

THE PROSPECTS FOR NORMATIVE NATURALISM

A Thesis

by

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MASTER OF ARTS

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THE PROSPECTS FOR NORMATIVE NATURALISM

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Abstract

This thesis is an investigation and defence of normative naturalism, as it is formulated by its most outspoken proponent, Larry Laudan. Normative naturalism is a contemporary epistemology of science, or meta-methodology. It is naturalistic in that it sees the epistemology of science as tied up inextricably with the history of science. It is normative in that it goes beyond a mere description of the workings of science; it is prescriptive, it tells us how we ought to act. Generally speaking, normative naturalism tries to provide an answer to the question: What warrants a methodology? The answer Laudan gives is that methodological rules are warranted hypothetically, in reference to cognitive aims, following the principle of means/ends, or instrumental rationality. So, for example, the methodological rule 'one ought to do x ' is translated as 'if one's goal is y , and x is the best means to y , then one ought to do x '. Since methodological rules get their prescriptive force by reference to aims, cognitive aims, or axiology, play an important role in Laudan's meta-methodology. These two features: Axiology and methodology, are the main components of Laudan's normative naturalism. I spend the first chapter of this thesis elaborating on Laudan's position, and the following four chapters responding to criticisms of it.

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Introduction

My aim in this thesis is to investigate a contemporary epistemology of science, namely 'normative naturalism', as it has been developed by its most prominent exponent, Larry Laudan. The purpose of this thesis is to show how Laudan's normative naturalism can achieve success as an epistemology of science. This subject is currently a hotbed of controversy; there are many criticisms against which one could defend Laudan's normative naturalism. I have had to limit myself to the most important of them. Particularly, I have chosen four main forms of criticisms of Laudan's account, each one occupying its own chapter of this thesis. The purpose of this introduction is to locate normative naturalism. What follows is a taxonomy of the main divisions in contemporary philosophy of science.

According to one of the most prominent figures in the field, Ernan McMullin, within the philosophy of science there are two general branches: The epistemology of science and the ontology of science.¹ The epistemology of science is interested with science insofar as science is a way of knowing. As McMullin puts it, the epistemology of science "is a general methodology of empirical science."² The ontology of science, on the other hand, is concerned with the ontological relevance of the claims made by science. McMullin suggests that the ontology of science is essentially reducible to a single question: "To what extent do the postulational structures of science reveal a "real" structure, whether of the

¹ Ernan McMullin, "The History and Philosophy of Science: A Taxonomy," in Historical and Philosophical Perspectives of Science, Roger Stuewer, Ed. Minnesota Studies in the Philosophy of Science, Vol.V (Minneapolis: University of Minnesota), 1970. All page references are to the reprint in Janet A. Kourany Scientific Knowledge: Basic Issues in the Philosophy of Science, (Wadsworth Publishing), 1987. pp. 3-19.

² McMullin, 1987, p.10.

world or of the human mind?"³ This split, between the epistemology of science and scientific realism, is a commonly accepted division of labour in the philosophy of science; McMullin's comments are echoed in the words of another eminent figure in this field, Ian Hacking, who states: "The two current issues of philosophy of science are epistemological (rationality) and metaphysical (truth and reality)."⁴ Laudan's normative naturalism is an epistemology of science; thus, in this thesis I disregard (as much as philosophically possible) any questions regarding scientific realism.

To further situate Laudan's normative naturalism, within contemporary epistemology of science there are two contemporaneous movements. The main contenders in this debate are the sociologists, on the one side, and the rationalists (predominantly philosophers), on the other.⁵ The sociologists attempt to understand science through non-cognitive explanations; according to James Robert Brown, "They preach (and practise) a radically sociological approach to the understanding of how knowledge (scientific knowledge, in particular) is acquired."⁶ The traditionalists in this debate are the rationalists. As Brown states, they "see 'evidence', 'good reasons', and 'rational belief' rather than non-cognitive 'interests' as the guiding force behind the development of science."⁷ Laudan, and his normative naturalism, belong to the rationalist tradition.

One further distinction can be drawn: Within contemporary rationalist

³ McMullin, 1987, p. 10.

⁴ Ian Hacking, Representing and Intervening, (Cambridge University Press), 1983, p.112.

⁵ For the best and most up to date overview of this debate see James Robert Brown, The Rational and the Social, (Routledge), 1989.

⁶ Brown, 1989, p.vi.

⁷ Brown, 1989, p.vi.

epistemology of science, there are two opposing groups.⁸ One group of philosophers holds that a philosophy of science should be independent of the history of science; this group advocates *a priori* methodology, and is a continuation of the positivist tradition in the philosophy of science.⁹ The other group of philosophers proposes that a philosophy of science should be sensitive to the history of science; proponents of this group think that “scientists are good at doing science and consequently that there is an evidential relationship between the history of science and the normative philosophy or methodology of science.”¹⁰

Laudan is on the side of history; his normative naturalism rejects *a priori* epistemology. According to Laudan, within this group of philosophers of science,

All these authors developed models of scientific change and progress which, they insisted, were based upon and supported by empirical study of the workings of actual science, as against the logical or philosophical ideals of epistemic warrant emphasized by the positivist tradition. All made it a hallmark that the philosophy of science be rooted in and responsible to its history.¹¹

If Laudan (et al.) is right, and we can have a thoroughly naturalized normative epistemology, then, indeed, his position has a deep attraction in today’s debates. Positivism’s reign is long since over, and its traditional privileging of logical analysis is no longer taken for granted as the necessary method of choice amongst North American analytic

⁸ Brown, 1989, p.98.

⁹ In this group we find Gerald Doppelt, Harvey Siegel, and John Worrall, to name a few.

¹⁰ Brown, 1989, p.98. Also included in this group are David Stump, James Maffie, Jarrett Leplin, and Alexander Rosenberg.

¹¹ Larry Laudan, Arthur Donovan, Rachel Laudan, Peter Barker, Harold Brown, Jarrett Leplin, Paul Thagard, Steve Wykstra, “Scientific Change: Philosophical Models and Historical Research”, in *Synthese*, Vol. 69, 1986, p. 143.

philosophers.¹² Also, these philosophers are today calling into question the validity of *a priori* propositions, not least of all those about knowledge. So, not surprisingly, the resurgence of naturalism is receiving both attention and applause. Yet, the yearning for normativity has far from subsided. Most of us today still want to know how to act best. We want a naturalistic epistemology to “present a compendium of cognitively optimal processes for all those contexts in which human subjects find themselves.”¹³ If epistemology is to remain a normative discipline, then at least one of its main tasks is to specify those strategies which promote attainment of cognitive goals. Laudan’s normative naturalism attempts to do just that; its success, however, is not uncontroversial. But this is something, throughout the course of this thesis, that each person can judge for them self.

Chapter one of this thesis is mainly exegetical. To start with, I give a general overview of epistemic naturalism. Then, I introduce Laudan’s normative naturalism, and outline its two main components: Methodology and axiology.

In chapter two I reply to the objection that Laudan’s descriptive axiology, i.e. his view regarding aim variance in the history of science, leads to the problem of relativization. The idea here is that methodological contingency on variant aims entails that methodological rules will have to be relativized to contexts where aims are shared. I show, however, that aim variance does not detract from the universality of the principle of instrumental rationality: Whatever your end, adopt whatever method

¹² Tyler Burge, “Philosophy and Language and Mind: 1950-1990”, in *The Philosophical Review* Vol.101, No.1, 1992, p. 28. Burge is right to note that the boundaries for what counts as ‘analytic philosophy’ have been sufficiently blurred, so that today the term lacks both force and a clear meaning; no doubt a partial cause of this blur is the displacement of logical analysis from its privileged home.

¹³ Philip Kitcher, “The Naturalists Return”, in *The Philosophical Review*, Vol.101, No.1, 1992, p.76.

which will best promote the attainment of that end.

In chapter three I look more closely at Laudan's account of rational aim change. Here, I examine the objection that the main criterion guiding rational aim change, i.e. the utopianism of aims, is non-naturalistic. To respond to this I elaborate on the definition of naturalism, and end up with the following two definitions: Naturalism-one, which states that something is naturalistic if and only if science does it, and naturalism-two, which states that something is naturalistic if and only if it has quality P, where P equals empirical testability. I conclude that, given these definitions, Laudan's criterion of utopianism, or realizability, is indeed naturalistic.

In chapter four I examine a cluster of objections aimed at Laudan's position, which charge that his descriptive view on aim change in the history of science poses a problem for his methodology *and* his axiology. The main objections here are that, first, changes in aims and methodologies do not make sense if there is not a transhistorical goal of science, and second, that without such a goal the notion of progress is essentially empty. I show that these problems can be easily answered once we consider both Laudan's account of piecemeal change within a historical triad, and his notion of progress as relative progress.

In the fifth chapter of this thesis I respond to the final obstacle facing Laudan's normative naturalism: Since the realizability criterion does not uniquely pick out aims, is the reticulated model of scientific rationality capable of meeting the demands of a strong normative criterion? That is, can it tell us 'you ought to do x', even if that categorical proposition is contained in a hypothetical one. After I draw the distinction between two forms of categorical propositions: the permissible sort and the obligatory sort, it becomes clear that Laudan's axiology gives us a perfectly robust

sense of strong normativity.

In the end, I conclude that the prospects for Laudan's normative naturalism are good. That it is capable of doing all that we would expect from an epistemology of science. It has a naturalistic criterion which enables us to rationally evaluate cognitive aims, and it instructs us, once we have those aims, to follow the methodological principle of instrumental rationality for most efficient means/ends realizations.

Chapter One

What warrants a methodology? Since the mid 1980's, a decade after the publication of his Progress and its Problems (1977), Larry Laudan has argued that the answer to this question can be found in a "naturalistic theory of methodology which preserves an important critical and prescriptive role."¹ Laudan details his position in the first of a series of articles on his normative naturalism, entitled "Progress or Rationality? The Prospects for Normative Naturalism"². In this mainly exegetical chapter I give a basic introduction to that position; this will provide the necessary set up for the evaluation of Laudan's normative naturalism which occupies the following four chapters.

