptive statements can entail a set of prescriptive ones. This applies to naturalist philosophy of science as much as it does to traditional a priori philosophy of science, but the difference is that the traditional philosopher of science adds insult to injury by compounding the logical error with a conceit that results in the universalization of value claims. They move, in other words, from a descriptive claim about epistemic practices in the here and now to a trans-historical prescriptive claim about the universality of epistemic standards. The naturalist, however, rejects this transcendentalist tendency. They are instead dedicated to ongoing empirical investigations into epistemic practices and consequently tempers their prescriptive claims with a fallibilism, to allow for changing norms, coupled with a recognition that their applicability might be limited to the culture from which we they are gleaned. Consequently, the naturalist's indulgence in the naturalistic fallacy is less grievous than it otherwise might be. Besides, as Laudan once said, 'where's the fun in being a naturalist, if one is not thereby licensed to commit the naturalistic fallacy?' (Laudan 1990a, 46).

5. Realizability Revisited

So where does this leave Laudan? The epistemic naturalist is entitled to a normative naturalism so long as the norms and values that they adopt in their epistemology are those that they discover through their empirical investigations of natural science. And they are even allowed to commit the naturalistic fallacy since unlike their a priori-minded friends, they are modest about the scope of their normative claims. The naturalist's principal responsibility, in general, is to maintain ongoing empirical investigations of science and have their epistemology reflect what they find therein. When it comes to constraints on aims, what this means is that the naturalist must accurately employ in their epistemology whatever values and norms are found to influence the abandonment or adoption of aims in science proper. While this opens the door to a naturalistic justification of axiological constraints, it inadvertently lets in certain unwelcome consequences for Laudan's model of the rationality of large-scale change in science. To see why this is so, we need to look more closely at the realizability criterion.

As we have just seen, the best naturalistic justification that we have for our axiological constraints is that those constraints are predominant in scientific practice. Accordingly, whether or not the realizability criterion is just such a constraint is an empirical question, one that can be settled only by studying aim change in the history of science. Yet, despite Laudan's implicit endorsement of the realizability criterion on these naturalistic grounds, he occasionally talks about it as if it were a permanent fixture in science (fuelling the confusion by his critics on this point). It is irrational, he claims, to strive for unrealizable aims. Does he mean irrational, given our current research tradition and corresponding set of theories, methods, and aims? Or is it always

irrational? He never quite says, but there are only a few possible answers to this query, and each one of them lands Laudan in a place that he does not want to be.

The best response that Laudan has to this question and the one that is most closely aligned with his naturalistic aspirations is that the realizability criterion is a contingent constraint on aims, dependent on our current research tradition and corresponding set of values. Naturalism aside, this response has another thing going for it as far as Laudan is concerned, which is that it goes well with his story about changing aims in science. After all, if Laudan's historical account is right, and there have been significant shifts in aims over time, then why not suppose that there have been shifts at the level of constraints on aims? This idea fits well within the broader picture of Laudan's normative naturalism; what it suggests is that the realizability criterion, like all other features of natural science, is liable to change, to reticulate. Indeed, one could argue that axiological constraints like realizability are best thought of as *second-order* aims, i.e. the kinds of aims that tell us when to abandon or adopt such *first-order aims* as 'find a cure for cancer'. If this and other axiological constraints (for example, 'strive only for fundable goals' or 'strive only for goals which have practical applications' or 'strive only for goals which are empirically testable') are best construed as aims, then we should expect them to reticulate along with other aims, methods, and theories. Either way, then, whether or not axiological constraints are themselves understood as aims, on this picture they will be a contingent feature of a scientific tradition, liable to change alongside other features of natural science. What this means is that none of these constraints—realizability included—can stand as a permanent, trans-historical feature of science. Yet, as we will see shortly, in order to avoid epistemic relativism Laudan's reticulated model needs precisely this kind of fixed constraint. But before turning to that problem, I want to consider a number of other possible responses to our present query.⁷