First, I will give a general overview of epistemic naturalism; I discuss how naturalism in the philosophy of science is typically motivated and I go over a common attack on it, namely the 'naturalistic fallacy' (in its epistemic form). Then I turn to the particular case of Laudan's (1987) 'normative naturalism'. Normative naturalism is a meta-methodology³ comprised of two basic components: Methodology and axiology (i.e. cognitive aims). The methodological half of Laudan's normative naturalism is relatively straightforward and, when viewed independently of axiology, unproblematic. After I trace out the rudiments of Laudan's methodology I turn to his axiology. Laudan's axiology, we will soon see, is central to his normative naturalism. It underscores his methodology, which he claims is

¹ "Progress or Rationality? The Prospects for Normative Naturalism", in American Philosophical Quarterly, January, 1987, Volume 24, p.29.

² See Bibliography for other Laudan references.

³ 'Meta-methodology' is defined as a meta-epistemology, i.e. an epistemology *about* the methodology of empirical science. Thus, Laudan's normative naturalism is in fact a species of meta-methodology.

"parasitic on a given set of cognitive ends".⁴ In this chapter I introduce two key features of Laudan's axiology: His view regarding aim variance in the history of science, and the reticulated model of scientific rationality which dictates the conditions under which aim change is rational.

Before I look at Laudan's normative naturalism, it is important to have some idea what is generally meant by 'naturalistic epistemology'. Epistemic naturalism is best understood as meta-epistemology, a view *about* epistemology. It is motivated in part by the 'historical turn' in the philosophy of science. The historical turn was inaugurated by Thomas Kuhn's seminal The Structure of Scientific Revolutions, with its famous opening sentence "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed."⁵ Thanks to Kuhn's initiatory work, history did indeed produce a definite transformation in the philosophy of science; as Ronald Giere notes, "Although he did not use exactly these words, Kuhn was advocating a *naturalized* philosophy of science."⁶ In effect, the historical turn charges that an epistemology of science (i.e. a theory of rationality) should fit with the actual record of how science has been successful; in other words, the history of science matters to normative epistemology. It is this esteem of the scientific enterprise, and a consideration of its history, which defines epistemic naturalism.⁷ Hence, all versions of epistemic naturalism look to the natural sciences for answers to epistemic questions, and dictate that we should adjudicate knowledge

⁴ Laudan, 1987, p.29.

⁵ Thomas S. Kuhn, The Structure of Scientific Revolutions, (University of Chicago Press, 1962, 1970) p. 1 (all references are to the 1970 second edition).

⁶ Ronald N. Giere, "Philosophy of Science Naturalized", in Philosophy of Science, Vol.52, 1985, p. 332.

⁷ This preliminary definition of naturalism is elaborated in chapter three, pp.38-40 below.

claims the same way we would claims in science.⁸ Generally speaking, epistemic naturalists would like us to see philosophy and science as woven of the same cloth.

Epistemic naturalism is also motivated in part by the rejection of a traditional positivist conception of knowledge. As Philip Kitcher notes, "In recent years, confidence in conceptual analysis and in 'first philosophy' has begun to waver."⁹ Some argue that the rejection of traditional positivist epistemologies is due to the inability of methodological a prioriism to come up with a single *a priori* stance to warrant methodology. As Jarrett Leplin states, "If methodology is a priori, then a priori reasoning should be able to adjudicate methodological controversy. And this it manifestly fails to do."¹⁰ The failure of *a priori* epistemology is yet further confirmation for the naturalistic turn in philosophy of science: Since history matters, epistemology cannot be solely an *a priori* enterprise.

It should now be clear that the fundamental tenets of epistemic naturalism include a rejection of *a priori* epistemology, and a reflection on the ways in which quintessential seekers of knowledge, i.e. scientists, have actually gone about their business. To get a feel for what a view like Laudan's normative naturalism is up against, notice that even this bare outline of epistemic naturalism invites objection. Science, the objection goes, has historically been conceived of as a descriptive enterprise; philosophy, on the other hand, is prescriptive in nature. How, then, can they be 'woven of the same cloth'? How can descriptive claims about

⁸ Larry Laudan, "Normative Naturalism", in Philosophy of Science, 1990, Vol. 57, p.44. As it turns out, history is not the only foundation for a naturalized epistemology. Quine, for example, used psychology to ground his naturalism.

⁹ Philip Kitcher "The Naturalists Return", in The Philosophical Review, Vol.101, No. 1, 1992, p.55.

¹⁰ Jarrett Leplin "Renormalizing Epistemology", in Philosophy of Science, 1990, Vol. 57, p.21.

knowledge and prescriptive claims about knowledge be subject to the same sorts of adjudication? This age-old objection, the naturalistic fallacy in its epistemic form, is an initial hurdle which all naturalists must overcome. And it is enough for some naturalists to give up the normative project - but not Laudan. Laudan contends that his meta-methodology can properly, successfully fulfill a normative role, that is, it can tell us how we ought to act in any given situation.

Before we can determine if Laudan's normative naturalism can preserve its normative function we need to know a little more about it. As we know, "normative naturalism is a *meta-epistemology* or, more narrowly, a meta-methodology."¹¹ It is comprised of two basic components: Axiology and methodology. Methodology for Laudan consists of rule based prescriptions, and turns on a linguistic analysis of general methodological rules. These methodological rules, under analysis, turn out to be most accurately characterized as imperatives. Moreover, they are hypothetical, rather than categorical, imperatives.. In other words, a methodological rule of the form 'one ought to do x' is best understood as 'if one's goal is y, then one ought to do x'.¹² Normative rules are thus construed as means/ends statements. Laudan's naturalism dictates, moreover, that these hypothetical imperatives be judged empirically, that is, tested in the same way that we would test any and all other empirical theories. Thus, the methodological half of Laudan's normative naturalism consists of prescriptive hypothetical rules, grounded in empirical, and hence defeasible, theories.¹³

¹¹ Larry Laudan, "Aim-Less Epistemology?" in Studies in the History and Philosophy of Science, 1990, Vol. 21, p.315.

¹² Laudan, 1987, p.24.

¹³ Laudan, 1990, p.46.

To get a better idea of how this is to work, take for instance the hypothetical imperative: 'If one's goal is y , one ought to do x '. The force of this methodological rule depends on our theories about x and y . If these theories tell us that x is the most effective way, i.e. in terms of probability maximization, to achieve y , then we ought to act on this particular methodological rule (to achieve y). Conversely, if we have no good reason to believe that x will bring about y , we have no good reason to act on the methodological rule in question. In other words, the methodological rule 'if one's goal is y , one ought to do x ' holds only if x is the best means of achieving y ; if, however, through empirical testing it is discovered that z and not x is the best means at getting y , then the valid methodological rule becomes 'if one's goal is y , [and z is the best means to y] then one ought to do z '. Thus, as long as we are somewhat clear on how to go about testing theory claims, we will have no difficulties testing rival methodologies.

In "Normative Naturalism" (1987) Laudan anticipates only one major difficulty with this account of methodological rules, and in concluding his proposal attempts to deal with it. It is important to look at this problem and Laudan's solution to it because it furnishes us with a more complete picture of Laudan's meta-methodology. The problem, as Laudan states, is

that we could 'test' a methodological rule only by taking for granting the prior establishment of some other methodological rule, which will tell us how to test the former. And that latter rule, in its turn, will presumably require for its justification some previously established rule, etc.¹⁴

His account of methodological rules, Laudan concludes, faces a vicious circle or an infinite regress. Laudan suggests the solution to this problem is to find a single rule that is shared by all otherwise disputing methodologies.

¹⁴ Laudan, 1987, p.25.

He offers one such rule (R1), which is intended to be simple enough to get agreement from all contending parties:

(R1) If actions of a particular sort, *m*, have consistently promoted certain cognitive ends, *e*, in the past, and rival actions, *n*, have failed to do so, then assume that future actions following the rule "if your aim is *e*, you ought to do *m*" are more likely to promote those ends than actions based on the rule "if your aim is *e*, you ought to do *n*".¹⁵

In support of (R1), Laudan's sole justificational recourse is to common sense: "If (R1) is not sound", Laudan claims, "no general rule is"¹⁶. Thus, Laudan concludes, there is no need to talk about intuitions (shared or not), or of the rationality or irrationality of past scientists. We simply apply (R1) to rival methodologies, and we can go easily about the business of testing methodological rules.

Rule (R1), as I said, provides the finishing touch to Laudan's account of methodological rules, and how those rules are supposed to work within his normative naturalism.¹⁷ The explication of (R1) concludes the exegesis on the methodological half of Laudan's normative naturalism. It is now time to introduce the main features of Laudan's axiology.

As I said, Laudan's axiology is central to his normative naturalism. Its importance has already been anticipated: we have just learned that methodological rules get their prescriptive force *only* with reference to cognitive aims. Yet, although "methodology gets nowhere without axiology",¹⁸ methodology is not in a position to evaluate cognitive aims, since it is concerned only with the assessment of means to ends.

¹⁵ Laudan, 1987, p.25.

¹⁶ Laudan, 1987, p.26.

¹⁷ This is the only really problematic feature of Laudan's methodology (that is, when viewed independently of his axiology). Potential solutions to charges of circularity faced by Laudan's account in general are examined in chapter three, pp.49-52 below.

¹⁸ Laudan, 1987, p.29.

Methodology needs to be supplemented "with an investigation into the legitimate or permissible ends of inquiry."¹⁹ This investigation into cognitive aims, or 'axiological inquiry', is the subject of Laudan's Science and Values.²⁰ There we discover the two main features of Laudan's axiology; one of those features is descriptive, the other, prescriptive.²¹ The descriptive element of Laudan's axiology is his view on aim change in the history of science; the prescriptive element is his reticulated model of scientific rationality, which tells us under what circumstances aim change is rational. I will sketch the descriptive component first.

In the following chapter the importance of Laudan's view on aim change in the history of science to his normative naturalism is explained; here I give but a brief description of that view. Aim variance is an aspect of Laudan's axiology which is straightforward and well-evidenced; basically, his view is that "the aims of science in particular and of inquiry in general have exhibited certain significant shifts through time."²² To justify this position, Laudan cites instances of (what he considers) *significant aim change* in the history of science. To get an idea of what Laudan means by 'significant aim change' we can look at a favorite example of his, namely the abandonment of 'infallible knowledge' as a cognitive aim for science.²³ Here it may be helpful to quote Laudan in full:

More or less from the time of Aristotle onward, scientists had sought theories that were demonstrable and apodictically certain. Although empiricists and rationalists disagreed about precisely how to certify knowledge as certain and incorrigible, all agreed that science was aiming exclusively at the production of such knowledge. This same view of science

¹⁹ Laudan, 1987, p.28.

²⁰ Larry Laudan, Science and Values (University of California Press), 1984.

²¹ As we will see in chapter three, there is a descriptive component to what I am here identifying as solely prescriptive .

²² Laudan, 1990, p.48.

²³ Laudan, 1984, p.83.

largely prevailed at the beginning of the nineteenth century. But by the end of that century this demonstrative and infallibilist ideal was well and truly dead. Scientists of almost every persuasion were insistent that science could, at most, aspire to the status of highly probable knowledge. Certainty, incorrigibility, and indefeasibility ceased to figure among the central aims of most twentieth-century scientists.²⁴

According to Laudan, examples like this one, where one cognitive aim has clearly been replaced by another, are abundant in the history of science;²⁵ and, they provide the necessary support for his position "that the predominant goals of the scientific community have changed through time, often in deep and significant respects."²⁶

This descriptive component of Laudan's axiology is supplemented by a prescriptive component, namely an account of the rational evaluation of aims. This notion is captured in what Laudan calls the reticulated model of scientific rationality (or justification). This prescriptive element of Laudan's axiology is not quite as straightforward as his view on aim change in the history of science, and in order to get a good understanding of it we need to first see how it is motivated.

Laudan's reticulated model of scientific rationality is motivated by a dissatisfaction with what he calls "the best-known contemporary solution to the problem of consensus formation in science",²⁷ i.e. the hierarchical model of justification. According to the hierarchical model, factual disagreements (e.g. in science) happen at the lowest level of the hierarchy, and are resolved by appeal to the next level up the ladder, i.e. methodological rules. Sometimes, however, scientists disagree over which

²⁴ Laudan, 1984, p.83.

²⁵ See also Laudan, 1990, p.49.

²⁶ Laudan, 1984, p.47.

²⁷ Laudan, 1984, p.23.

methodological rules to use, or how to apply them. When this happens, we move one more rung up the hierarchical ladder to the level of shared aims or goals. According to the proponents of the hierarchical model, this is how scientific consensus is forged.

Laudan claims that there are some fundamental difficulties with this model, and goes on to list a plethora of problems.²⁸ One main weakness is that it tells us that when we have disagreements over our methodological rules, we are to turn to our shared goals in order to find resolution. Yet, an appeal to common goals does not always resolve differences at the methodological level; as far as Laudan is concerned, "although the invocation of shared goals may sometimes make methodological consensus possible, it is crucial to stress that this is not a cure-all for all manner of methodological disagreements."²⁹ This problem with the hierarchical model is illustrated by two counter-examples. The first counter-example Laudan looks at is when two rules equally achieve the same goal; in this case, he asks, how do we adjudicate between rules? The hierarchical model does not provide an answer to this dilemma. To highlight the second kind of problem facing the hierarchical model, Laudan envisions a scenario where there are two, mutually exclusive goals; and, as one goal is being promoted, the other is being thwarted. Again, Laudan's concern is that this situation appears to be irresolvable, "irresolvable, that is, if we stick to the limited resources of the classical hierarchical model."³⁰ Yet, according to Laudan situations like these can be and in practice are resolvable.

The most fundamental problem with the hierarchical model,

²⁸ Laudan, 1984, pp.26-41.

²⁹ Laudan, 1984, p.37.

³⁰ Laudan, 1984, p.41.

however, is that it lacks the resources to evaluate cognitive aims. As Laudan claims, "there is a point where the model breaks down badly and repeatedly: specifically, when scientists disagree about (some of) their basic cognitive aims or goals."³¹ Since goals are the final court of appeal in the justificational ladder of the hierarchical model, there is no way (following that model) to resolve differences in the axiological commitments of scientists. Yet, as we already know, axiological differences do indeed exist. Moreover, that aims have changed throughout the history of science shows that aim differences have been (and can be) rationally evaluated. Thus, Laudan concludes that "the frequent closure of axiological disagreements in science demonstrates the urgency of supplementing that [hierarchical] model with other machinery."³²

The striking failure of the hierarchical model to account for rational aim change in the history of science is what motivates Laudan's reticulated model of scientific rationality. The reticulated model is a substantial improvement on the hierarchical model,³³ primarily because it *can* account for the rational evaluation of aims. Unlike the hierarchical model which has justification running unidirectionally, with the reticulated model justification flows both upward and downward in the hierarchy.³⁴ According to Laudan, this allows for a more sophisticated and "complex process of mutual adjustment and mutual justification going on among all three levels of scientific commitment."³⁵ No longer is any one of these

³¹ Laudan, 1984, p.42.

³² Laudan, 1984, p.43.

³³ I should note that in (1984) Laudan's critique of the hierarchical model is more than partly motivated by his critique of an even bigger picture, i.e. Kuhn's account of non-rational aim change.

³⁴ See Laudan, 1984, p.63 for a helpful diagram of the reticulated model.

³⁵ Laudan, 1984, p.62.

levels more privileged than the others. Goals or aims are no longer construed as inflexible, nor are they the final court of appeal. Aims are informed by theories and methods, just as theories and methods are informed by aims. Importantly, this reticulation between goals, methods and theories is how we get a rational evaluation of aims. Specifically, aims are evaluated on the basis of information supplied by theories and methods, following two general modes of criticism: The utopianism, or unrealizability of aims, and the discordance between explicit and implicit aims. Of these two standards of evaluation, the latter is awarded far less weight throughout the bulk of Laudan's writings; I will examine it first.

According to Laudan, one may argue against a goal on the grounds that "it fails to accord with the values implicit in the communal practices and judgements we endorse."³⁶ The kind of situation Laudan has in mind is a fairly common one; often, there appears to be a tension between the aims an agent explicitly endorses and those which seem³⁷ implicit in her actions. Laudan claims that in cases like this the rational person, "on pain of being charged with inconsistency,"³⁸ should attempt to bring the two in line with each other; "whenever a case can be made that a group of scientists is not practising what it preaches, there are prima facie grounds for a change of either explicit or implicit values."³⁹ Again, the main justification for this mode of critiquing cognitive ends is a consistency criterion, though Laudan also mentions that the rational person would want to avoid hypocrisy, dishonesty, and desire to overcome a state of

³⁶ Laudan, 1984, p.50.

³⁷ Laudan is not unaware that there are problems with attributing goals to people based on their actions, but he is convinced that they can be averted by simply asking agents themselves to report on their own sets of implicit values.

³⁸ Laudan, 1984, p.55.

³⁹ Laudan, 1984, p.55.

disequilibrium.⁴⁰ This change in cognitive ends may happen over a long or short period of time, and it may include the abandonment of either the implicit or explicit aim, or both. To elucidate how this critique of aims is supposed to work (and lend further support to this view), Laudan provides a number of historical examples;⁴¹ he summarizes one significant historical case involving the abandonment of 'observability of entities' as a cognitive goal in these brief comments:

The chief source of this shift in the explicit attitudes of philosophers and scientists toward the legitimacy of postulating unseen entities was a prior shift in the character of physical theory itself. Specifically, by the 1830's scientists found themselves working with theories that, as they eventually discovered, violated their own explicit characterizations of the aims of theorizing. Confronted by that discovery, they eventually reappraised their explicit axiology.⁴²

This case nicely illustrates how the process of harmonizing implicit with explicit aims will naturally reject one (or more) inconsistent set of aims. I will now look at the other, more prominent tool for evaluating cognitive aims which Laudan puts forth.

Utopianism, or realizability, is what Laudan proposes as the main criterion for the rational evaluation of aims.⁴³ In general, an aim is 'utopian' if there is no conceivable way for that aim to be actualized. Our knowledge of the world (i.e. our theories) and of available methods of inquiry tell us when an aim is unrealizable. And, Laudan claims, 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

⁴⁰ Laudan, 1984, p.55.

⁴¹ See Laudan, 1984, pp.56-62.

⁴² Laudan, 1984, p.56.

⁴³ It is the realizability criterion that Laudan continues to rely on in (1987,1987(b), 1990, and 1990 (b)).

espoused is *in principle unrealizable*, then continuing to hold that goal makes a nonsense of the notion of rational action.”⁴⁴

In (1984) Laudan identifies three different sorts of ‘utopian strategies’, (although in his later writings these distinctions are subordinated to ‘realizability in general’.) The first utopian strategy is demonstrable utopianism; a goal is demonstrably utopian if it cannot be achieved, given our understanding of logic and the laws of nature. The second is semantic utopianism; a goal is semantically utopian if it cannot be characterized in a succinct and cogent way. The third utopian strategy is epistemic utopianism; a goal is epistemically utopian if the criteria for determining its achievability are unclear.⁴⁵ Although the lines between these three strategies are sometimes blurred, together, according to Laudan, they combine to explain a majority of cases of the rational abandonment of goals throughout the history of science; he states: “it is the adjudication of such criticism and the responses it produces which have led to the revision of some of our once highly cherished cognitive ambitions for science.”⁴⁶

Again Laudan relies on historical examples to lend further support for his view, and to help to illustrate how realizability functions as a tool for evaluating aims.⁴⁷ To see how realizability is supposed to work, take the example (on pages 7-8) above, regarding the abandonment of ‘infallible knowledge’ as a cognitive aim in science. The abandonment of this cognitive aim can be best explained by epistemic utopianism. Eventually, the story goes, scientists concluded that there was no obvious, agreed upon method for demonstrating the infallibility of knowledge claims (even if

⁴⁴ Larry Laudan, “Relativism, Naturalism and Reticulation”, in *Synthese* Vol.71, 1987(b), p.227.