The answer just sketched suggests that realizability is a contingent constraint on aims, one that is dependent on current practices and liable to change alongside other (temporary) features of natural science. But perhaps that is too quick. One could make the case that even if the realizability criterion is a contingent constraint on aims in science, it is nevertheless universal, and thus immune to revision through reticulation. The idea here is that the while the criterion is a contingent one, it is one that is found in *all* our best current and past practices. This response is worth considering because, if successful, it would be a panacea for Laudan. It would enable him to posit an enduring, universal criterion for changing aims in science without thereby compromising his naturalistic stance. Unfortunately, there are a number of fatal flaws with it. In the first place, if the analysis presented above is correct, and axiological constraints are best understood as aims, then by Laudan's own account of aim change in the history of science, these constraints will be neither fixed nor universal. This worry draws attention to a more general point, which is that as an empirical hypothesis, this claim is open to refutation by the historical record. As such, we need to ask ourselves whether there is evidence which shows that 'strive only for realizable goals' is a universal axiological constraint in science. The answer to this question, I suspect, is that no such evidence is to be found. Certainly, there is no general consensus among historians or philosophers

of science that ‘unrealizability’ is the driving force behind aim change in science, and presumably a detailed examination of the history of science would prove the point. Fortunately, however, we do not need to go that far, since even Laudan is not prepared to defend this claim. Indeed, he is adamant that realizability is not the only constraint on aims in science or one that is universally present in controversies over aims. As he states: ‘Let it be clear that I do *not* maintain that every abandonment of a cognitive end in the history of science has been driven by a belief that the end was unrealizable’ (Laudan 1987b, 229; italics are Laudan’s). Thus, while it would prove helpful for Laudan if he could maintain that the realizability criterion was a contingent but universal axiological constraint, this is an unlikely story that he himself rejects out of hand.

There is one final response to the question of whether or not, for Laudan, it is *always* irrational to pursue unrealizable aims, which is that the notion of realizability is part of the meaning of rationality. The idea here is that it is implicit in the concept of a ‘rational action’ that the action be realizable. While there is something right about this response, it too faces a number of problems—most obviously that it implies an analytic (as opposed to naturalistic) justification for the realizability criterion. But to see what is really wrong with this response, we first need to see what is right with it, which is that it fits nicely with Laudan’s idea of methodological norms as hypothetical imperatives. After all, if rationality is instrumental, such that acting rational is finding the best means to one’s ends, it seems reasonable to suppose that to act rationally, one must have some idea of how to realize one’s ends. But this assumption turns out to be problematic. The idea that realizability is a necessary condition for rationality, or otherwise part of the meaning of the term, puts unwarranted pressure on our ordinary understanding of rationality (thus also putting pressure on Laudan’s account of methodological rules as instrumental, but that is another problem). Certainly, if it were the case that actions had to be realizable to be rational, we would be forced to deem irrational a large amount of behaviour that we would commonly judge as perfectly rational. So, for example, Laudan claims that the goal of ‘true theories about the world’ is epistemically unrealizable, since, he argues, even if we were to meet this goal, we would not be in a position to know it (Laudan 1984, 52–53). Of course, this is open to question, but suppose we grant Laudan the point; is it thereby *irrational* to strive for truth? Do we think that scientific realists, for instance, are *irrational*? Certainly not. We might think that the realist is wrong or reject their implicit attachment to one or another theory of truth, but we do not think their thereby irrational. The same can be said for those people who strive for the goals of ‘world peace’ or ‘ending poverty’, or even ‘eradicating all disease’. These goals may be ‘demonstrably unrealizable’ (in Laudan’s terminology), but we do not typically classify as irrational those individuals who attempt to promote them. This current practice seems to me exactly right. Arguably, what cases like these show is that if the goal in question is believed to be worth while, it can render rational any action that attempts to promote it, even if that goal is in principle unrealizable. This is not only true of the lofty goals just mentioned but also true of our more mundane personal goals such as ‘always get a good night’s sleep’ or ‘never fight with your mother’. It is not even obvious that it is irrational to aim for goals that appear to be physically unrealizable, such as Laudan’s example of being in two places at once;

indeed, contemporary physics is peppered with projects whose goals, like this one, seem unrealizable.⁸ To be clear, none of this contradicts the empirical claim that realizability has at times functioned as an axiological constraint in science; Laudan's historical case studies establish at least this much (Laudan 1984, 51–53 and 82–87). But it does put pressure on the idea that realizability is a necessary condition for rationality or somehow part of the meaning of the term. Certainly, the idea is debatable and contradicts many of our ordinary intuitions about what it means to be rational. Thus, whether or not Laudan had this in mind for his realizability criterion is almost beside the point.