⁴⁵ See Laudan, 1984, pp.51-53 for examples of each utopianism strategy.

⁴⁶ Laudan, 1984, p.53.

⁴⁷ See Laudan, 1984, pp.51-53, and pp.82-87.

theories at the time suggested such knowledge existed); in other words, the criteria for determining infallibility were utterly unclear. Thus, infallibility came to be seen as an unrealizable cognitive goal of science, and consequently was replaced by the (believed to be) realizable goal of 'highly probable' knowledge.

The complex nature of the reticulated model of scientific rationality should now be evident: With this model, justification runs in all directions; aims inform theories and methods just as methods and theories inform aims. Finally, and importantly, in virtue of this reticulation Laudan's axiology exhibits the empirical nature of meta-methodology.⁴⁸ Hence in Laudan's meta-methodology, axiology, as well as methodology, is naturalistic.

The exegetical part of this thesis is now completed. We have a basic idea of what, broadly speaking, constitutes a naturalist epistemology. More narrowly, we examined in relative detail a contemporary naturalist epistemology, namely Laudan's normative naturalism, and its two main components: Methodology and axiology. We are now sufficiently equipped to assess Laudan's normative naturalism; this evaluation will occupy the following four chapters.

⁴⁸ This claim is elaborated and defended in chapter three.

Chapter Two

As we have seen, Laudan claims that the aims of science have changed through history. This change is important for two reasons. First, aim variance provides Laudan with a response to what he calls "the most influential argument in recent years against the methodological enterprise."¹ This argument, namely the 'argument from historicism', is in Laudan's mind the single largest threat to his normative naturalism. I will examine this argument, as Laudan characterizes it, and then show how aim variance furnishes him with a reply to it.

Second, aim variance in the history of science motivates an objection of Laudan's view which is even stronger than 'argument from historicism'. This objection is known as 'the problem of relativization', and it is just this: If (as some historicists and Laudan have argued) the basic aims of science change over time, and, all formulation of methodological recommendations are relativized to contexts within which cognitive goals are shared, then we cannot escape the complete relativization of epistemology. Laudan fails to address the problem of relativization. However, two of his (like-minded naturalist) critics, Alexander Rosenberg and Jarrett Leplin, in their efforts to preserve a normative naturalism offer some ways around the problem.² After I look at their solutions I will offer an alternative route available to Laudan, one which both overcomes the problem of relativization and leaves intact his axiological commitment to aim change in the history of science.

To begin with, then, I will look at Laudan's characterization of the

¹ Laudan, 1987, p. 19

² Jarrett Leplin "Renormalizing Epistemology", and Alexander Rosenberg "Normative Naturalism and the Role of Philosophy", both offer responses to Laudan's normative naturalism in Philosophy of Science, March, 1990, Volume 57, p.20-33 and p.34-43, respectively.

'argument from historicism', and his response to it. According to Laudan, the historicists, among whom he includes Kuhn, Feyerabend, and the early Laudan, argue that our contemporary philosophical notions of scientific rationality, as manifest in familiar methodologies of science, fail to capture the rationality of some of the greatest achievements in the history of science. The historicists also presuppose what Laudan calls the rationality thesis (RT), which states that:

most great scientists have made their theory choices rationally³

Additionally, the historicists hold the meta-methodology thesis (MMT), which states that:

a methodology of science is to be evaluated in terms of its ability to replicate the choices of past scientists as rational⁴

These three suppositions: The RT, the MMT and the apparent failure of today's methodologies to capture past acts as rational, is what, in Laudan's mind, leads the historicists to the conclusion that today's methodologies are inadequate.

Laudan claims that this argument fails, and the reason for this is because the historicists fail to recognize an important implication regarding aim change in the history of science. To expose the historicists' *wrong turn*, Laudan singles out the historicists' insistence on the MMT thesis, a thesis which he claims is seriously flawed. Why should we suppose, he wonders, that today's methodological rules could capture past acts as rational? The historicists, it seems, have overlooked something very important: namely, that "*both the aims and the background beliefs of scientists vary from agent to agent, and that this is particularly so when one is talking*

³ Laudan, 1987, p.20.

⁴ Laudan, 1987, p.21.

about scientific epochs very different from our own."⁵ Thus, today's methodological rules, geared as they are to our aims and propelled by our sets of beliefs, cannot entail anything about the rationality/irrationality of other agents. The MMT is a bogus restriction on today's methodologies; "rationality and methodology", Laudan concludes, "need to be sharply distinguished".⁶

As I said at the start of this chapter, this argument is not, as Laudan claims, a serious threat to his normative naturalism. In fact, it is a bizarre point of objection with the historicists. It is strange that Laudan would criticize the historicists for their (so construed) backwards penchant of judging the acts and decisions of past scientists by the yardstick of today's methodological standards. It is simply false that the historicists, particularly Kuhn or Feyerabend, would have disagreed with Laudan's clarification that *both* aims and beliefs change over time. Furthermore, there is certainly enough evidence to think that Feyerabend would not only agree with Laudan's claim, but in fact held a similar view.⁷ What's more, the historicists themselves have, time and again, filed this very same complaint against *a priori* epistemologists.⁸

Not only is his charge against the historicists misguided, but what Laudan has dubbed the 'main threat coming from historicism' is not even an accurate characterization of historicism because Laudan confuses the meaning of the term 'historicism'. He seems to (at least ostensibly) define historicism in two ways. First, as the conjunction of the RT and the MMT, and second, as personified by Kuhn, Lakatos, Feyerabend, and the early

⁵ Laudan, 1987, p.20.

⁶ Laudan, 1987, p.21.

⁷ Paul Feyerabend Against Method, (New Left Books), 1975, see especially chapter 11.

⁸ Kitcher, 1992, p.68, fn.43.

Laudan. However, it is not obvious that, for example, Kuhn or Feyerabend would subscribe to the RT and/or the MMT. In what follows I look at the real challenge that historicism poses to normative methodology. In that discussion the term 'historicism' will refer the view held by *some* members of the historicist camp, notably Feyerabend and Kuhn, that the aims of science have changed over time.

The serious problem historicism poses to Laudan's normative naturalism, which he never directly addresses, is the relativization problem.⁹ The relativization problem is motivated by the historicist view, *shared by Laudan*, that the aims of science have changed over time. Recall, the problem is just this: If the basic aims of science change over time, and our methodological rules are contingent on cognitive aims, then how can we avoid the thorough going relativization of epistemology? The worry here is that any formulation of methodological rules must be relativized to some context within which cognitive goals are shared. And, since goals vary, those contexts will not be universal; hence, the prospect of a universal normative epistemology seem dim.¹⁰ This problem is unique to the epistemic naturalist, since the naturalist claims that methodology is to be extracted from the history of science. Hence, this is a challenge to which the naturalist must respond. Yet, Laudan not only ignores it, but because he espouses an axiology of goal variance,¹¹ he walks right into it.

The problem of relativization is one which is often framed by radical

⁹ at least, he never addresses it directly in 1987. He does mention it in 1990 (p. 47), but, again, fails to really address the problem.

¹⁰ normativity would be, in Feyerabend's words, just 'empty moralizing'.

¹¹ "the historicists are right that the aims (and methods) of science have changed through time, although some of their claims about how these changes occur (especially Kuhn's) are wide of the mark." Laudan, 1990, p.47. Laudan is vague about who he includes in the historicist camp; certainly *not all* historicists, e.g. Lakatos, hold this view of cognitive aims.

naturalists, such as Quine,¹² who see the collapse of *a priori* epistemology as the end of normativity. Less radical naturalists, like Laudan, Rosenberg and Leplin claim that we can deny apriorism and still retain the notion of universal goals in science, only those goals will not be justified *a priori*, rather they will be justified empirically. This is where the naturalist and the historicist meet: Naturalists "extract methodology from the actual record of how progress has been achieved."¹³ This may get us some distance towards a solution to the problem of relativization; naturalism, its advocates maintain, has the resources to determine the goals of science empirically. Thus, the denial of apriorism does not necessarily entail the end of normativity. However, the historical turn can be self-destructive. It informs us that there is no single universal goal of science. If the historicist view is right, then the naturalist may be forced to relativize methodological rules to particular contexts where cognitive aims are shared, and therefore abandon the prospect of a universal normative epistemology.

Leplin sees an offshoot of the problem of relativization as the greatest challenge to Laudan's position. According to Leplin, Laudan's normative naturalism "is inductive in that it extrapolates the methods it endorses from the record of how particular measures have fared at advancing particular ends".¹⁴ It is this inductive aspect of Laudan's meta-methodology that Leplin thinks is problematic. The way induction works in Laudan's account, as Leplin describes it, is like this: We see how well, for example, *x* has fared in the past at getting us to goal *y*; if it has fared better than any alternatives then in the future, according to Laudan's

¹²See especially W.V.O. Quine, "Epistemology Naturalized", in Ontological Relativity and Other Essays, (New York: Columbia) 1969, p.69-90.

¹³ Leplin, 1990, p.21.

¹⁴ Leplin, 1990, p.22.

normative naturalism, we ought to choose x if our goal is y . But, Leplin's worry is that if aims are frequently shifting, if y is not a constant goal of science, then we cannot "get normative methodology from history, for there would be no suitable material to induce from."¹⁵ What, Leplin wonders, are we to do if our goals shift from, for example, y to z ? His answer is that we will not know what to do; that in order for methodological rules to have any prescriptive force, i.e. for them to be in any way instructive, they will have to be relativized to some context where there is a shared goal.

Leplin, in his own effort to preserve a normative naturalism, tries to avert the problem by simply denying axiological change. He claims that "modern science - physical science from Galileo on, say - exhibits general, sustained methodological and axiological themes that survive changes in the localized prescriptions and constraints that scientific discoveries introduce."¹⁶ This passage indicates Leplin's belief that there have been aim changes and method changes in the history of science. But what is important is that, according to Leplin, these are not fundamental changes. The following comments by Leplin suggest what he thinks are the constant axiological and methodological themes throughout the history of science:

But through it all, science continues to be mathematical and, where possible experimental. It continues to seek truth and generality. It continues to count empirical adequacy as a criterion of truthlikeness, and deductive systematization as a criterion of generality. It continues to demand testability of its hypotheses. Knowledge in one form or another remains its overriding objective.¹⁷

This solution to the problem of relativization is not uncommon among

¹⁵ Leplin, 1990, p.23.

¹⁶ Leplin, 1990, p.24.

¹⁷ Leplin, 1990, p.23.

naturalists wishing to secure the normative project. Rosenberg's response, although directed at a general construction of the problem of relativization and not Leplin's particular construal of it, resembles Leplin's. Like Leplin, Rosenberg thinks that the best way to avoid the relativization of methodological rules is to argue that there is only one single universal goal of science. To establish this view Rosenberg makes a distinction between the goals of scientists and the goals of science. The historicists and Laudan are wrong, he claims, because they fail to notice this distinction. Consequently, they confuse the various goals of particular scientists with the single goal of science. To ground normativity, that is, to obtain prescriptive force from our methodological rules, we cannot look at something as weak as particular scientists whims and transitory interests. Instead, normativity "must derive from some goals constitutive of science";¹⁸ the likely candidate for the 'goal constitutive of science' cited by Rosenberg is (unsurprisingly) 'knowledge'. Certainly, there seem to be have been a lot of different goals in the history of science. But these, he claims, are best seen as instrumental goals. And the reason why these instrumental goals differ, Rosenberg explains, and why we experience shifts in method, is due to the background beliefs of scientists which have themselves shifted over time, suggesting different ways of achieving 'knowledge.'

As we know, Rosenberg's solution to the problem of relativization is not unlike Leplin's (though alternately premised). Namely, to deny that there has been more than one substantial aim, i.e. knowledge, in the history of science. In other words, Leplin and Rosenberg charge that the historicists and Laudan have their history wrong. Whether Leplin and

¹⁸ Rosenberg, 1990, p.36.

Rosenberg have *their* history right will evidently determine how successful their response is to the problem of relativization; but it is not obvious how to decide whether or not they are right. For instance, there may be certain identifiable aim changes in the history of science that Laudan, Leplin and Rosenberg each agree on.¹⁹ Yet, Laudan will say that axiological change is significant, expressing a real change in aims in the history of science. On the other hand, Leplin will say of that same aim change that it is *insignificant* axiological change; and Rosenberg will say that very same change reflects not a change in the aims of science, but a change in the interests of particular scientists. To a certain degree, as Leplin claims, "the issues here are semantic: what one counts a change of aims, another counts a change of method; and another, a change of substantive, empirical belief."²⁰ But the issue here is *not entirely* semantic. According to Laudan, the aims of science (however identified) have changed significantly through time, and it is a great injustice to history to classify real shifts in method and goals as merely instrumental, and hence insubstantial; he states that "the terse formula 'science aspires to knowledge' disguises a plethora of fundamentally disparate notions."²¹ And some historicists, notably Kuhn and Feyerabend, agree with Laudan. Feyerabend argues, moreover, that these fundamental shifts in goal and method have been absolutely necessary for scientific progress.²²

As I said, it is not clear how to decide who has the more accurate characterization of axiological change in the history of science. What *is* obvious, however, is that if Laudan is committed to this view on aim

¹⁹ For example, the change from 'infallible' knowledge to 'highly probable' knowledge as a cognitive aim of science

²⁰ Leplin, 1990, p.28.

²¹ Laudan, 1990, p.49.

²² Feyerabend, 1975, p. 14.

change, then the Leplin/Rosenberg solution to the problem of relativization is not available to him; he must provide an alternative route around the problem. For the time being, let us agree with Laudan's descriptive view that the aims of science have changed through history, so that we can examine the alternatives available to him. Specifically, we need to find out if there is a way to sidestep the problem of relativization without denying aim variance in the history of science.

It may be helpful at this stage to briefly recap the problem of relativization. Aim change is supposed to be a problem for the historicist view (shared by Laudan), if we suppose that fundamental, basic aims of science have changed over time, and that methodological recommendations are hypothetical to and contingent on some particular goal. So, for example, the rule "if one's goal is y , then one ought to do x ", is specific to contexts where, for example, x is a shared cognitive goal. Again, the problem is that since the contexts where goals are shared is always changing, methodological rules will have to be relativized to those particular contexts. The conclusion here is that the naturalist is compelled to abandon the prospect of a universal normative epistemology.

This is a substantial obstacle facing Laudan's normative naturalism. That said, it may not be as hard to overcome as initially anticipated. To start with, two things need to be made clear. First, although particular methodological rules are contingent on particular cognitive aims, the form of each of those rules is the same. Every methodological rule in Laudan's normative naturalism is an instantiation of the general methodological rule of instrumental rationality, which states: Whatever your end, act on whatever means will best promote the attainment of that end. *This* methodological rule is unaffected by Laudan's axiological commitment to

the changing goals of science. Second, a meta-methodology, if it is to be successfully normative, needs to "specify those strategies which promote attainment of cognitive goals"²³. And, as should now be clear, Laudan's normative naturalism does exactly that. Leplin's worry, that if, for example, *y* is a new goal, we won't know which "*x* will best promote *y*", only means that when we encounter new goals we will have to test alternative methods. Yet this is *exactly* what Laudan wants from a methodology: That methods should be assessed and rejected, in the same way as are scientific theories. True, Laudan's proposal does not tell us what to do in any situation, but even if we were to accept the Leplin/Rosenberg axiology, we would still have to know how to go about achieving particular (instrumental) goals. And to expect a methodology to provide recommendations for every single cognitive aim is absurd.

Although the problem of relativization is not identical to the problem that Leplin raises, they are both countered in the same way. Particular epistemic recommendations will be relativized to particular goals, certainly. But, again, this fact in no way threatens a universal normative methodology which states: Whatever ones goal's are, one ought to chose the course of action which will best promote the attainment of those goals.

As I have characterized it so far, Laudan's normative naturalism is, essentially, a version of instrumental rationality.²⁴ As Gerald Doppelt notes, "[Laudan's] naturalism is really embedded within a philosophical paradigm of scientific rationality as means-ends or instrumental rationality."²⁵ And as

²³ Kitcher, 1992, p.79.

²⁴ The fact that Laudan's normative naturalism looks strikingly similar to a programme for instrumental rationality - in spite of all of his attempts to divorce methodology from rationality - is an objection which is raised and nicely dealt with by Gerald Doppelt in "The Naturalist Conception of Methodological Standards in Science: A Critique", in Philosophy of Science, March, 1990, Volume 57. esp. pp.5-10.

²⁵ Doppelt, 1990, p. 7.

such it is a fairly good one. It is able to instruct us, once we have our goals, how we should go about achieving them. Moreover, it can be used for judging the rationality of past actions. All we need to know is what the goals at the time were, and then we can determine if the actions undertaken were, at the time, the most likely to fulfill those goals.²⁶ Whether Laudan's normative naturalism is an appealing meta-methodology all depends, of course, on what we require of a theory of rationality in the philosophy of science. If what we demand is a universal normative epistemology that enables us to plan our own actions and judge the actions of others, then Laudan's normative naturalism looks like a successful candidate for an epistemology of science.

But we have only had a first glimpse of Laudan's meta-methodology. We need to further examine some key aspects of Laudan's normative naturalism. In particular, we need to look deeper into Laudan's axiology. In the next chapter I do just that, and attempt to determine to what extent it is, indeed, 'naturalistic'.

²⁶ "Lavoisier's rationality [can] be assessed only by determining whether his actions further his own ends", Laudan, 1987, p.23. Laudan simply fails, however hard he tries, to detach his methodology from the rationality of past actions. As Doppelt points out, "The instrumentalist paradigm of rationality presupposed by his views can scarcely be time-bound, limited in validity to present scientific choice." Doppelt, 1990, p. 7.

Chapter Three

I began the last chapter by claiming that Laudan's axiology posed a problem for his normative naturalism. The worry I dispelled was that Laudan's view on aim change in the history of science led to the problem of relativization. In this chapter axiology is again the focus, although this time the challenge raised may present an insuperable obstacle for Laudan's normative naturalism. The present criticism is due primarily to Gerald Doppelt¹; Doppelt's worry is that Laudan's account of the rational evaluation of aims, namely the reticulated model of scientific rationality, does not have a *naturalistic* foundation. This is a serious criticism which has the potential to render untenable any naturalistic version of instrumental rationality.

Because this criticism is aimed at Laudan's prescriptive axiology, I want to preface it by examining the position that axiology holds within normative naturalism. To determine this I explore a number of interpretations of a notorious claim Laudan made in his seminal paper (1987) on normative naturalism, a claim which has brought him some attention; namely, that "methodology gets nowhere without axiology".² Once the role axiology is supposed to hold within Laudan's meta-methodology is confirmed, I turn to Doppelt's criticism. I will outline in detail Doppelt's argument; then, I will show how Laudan *could* respond to Doppelt, and offer evidence which suggests that is how Laudan *would* respond to the criticism. Finally, at the end of the chapter, I discuss two criticisms that are motivated through the course of this chapter.

¹ This argument is referred to in Doppelt, 1990, though initially elaborated in Gerald Doppelt "Relativism and the Reticulational Model of Scientific Rationality", in Synthese Vol.69, 1986, pp.225-252.

² Laudan, 1987, p.29.

"Methodology gets nowhere without axiology". How is this statement to be interpreted? We have examined in detail Laudan's position on aim change in the history of science, and we have looked closely at his reticulated model of scientific rationality. We now need to determine exactly what place axiology is supposed to inhabit in Laudan's meta-methodology; that is, within the scope of Laudan's normative naturalism, how contingent is methodology on axiology?

In (1987) Laudan claimed that an epistemology or philosophy of science is crudely immature if it is unable to "certify or de-certify certain proposed aims as legitimate,"³ and that "we thus need to supplement methodology with an investigation into the legitimate or permissible ends of inquiry."⁴ Laudan is explicit about his belief that axiology is the underdeveloped aspect of meta-methodology, "whose centrality is belied by its crude state of development."⁵ Unfortunately, these comments are unhelpful, since they leave open the question whether axiology (underdeveloped or not) has any necessary, or even substantial bearing on meta-methodology. Certainly, they do not pick out any constraints that must be imposed on the methodology side of our meta-methodology while axiology is under construction. In fact, these comments leave one with the impression that Laudan thinks axiology must be 'cleaned up' for aesthetic reasons, or so his normative naturalism presents as a mature and complete meta-methodology.