We are now in a position to conclude that, from a naturalistic perspective, there is no basis for the claim that realizability is a fixed or universal axiological constraint. Moreover, the above analysis gives us some insight into what is wrong with Laudan's insinuation that it is *always* irrational to strive for unrealizable goals. It might be that I think it is perfectly rational to strive for unrealizable goals so long as those goals are worthy ones, such as true theories about the world, world peace, not fighting with my mother, or time travel. On the other hand, I might think it is irrational to strive for goals that, while realizable, are not fundable or empirically testable, or have no practical applications. What we can conclude from this is that even if aims in science turn out to be subject to rational adjudication, importantly, what counts as rational is itself contingent and variable. This means that there can be no criterion, realizability included, which can stand as an enduring, trans-historical constraint on aim change in science.

This conclusion can be generalized beyond Laudan's particular brand of naturalism. It is true not just of the realizability criterion, in other words, but also of the other norms, values, or aims which the epistemic naturalist borrows from natural science. If naturalism is going to be a meaningful way of doing epistemology, then the naturalist must remain committed to ongoing empirical investigations of scientific practice and have their epistemology reflect the contingencies found therein. Thus, the naturalist must be a fallibilist with respect to criteria for guiding aim change, and adopt in their epistemology whatever criteria are dictated by empirical results into the workings of scientific practice. This is a conclusion that Laudan *should* be happy with as it fits well with the naturalistic character of reticulation in his model of scientific rationality and with his naturalism more generally. However, as we will see below, it raises the issue of epistemic relativism—the very problem that, with his reticulated model, Laudan set out to avoid.

6. Realizability, Reticulation, and Relativism

If the above argument is correct, then the realizability criterion cannot stand as a trans-historical or universal axiological constraint. Just as aims, theories, and methods are historically contingent and act as constraints on one other only within the context of a particular research tradition, so too will second-order aims like realizability function as constraints on first-order aims only within the context of a given research tradition. As I indicated above, this result is perfectly in sync with epistemic naturalism, but it is problematic for other reasons, or at least it is problematic for Laudan. As the previous

discussion has foreshadowed, without a trans-historical feature or set of unchanging aims in science, talk of rationality is necessarily going to be relativized to particular research traditions.

To see exactly why this is so, we can look again at the reticulated model of scientific rationality. Recall that this model is supposed to show how change from one research tradition to another can be construed as rational. Accordingly, Laudan illustrates how change from one research tradition to another happens in a piecemeal fashion, how certain standards of a tradition remain stable, and how against these stable elements we are able to judge whether others should be adopted or dropped. What this demonstrates, significantly, is that we can speak of rational change within a particular research tradition or triad. Within any given research tradition or triad, there will be *reasons* for adopting or rejecting particular theories, methods, and aims, and what makes these reasons good or bad are the values within the triad. What counts as a good reason for change, in other words, will depend on the values that are present within a given scientific community. These values are what shape our axiological constraints, such as ‘strive only for realizable goals’ or ‘strive only for goals with practical applications’, and these constraints, in turn, are what guide us when we are trying to resolve tension within that triad. Indeed, it is the values within a given research tradition that dictate, in the first place, that we ought to harmonize a triad or work to resolve tensions that arise therein. It could be that for some research tradition down the line, tension is not something that needs to be overcome but is instead seen as desirable (perhaps as an expression of the diversity of its members). The point is that for the naturalist, what will count as a good reason for a change in aims will itself be contextual, contingent on a given research tradition. And if our examination of the realizability criterion is any indication, these contingent features will not also be universal. More likely, we can expect there to be a proliferation of ‘good reasons’ and ‘bad reasons’, each relative to its own research tradition. There are those—arguably Kuhn—who would contend that this is precisely what the historical record shows.⁹ Indeed, recent work in feminist philosophy of science demonstrates this idea that the values that drive research traditions are neither neutral nor universal, but dependent on a variety of different factors (such as gender) connected to the context knowledge production, resulting in different and potentially contradictory standards of evaluation in science.¹⁰