On the above view, however, axiology can hardly be said to play the integral role Laudan would have it in a meta-methodology. Surely, then, Laudan had something more serious in mind when he claimed

³ Laudan, 1987, p.29.

⁴ Laudan, 1987, p.29.

⁵ Laudan, 1987, p.29.

"methodology gets nowhere without axiology". Recall, Laudan's thesis is that all methodological rules are disguised hypothetical imperatives, and hypothetical imperatives demand axiological context. As Laudan claims, methodological rules are "hypothetical imperatives whose antecedent is a statement about aims or goals, and whose consequent is the elliptical expression of the mandated action."⁶ We have already seen how this is supposed to work; Laudan's methodological dictum has it that the rule which states 'one ought to do x' is read as 'if one wants y, and x is the best means to getting y, then one ought do x'. Notice, without the putative goal (in this case 'y') the methodological rule gets nowhere, it is inoperative. Fundamentally, methodological rules need goals in order to be guidance providing, i.e. normative.

Thus, in the first sense outlined above, methodology needs axiology in order to present as a proper or complete epistemology of science (i.e. meta-methodology); in the second interpretation (and more plausibly credited to Laudan), methodology needs axiology necessarily, that is, methodological rules need (some) goals. But observe, even following this interpretation, we do not need to evaluate or chose between goals, we just need to have them. As long as we have goals - a relatively unproblematic requisite - our methodological rules can operate.

Again, even though the second interpretation is relatively undemanding (and hence appealing), it seems that it is also not what Laudan could have meant by "methodology gets nowhere without axiology"; at least not on pain of consistency, since he concurrently held the view (as captured by his reticulated model of justification) that aims do indeed demand evaluation.

⁶ Laudan, 1987, p.24.

There is another interpretation of 'methodology gets nowhere without axiology' which is certainly the most compelling. This interpretation, although not espoused by Laudan himself (indeed, as it presents a serious problem for his normative naturalism), is motivated by Laudan's views on the rational evaluation of change. Recall, the reticulated model of scientific rationality dictates the conditions under which a change in aims is rational. And, it tells us that not all aims, specifically those that are unrealizable,⁷ are justified. Methodological rules, it seems, do not just need goals in order to have normative force, they need *justified goals*. Hence, the third and most accurate reading of "methodology gets nowhere without axiology" is "methodology gets nowhere without *justified aims*".

We now have a better understanding of the central role axiology occupies in Laudan's normative naturalism, and how utterly and critically contingent on it are methodological rules. The stage is now set for Doppelt's main criticism. Doppelt argues that Laudan fails to provide a *naturalistic* account of aim justification, i.e. that Laudan's reticulated model of scientific rationality is non-naturalistic. Doppelt goes on to argue that, given the centrality of axiology to methodology, without an adequately *naturalistic* axiology, a thoroughly naturalistic meta-methodology is impossible. Hence, Doppelt concludes, Laudan's normative naturalism is not naturalistic, after all. I will now elaborate on this objection.

To start with, I want to examine Doppelt's main objection, namely that Laudan fails to provide an adequately *naturalistic* account of aim change. According to Doppelt, if Laudan cannot provide a naturalistic

⁷ Although a second criterion for unjustified aims was outlined above (p.16), since realizability is the main criterion, and the one Doppelt's criticism is aimed at, I rely on it solely. Moreover, although there are different strands of realizability outlined above (p.18), hereafter I refer to its general instantiation.

account of justified aim change, then his meta-methodology is confined to "contexts where basic cognitive values and standards are taken to be fixed and unproblematic."⁸ Of course, this would be a huge problem for Laudan, since (as we know) Laudan holds that there are no contexts where basic cognitive values are fixed.

The objection that Laudan fails to provide a naturalistic account of aim change may at first seem somewhat odd, since we have already reviewed an account of justified aim change, dubbed by Laudan as naturalistic. Recall, Laudan proposes the realizability of aims as the main criterion for rational aim choice, and he insists it is a naturalistic measure; referring to the realizability criterion, Laudan states "I have described such an axiology, or at least parts of it, and that too has a strong empirical or naturalistic component."⁹ Doppelt simply disagrees; he states that its prominence (or not) as a criterion for rational aim change aside, realizability is simply not a *naturalistic* criterion. Doppelt argues that, essentially, realizability has no more a naturalistic foundation than, say, internal consistency with our theory preferences, or any other super-empirical (i.e. conceptual) criterion. Doppelt admits it may be true that our theories can tell us - i.e. it is an empirical matter - that this goal x or that goal y is unrealizable; however, he argues, it is certainly not an empirical matter that we should not strive for unrealizable goals. As Harvey Siegel has aptly put it, "That an aim is utopian (e.g.) may be established naturalistically; that a utopian aim ought not to be pursued is not."¹⁰ This is why Doppelt claims that Laudan's proposal is "a far cry from the straightforward naturalist

⁸ Doppelt, 1990, p.4.

⁹Laudan 1987(b), p.232.

¹⁰ Harvey Siegel, "Laudan's Normative Naturalism" in Studies in the History and Philosophy of Science Vol.21, No.2, p. 311.

method of appealing to empirical evidence in order to determine whether the means pursued are conducive to the particular ends embraced.”¹¹

And, because Laudan’s realizability criterion fails to do what he needs it to, aim choice is left without what it so desperately needs, i.e. rational evaluation.

Once Doppelt has outlined this argument, he raises a further criticism. Doppelt argues that Laudan’s methodological naturalism is neither compelling nor interesting because it rests on a non-empirical axiological foundation. And, if indeed his naturalistic methodology is contingent on a non-naturalistic account of aims, one is left wondering what is the motivation to turn to naturalism at all? If, as Doppelt argues, there is some non-naturalistic warrant for settling aims, then there may be some non-naturalistic warrant for deciding methodological norms as well.¹²

Laudan gets a golden opportunity to respond directly to Doppelt’s criticism in (1990); unfortunately, he blatantly sidesteps the criticism, and instead accuses Doppelt of suggesting that his intention was to put forward a non-naturalistic axiology.¹³ Plainly, Doppelt is instead claiming that Laudan’s account just is (intentionally or not) non-empirical; that he has not yet offered a sufficiently naturalistic analysis of aims. Hand wringing aside, has Doppelt in fact convinced us of this?

First, recall that Laudan’s meta-methodology requires the rational evaluation of aims, and that ‘methodology gets nowhere without *justified aims*’. According to Doppelt, though the actual picking through aims to determine which ones are realizable is an empirical process, the criteria we employ which places value on certain empirical characteristics and not

¹¹ Doppelt, 1990, p.5.

¹² Doppelt, 1990, p.5.

¹³ Laudan, 1990, p.51.

others is super-empirical (i.e. conceptual). To get an even clearer picture of this point, take, for example, a case like this: Imagine that we bounced back and forth between theories and aims, following the reticulated model of scientific rationality, but instead of relying on our theories to determine which ends are realizable, we instead ask them which ends are happiness-inducing. This example should help to illustrate Doppelt's point that while a super-empirical criterion may successfully pick between aims, there is no empirical basis underlying this choice; realizability has no more empirical impetus than, say, happiness-inducing. Hence, Doppelt concludes, there is no naturalistic warrant for the realizability criterion. And, since Laudan's naturalism needs to be able to rationally evaluate aims, urgently as a matter of fact, as "Laudan's conception of scientific rationality really stands or falls on the basis of its account of rational aim choice,"¹⁴ his normative naturalism simply fails.

Before going on, it is important to define the sense of 'naturalistic' that Doppelt is here working with. There are two commonly held (though rarely clearly distinguished) definitions of 'naturalism' operative in contemporary philosophy of science. The more traditional one is: [naturalism-one] Something is naturalistic if and only if science does it. Almost as common a definition is: [naturalism-two] Something is naturalistic if and only if it has quality P, where P equals empirical testability (or something closely related to it). Looking at the evidence Doppelt gives to support his position, it is obvious that he is working with naturalism-two. Realizability, he claims, is non-naturalistic because it has no direct empirical foundation, and *not* because science does not use it. The strength of Laudan's position, as we shall soon see, is that his axiology is naturalistic in

¹⁴ Doppelt, 1986, p.234.

both senses defined above.

Back now to Doppelt's criticism. Since his second criticism is an elaboration on the first, we can start by returning to our original question: Has Doppelt convinced us that Laudan's account of justified aim choice is non-naturalistic? Well, although (in his defence) littered throughout his writings is Laudan's persistent claim that realizability is a naturalistic criterion, nowhere in those writings is there an accompanying argument in support of this claim. There are, however, bits and pieces from which it is possible to construct an argument on Laudan's behalf. Shortly, I will attempt this; for now, however, Doppelt has certainly shown the necessity of such an argument for a naturalistic justification of aims.

In order to see how Laudan *could* respond to Doppelt's objection, we need to first sketch a preliminary answer to this question: What would a naturalistic account of justified aim change look like? In other words, what are the general conditions under which an axiology would be considered naturalistic? The answer to this lies within the two definitions of naturalism, which I will now elaborate. Naturalism-one states: Something is naturalistic if and only if science does it. Why? To make sense of this definition we need to take a couple of steps back and review the main tenets of epistemic naturalism which were outlined in chapter one. The motivation for the 'naturalistic turn' in the philosophy of science (in the first place) was the success of the natural sciences.¹⁵ Naturalistic philosophers surmised that any philosophy of science had to look at and copy the workings of the natural sciences; naturalism is *about* turning to science. Thus, all versions of epistemic naturalism look to the natural sciences for answers to epistemic questions, and dictate that we should adjudicate knowledge

¹⁵ And a dissatisfaction with traditional philosophies of science (i.e. logical empiricist and logical positivist), that - a la Kuhn - didn't seem to fit with the history of science.

claims the same way we would claims in science.¹⁶ Epistemic naturalism, so construed, necessitates reflection on the ways in which quintessential seekers of knowledge, i.e. scientists, have actually gone about their business; plainly, the decree of naturalism is that we (as epistemologists, as philosophers of science) copy science. A naturalistic justification of aims, for the epistemologist, amounts to an imitation of a scientific justification of aims; the burden on the epistemic naturalist is to accurately employ (in her philosophy of science) whatever criteria are found to influence the abandonment or adoptment of aims in science proper. In other words, following naturalism-one: An axiology is naturalistic-one if and only if that axiology is prevalent in science.

The path we will take to better understand naturalism-two is now well-paved: Science, we have said, is the epitome of naturalism. Why? Well, (to greatly simplify things) predominantly this is because in science empirical testability reigns supreme. Thus, naturalism-two states: Something is naturalistic if and only if it has quality P, where P equals empirical testability. Empirical testability, to the naturalist philosopher of science, can be accurately construed as the empirical process undertaken to determine what sort of constraints scientists place on aims. This process is not one that the epistemologist could pursue *a priori*; it is thoroughly empirical, i.e. one has to go out and check what are the most popular methodological constraint on aim change in science. Thus, an axiology is naturalistic-two if and only if that axiology has been arrived at by empirically testable methods.

In any case where an axiology is naturalistic-one it will also be naturalistic-two. If the naturalist's axiology is an accurate imitation of the

¹⁶ Laudan, 1990, p.44

popular scientific axiology, one can be sure that axiology has been arrived at empirically. However, an axiology can be naturalistic-two and not naturalistic-one. Just imagine a case where the naturalist philosopher of science, through empirical testing, attempts to identify the most popular methodological constraint on aim change in science (i.e. satisfies naturalism-two), *but* gets the results wrong. In that case, the philosopher of science's axiology would not be the prevalent one in science, i.e. the conditions set out by naturalism-one would not be met.

We now have an answer to the question: what are the general conditions under which an axiology would be considered naturalistic? An axiology is naturalistic-one if and only if that axiology is prevalent in science, and an axiology is naturalistic-two if and only if that axiology has been arrived at by empirically testable methods. Since these two definitions exhaust our understanding of naturalism, it is safe to stipulate a third definition: An axiology is completely naturalistic if and only if it satisfies the demands set by *both* naturalism-one and naturalism-two. Even though Doppelt's objection is that Laudan's axiology is non-naturalistic in the narrow sense of naturalistic-two, I will see whether it is completely naturalistic.

It should now be clear how Laudan *could* respond to Doppelt's objection. Laudan only needs to point out that his job, as an epistemic naturalist, is to examine particular cases of successful aim change in the history of science in order to determine which criteria are employed as factors influencing such change, then to copy the findings. Specifically, what Laudan needs to establish is that realizability has been the main criterion governing the adoption and rejection of aims throughout the history of science. If Laudan can verify this, then it is not only acceptable

but obligatory that the naturalist also rely on realizability as the main criterion for aim change. Whatever the popular trend in science, the naturalist must follow suit. In other words, if the main criterion for justified aim change in science is realizability, then normative naturalism dictates the rationality of realizability to justify aim change in the philosophy of science.¹⁷

Doppelt's criticisms have now been answered: First, Laudan's account looks just like what a properly naturalistic account of justified aim change *should* look like. Second, the worry that a non-naturalist component to a naturalistic axiology betrays a naturalistic meta-methodology is plainly misguided; to think otherwise would be to confuse the naturalist's responsibility. At its core, rational aim change in science appears to be dependent on super-empirical criteria - Doppelt is right about this. But he is wrong to think that this is a problem for Laudan. How science is successful is not the naturalist's business. The point is that it is successful,¹⁸ and it's the naturalists duty to first, find out why, and then to copy the findings. The burden on the naturalist is to emulate science, that is, to determine by empirical means how things are done in science and mimic the findings. Again, that those findings indicate that justified aim change in science hinges on conceptual criteria has no bearing on a naturalistic axiology; the account of aim change here described is completely naturalistic. Moreover, realizability will be a rational criterion for aim change in the philosophy of science for so long as it is a rational criterion for aim change in science. If (and when) empirical investigation should show that other criteria are prominent in justified aim change in science, then the normative naturalist simply drops realizability and adopts

¹⁷ In many discussions on the topic of normative naturalism the distinction between the scientist and the philosopher of science is blurred. This is a carelessness which only serves to confuse the role of naturalism in the philosophy of science.

¹⁸ I explain the notion of 'success' within Laudan's normative naturalism in chapter five below.

those other criteria.

The foregoing discussion may give us an idea of how Laudan *could* respond to Doppelt's final concern. Recall, Doppelt argued that the employment of any particular super-empirical criteria in the rational evaluation of aims presupposed a theory of scientific rationality. In Laudan's case, the specific objection was that Laudan's attachment to realizability (instead of, e.g., happiness-inducing) as a standard methodological constraint on aims presupposed the rationality of realizability. The suitable response to this is clear: However valid this criticism may be, it is obviously aimed at the wrong target. It is not Laudan's difficulty that aim change in science presupposes a theory of rationality - if indeed it does. This criticism would be more appropriately directed to the scientist - not the philosopher of science - since it is not the naturalists job to justify aim change in science, only to replicate it.

We now know what the general conditions are under which justified aim change may be considered naturalistic; we have seen how Laudan *could* respond to Doppelt's objections. Now the important question is: Is there any reason to think that this is how Laudan *would* respond to Doppelt's objections? That is, can one extract from Laudan's writings enough textual evidence to construct an argument which will show that he thought axiology to be naturalistic for the same reasons as described above? The answer to this is yes. However, to construct such an argument takes a bit of work. This is because Laudan has (at least) two different ways to justify aim change, or so it appears, and he is not overly clear about either. One is naturalistic, and when made explicit resembles the 'copy science' argument elaborated above. The other justification is a non-naturalistic appeal to common sense. After careful review of the

evidence for both of these views, we will see that the latter is not so much an independent justification for aim change as an explanation of the naturalistic 'copy science' view. The conclusion to be drawn is that one can fairly attribute to Laudan the naturalistic argument, which satisfies both definitions of naturalism, constructed above.

It is obvious from (any sample of) Laudan's writings that he is convinced he has given sufficient evidence of the naturalistic character of his account of justified aim change. As we already know, the realizability criterion is the main principle; what we need to figure out is *why* Laudan thinks it is naturalistic. Realizability, he claims, is a criticism "which one regularly finds in scientific controversies."¹⁹ Quite clearly, this is a descriptive claim about the enterprise of science, specifically about how goals are evaluated in science. This seems to indicate Laudan's justification for the naturalistic character of his axiology. Namely, that the reason why normative naturalism relies heavily on the realizability criterion is because empirical testing has shown that scientists have acted with something approximating the realizability criterion in mind. And, since science relies on the realizability criterion for rational evaluation of aims, then, as normative naturalists, so should we. In other words, the above passage is indicative of a 'copy science' argument. It is true that Laudan never shows exactly how this is supposed to work. Still, there is a lot more evidence scattered through the bulk of Laudan's writing which supports this view.

Take, for example, this passage: "If we want to understand how science works, it is clearly important to understand the reasoning processes that drive communities of researchers so far as to change some of their

¹⁹ Laudan, 1984, p.53.

basic aims and goals".²⁰ Whereas in the last excerpt Laudan stated the conclusion of his empirical findings, namely that realizability is a decisive constraint of any proposed scientific aim. Here, he is pointing to the foundation of his naturalistic axiology: We need to find out how aims are rationally evaluated in science. And again, here is another statement of the results of his empirical research of aim change in science: "it is the adjudication of such criticism [i.e. realizability] and the responses it produces which have led to the revision of some of our once highly cherished cognitive ambitions for science."²¹

That Laudan attached himself to the 'copy science' argument is somewhat explicit in at least a few passages. For instance, he claims that "The naturalist, if true to his conviction that science and philosophy are cut from identical cloth, holds that the same mechanisms which guide the change of aims among scientists can guide the epistemologist's selection of epistemic virtues."²² The message here is transparent: The naturalist must, first, investigate to find out what the criteria are for aim change in science, and second, copy them. The picture we are given is that "the whole of meta-methodology is a mixed empirical/conceptual discipline, rather like the theoretical sciences, with precisely the same links to experience exhibited by those sciences."²³ The realizability criterion, according to Laudan, is just one of those links.

As we can see, there is plenty of documentation which supports the view that Laudan thought his axiology to be naturalistic in virtue of its ability to accurately imitate the empirical findings - axiology is naturalistic-one and

²⁰ Laudan, 1984, p.47.

²¹ Laudan, 1984, p.53.

²² Laudan, 1990, p.47.

²³ Laudan, 1987(b), p.231.

naturalistic-two. The problem, as his critics might point out, is that there is evidence to suggest Laudan has other justifications for his axiology; namely, a non-naturalistic appeal to common sense. Laudan has argued that realizability is a criterion for aim change because, in his opinion, unrealizability is overwhelming empirical confirmation of the utopianism of the goal, which in turn is overwhelming evidence to pursue something else. The justificational recourse here is to common sense: Wouldn't we think it was strange if people set out to achieve utopian goals? Thus, Laudan claims that "we customarily regard as bizarre, if not pathological, those who earnestly set out to do what we have very strong reasons for believing impossible.²⁴ This sentiment is echoed throughout Laudan's writings on normative naturalism. Here is another example reflecting the same conviction from a later writing:

But if I am right that goals are often abandoned when strong evidence emerges for their nonrealizability, then the abandonment of such goals is urgently demanded. *Those who think otherwise must explain to us how practical inference could ever proceed in the face of goals which could not be linked to possible actions to realize them.*²⁵

If Laudan has a naturalistic justification for his axiology, why does he bother with this non-naturalistic appeal to common sense? The most plausible answer is that the appeal to common sense is best viewed as an effort to explain why, in the first place, science gives such prominence to the realizability criterion. This can be inferred from many of Laudan's discussions on the realizability criterion. For instance, he claims that "This proposed constraint on rationally admissible goals - although not commonly

²⁴ Laudan, 1984, p.51.