Thus, while Laudan’s reticulated model gives us the resources required for measuring a rational change of aims, theories, and methods within a research tradition—the aspects of a triad that remain stable during periods of transition are what dictate this—because these values are themselves variable, change between research traditions will be underdetermined by competing standards of rationality. In other words, without a fixed axiological constraint, we lose the ability to adjudicate on the rationality of aim change in those cases where there is conflict between research traditions. Without an Archimedean standpoint, there is no *really* rational choice in aims, as distinct from those choices that look rational according to one or another set of local standards. Now, to be clear, for the naturalist who eschews a priori rationality, this sounds exactly right, which is why there is some reason to think that Laudan should be happy with this conclusion. But there is no mistaking that it results in a kind of Kuhnian epistemic

relativism, which is precisely what Laudan's reticulated model of scientific rationality was designed to overcome (and which is why there is also some reason to think that he would not be happy with this conclusion). For if our evaluations of the rationality of a change in aims (or theories, or methods) can come only from the perspective of either the new triad or the old triad, then in cases where norms of rationality conflict, in cases, in other words, where the stakes over the goals of science are generally high, changes in aims will be underdetermined by competing standards of rationality and will be due instead to various non-rational factors.

7. Relativism, Quietism, and Norms of Rationality

With the reticulated model of scientific rationality, Laudan attempts to show what is wrong with the (Kuhnian) idea that aim change in science is due to non-rational factors. By providing a constraint on aims, namely the realizability criterion, Laudan illustrates that there can be good reasons for abandoning aims in science; this, in turn, purportedly blocks the radical relativism that he attributes to Kuhn. What the above discussion shows is that this effort is undermined if realizability is not a fixed criterion, for then what counts as a good reason for abandoning aims in science will itself be variable. Disputes over the abandonment of aims move down one level to disputes over axiological constraints, and we are left, once again, with epistemic relativism. Moreover, this point can be generalized beyond Laudan's particular brand of naturalism. In so far as Laudan's normative naturalism lacks the resources to avoid epistemic relativism, other versions of epistemic naturalism will be in no better position. As we have just seen, we cannot have a naturalistic justification of normativity, in virtue of an empirical warrant of said norms, and at the same time claim that these norms stand outside a particular cultural moment. Norms, including norms of rationality, are contingent and contextual, and vary relative to the diverse communities that drive scientific practice.

This outcome would be anathema to many naturalists, not least of all Laudan. Although, to some degree, he has abandoned the a priori rationality of the 'arch-rationalist' philosopher of science, he is still committed to the privileging of scientific rationality that is epitomized by that tradition. If that is right, then one way for him to get around the present conclusion would be to posit some a priori universal principle of rationality. Of course, this solves the problem of relativism but at the expense of a fully naturalized epistemology, and thus is not much of a solution at all. A better response, I think, would be to reconsider what our reasons are for rejecting relativism, in the first place. If this form of epistemic relativism were pernicious, for instance, then we would have good reason to reject it. But I think there is some reason to believe that it is rather benign. One way to decide this issue is to see whether or not this form of relativism collapses into quietism when it comes to important questions concerning scientific rationality. If we accept epistemic relativism, in other words, will we be deprived of the ability to make meaningful evaluations regarding the rationality of adopting this or that theory, method, or aim? The answer to this question is an emphatic 'no', and Laudan's reticulated model of scientific rationality tells us why.

What this model illustrates is that during stable periods of scientific activity (what Kuhn would call ‘normal science’), there will be a set of agreed upon norms of rationality against which we can measure the rationality of changing aims, methods, and theories in science. Thus, where there are shared standards of rationality, scientific change, including changes in aims, will not be underdetermined by good reasons. In this respect, the reticulated model offers us a way of understanding scientific change that permits us to talk meaningfully about rational change within a scientific community or tradition. Indeed, Laudan’s analysis illustrates how the epistemic relativist is able to preserve a number of important conceptual distinctions in light of the admission of contextually based standards of rationality. The relativist can, for instance, maintain the distinction between what an individual thinks is rational and what, given the standards of their community or tradition, *is* rational. And this further enables their to distinguish those norms or values of a research tradition that are ‘objective’, in so far as they are intersubjectively agreed upon and verifiable. Aims, methods, and theories are thus subject to rational evaluation, according to local standards. Still, there is no denying that cases of competing standards of rationality and divergent axiological constraints will result in a proliferation of ‘acceptable’ aims, where what counts as acceptable or rational will be relative to particular communities—this is epistemic relativism, after all. But all that we really lose, qua relativists, is the ability to give an overall ranking of these divergent norms of rationality. For the naturalist who already eschews any kind of normative transcendentalism, this is not losing much at all.¹¹

8. Conclusion

If the above argument is correct, then a commitment to epistemic naturalism inevitably entails the rejection of trans-historical aims, methods, or standards of science. Indeed, as I have described it, it is for this reason that the path of the epistemic naturalist leads directly to epistemic relativism. I hope to have shown that this consequence need not be a *reductio* of naturalism, since, importantly, the admission of contextually based standards and aims of science does not result in quietism regarding important questions concerning scientific rationality. With Laudan’s reticulated model, for example, we can talk meaningfully of rationality within a tradition, and we can talk about progress with respect to our culturally situated goals. Indeed, the relativist is entitled to say what most sensible people think, namely that there are good reasons and bad reasons for changing theories, methods, and aims in science. In certain circumstances, some changes will be rational—objectively rational—and others irrational, and the values and norms of a given scientific community are what constitute the relevant circumstances. Granted, we lose the ability to compare one tradition against others with divergent norms of rationality, but this should not be too troubling for the naturalist. Indeed, I hope to have shown that for someone who already eschews transcendentalism, the epistemic relativism that is a consequence of the naturalistic commitments of Laudan and others is not such a far or fatal jump.

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Notes

- [1] See, for instance, Giere (1985, 1989), Stump (1991, 1992), and Kitcher (1992, 1993).
- [2] In Freedman (1999), I discuss a number of problems facing the methodological component of Laudan's naturalism.
- [3] A number of Laudan's critics reject this historical picture (and the issue is further confused by semantics, as what some call a method others call an aim); see Leplin (1990, 24) and Rosenberg (1990, 36). Knowles (2002, 176–78) also disagrees with this historical story, and argues further that it renders untenable Laudan's account of progress in science.
- [4] See Laudan (1984, 63) for a helpful diagram of the reticulated model.
- [5] Laudan (1984) also identifies a second criterion for the rational evaluation of aims, which is the harmonization of implicit and explicit aims, but this criterion is virtually absent from his later writings; it is the realizability criterion that Laudan continues to rely on (Laudan 1987a, 1987b, 1990a, 1990b, 1996).
- [6] See Laudan (1984, 51–53) for examples of three utopian strategies: demonstrable utopianism, semantic utopianism, and epistemic utopianism.
- [7] Some of these responses were helpfully pointed out to me by the reviewers at *ISPS*.
- [8] One example of this can be found in the work of Mallett (2000, 2003), a distinguished physicist at the University of Connecticut whose current research looks at weak and strong gravitational fields as a basis for time travel.
- [9] Of course, while it seemed to everyone (except perhaps Kuhn himself) that he was committed to this kind of epistemic relativism in *Structure* (1962/1970), Kuhn (1970) later argued that the rationality of science could be partially preserved in light of the fact that the history of science presents us with a more or less constant set of (five) objective values.
- [10] See in particular Longino (1995, 2002).
- [11] Knowles (2002, 179) disagrees. He argues that this form of relativism is pernicious, but for reasons different from those examined here.

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