²⁵ Laudan, 1987(b), p.227-228. Italics mine.

acknowledged by philosophers - should be anything but controversial."²⁶ Here, it seems clear that Laudan is interested in explaining why realizability is the preferred criterion of aim change in science.

Laudan implicitly holds the 'copy science' argument; this is not just how Laudan *could* respond to Doppelt, it is how he *would* respond. Laudan is guilty only of not being explicit or even clear about the way this is supposed to work. Simply put, there is a link missing in Laudan's writings, the connection which establishes the copycat nature of the naturalist. Without that link, it is certainly easy to see what motivated Doppelt's objection, since, of course, realizability is a super-empirical criterion with no naturalistic foundation. But with the link, that realizability has no naturalistic basis is no longer a problem for the naturalist, hence the objection disappears.

We now know what the general conditions are under which an axiology may be considered (completely) naturalistic. And we have reason to believe that these are the reasons why Laudan thinks his axiology is naturalistic. We also know that Doppelt claimed Laudan failed to satisfy naturalistic-two: An axiology is naturalistic if and only if that axiology has been arrived at by empirically testable methods. It is now quite clear that Doppelt was wrong; Laudan's axiology is established by empirically testable methods. The next question we must ask is: Does Laudan have it right? That is, has he done the appropriate testing, and is his conclusion, i.e. that scientists employ the realizability criterion, correct? In other words, is Laudan's axiology naturalistic-one?

Intuitively, it seems like Laudan axiology is not naturalistic-one. The intuition here is just the opposite, i.e. that a common aim in science is to realize hitherto utopian goals; if this is right, then it would be odd indeed if a

²⁶ Laudan, 1984, p.51.

main methodological constraint on aims in science hinges on their potential for realizability. As outsiders, don't we, in fact, deem supremely rational those scientists who made 'inconceivable' discoveries, who achieved what everyone else considered unrealizable? Laudan even admits this in Science and Values, where he says "judgments of this sort [e.g. realizability], like all other judgments, are fallible. More than occasionally, our background knowledge has turned out to be so flawed as to lead us to regard as logically or physically impossible what we later learned was entirely possible."²⁷

On the other hand, maybe the intuition here is wrong. Laudan does offer some evidence for the position that realizability is a main criterion for aim change in science. For example, he cites "the goal of developing apodictically certain theories; the goal of deducing theories from the phenomena; the goal of giving a fully deterministic account of all natural phenomena"²⁸ These are all scientific aims which, according to Laudan, were dropped when it came to be known that they were unrealizable.

In a sense this is all for naught. Certainly science uses some conceptual tools; it combines, tests, configures its facts according to certain general (if changing) principles. As naturalists, our responsibility is to discover and accurately copy that mixture. If Laudan has failed to come up with the right mixture, that is, if his axiology is not naturalistic-one, then he just needs to do more empirical testing in order to come up with the correct results. Thus, it would not be too devastating if his normative naturalism presently relied on incorrect criteria for aim change- *if* indeed this is so - since this is relatively easy to remedy. If further testing singles out other conceptual criteria which are preponderant in science then

²⁷ Laudan, 1984, p.51.

²⁸ Laudan, 1987(b), p.234, note 11.

Laudan's reticulated model of scientific rationality can rely instead on those other criteria.

We can now conclude that Laudan's normative naturalism has the resources to provide a completely naturalistic account of justified aim change. For those who think axiology must be naturalistic-one, i.e. something is naturalistic if and only if science does it, this account of axiology will be sufficiently naturalistic: Axiology in science hinges on the realizability criterion and therefore so does axiology in Laudan's philosophy of science. In this sense of naturalism, Laudan's axiology is *exactly* as naturalistic as axiology in science. This account will also be sufficiently naturalistic to those who think axiology needs to be naturalistic-two (such as Doppelt), i.e. something is naturalistic if and only if it exhibits P, where P is empirical testability. Following this sense of naturalism, Laudan's axiology is even *more* naturalistic than axiology in science. The reason for this is plain to see: the normative naturalist, in an effort to find out what principles do in fact guide aim change in science, is entirely restricted in her methods to empirical testing (save for the sole non-empirically testable criteria: find out what guides aim change in science²⁹). The scientist, on the other hand, in an effort to determine what should direct aim change in science, employs a combination of conceptual criteria and empirical investigation. Thus, if naturalistic-two is defined strictly as empirical testability, then in this sense normative naturalism is more naturalistic than science.

Hence, in Laudan's meta-methodology both methodology and axiology are naturalistic. We now have a more complete picture of this meta-methodology: Methodological change, for example from x to y, is (instrumentally) rational if our empirical investigations tell us that x is a

²⁹ This hints of a version of the notorious 'circle argument', which I discuss shortly.

better means to z than y. Axiological change, for example from X to Y, is (instrumentally) rational if our empirical investigations tell us that X is not realizable and Y is .

As we have seen, the presence of a conceptual component (namely realizability) has no bearing on the naturalistic character of Laudan's meta-methodology. But through the course of showing why this is the case I may have motivated a number of other criticisms. The most significant one which the critic may be posed to ask is: How it is that Laudan can investigate the methods of science, without first presupposing scientific method in order to conduct this investigation? Or, is it not the success of science that tells Laudan to copy science? Or, how is it that Laudan deems realizability naturalistic just because scientists pay attention to it? However dressed up, these are variations of the same general form of argument, namely the ill-famed 'circle argument'. The idea behind all circle arguments is that they must be either circular, beg the question, or lead to a regress.³⁰ The apparent inescapability of circle arguments has been pointed out by Richard Rorty: "To know what method to adopt, one must already have arrived at some metaphysical and some epistemological conclusions. If one attempts to defend these conclusions by the use of one's chosen method, one is open to a charge of circularity."³¹ With respect to Laudan's normative naturalism, the charge is that his study of scientific axiology to discover the criteria employed in rational aim change presupposes an aspect of scientific methodology; specifically, Laudan must presupposes some naturalistic criteria (e.g. empirical testability) in his effort to discover other naturalistic criteria (e.g.

³⁰ Giere, 1985, p. 333.

³¹ Richard Rorty, "Introduction", The Linguistic Turn, Richard Rorty ed., (University of Chicago Press) 1967, p. 1.

realizability).

Circle arguments have a long history in philosophy; within the philosophy of science, naturalism is especially susceptible to the charge of circularity since, as Giere put it, "any empirical investigation aimed at discovering the criteria that scientists use for evaluating evidence would necessarily presuppose at least some of the criteria it was supposedly setting out to discover."³² Though at the time he was discussing paradigms, Kuhn's comments on the circle problem fit here:

When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense. The resulting circularity does not, of course, make the arguments wrong or even ineffectual. The man who premises a paradigm when arguing in its defense can nonetheless provide a clear exhibit of what scientific practice will be like for those who adopt the new view of nature. That exhibit can be immensely persuasive, often compellingly so. Yet, whatever its force, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle. ³³

Not everyone - notably non-naturalists - will be satisfied with this answer (but of course that is Kuhn's very point). Yet, unless the naturalist is willing to take the self-defeating route and cite some *a priori* principle to defeat the circle, there may be no way around it.

That said, the naturalist can respond to the circle argument in one of two ways. Laudan could respond, for instance, by claiming that circularity is not really a problem for his position, after all.³⁴ The way this response

³² Giere, 1985, p.333.

³³ Thomas Kuhn, 1970, (second edition), p. 94.

³⁴ Ronald Giere thinks that evolutionary theory can show how the naturalist need not be worried about defeating the circle argument. See Ronald Giere, "Toward a Unified Theory of Science", in Science and Reality, ed.'s James T. Cushing, C.F. Delaney and Gary M. Gutting, (University of Notre Dame Press) 1984, pp.5-31, and Giere, 1985, especially pp. 339-349.

works is anticipated in Rorty's and Kuhn's remarks: The inescapability of the circle is *not* paradigm specific (so to speak) to 'naturalism'; what at first looks insurmountable proves to be so, but it is so for all philosophical domains. Thus, in as much as the circle problem presents an obstacle for the naturalist, it presents an obstacle for the non-naturalist alike (or the foundationalist, the *a priorist*, or the intuitionist...). For instance, the foundationalists' belief that there is a set of foundational beliefs which are somehow indubitable is itself supposed to be a foundational belief; but what justifies *that* belief? There is no good answer to this question, except to say, with Rorty and Kuhn, that every philosophical paradigm or domain needs to presuppose some of the fundamental tenets of what is arguing for. And that is why the circle problem ceases to be a difficulty that the naturalist must solve. Although this response does not make the circle criticism less devastating for the naturalist, at least it shows that it is not a problem unique to her position. Thus, the naturalist can rest assured knowing that the charge of circularity is equally applicable to a non-naturalist philosophy of science.

The other response the naturalist can make to the circle argument may be more compelling, but it motivates an altogether different criticism. The response and the criticism it provokes are both due to David Stump.³⁵ I will first examine his response to the circle argument.

According to Stump, the naturalist does not need to take the route described above, which is to argue that some types of circularity are acceptable.³⁶ To think so, Stump claims, is to ignore the plurality of methods

³⁵ David Stump, "Naturalized Philosophy of Science with a Plurality of Methods", in Philosophy of Science, Vol. 59, 1992, pp.456-460, and "Fallibilism, Naturalism and the Traditional Requirements for Knowledge", in Studies in the History and Philosophy of Science, Vol.22, 1991, p.451-469.

³⁶ Stump, 1991, p.469.

and aims in scientific activity. Once the naturalist recognizes this plurality, there opens up a good way to defeat the circle: A plurality of methods and aims in science means that there are areas in scientific practice diverse enough that one area can ground another without begging the question.³⁷ According to Stump, "only a Kuhnian holism makes it seem as if theories, methods and aims are tightly connected in a single matrix such that no theories or aims are independent, and makes the charge of circularity seem plausible."³⁸

This response is obviously a plausible one for Laudan. Recall, I stated earlier that the charge of circularity directed at Laudan's meta-methodology is aimed specifically at his employment of certain naturalistic criterion (i.e. empirical testability) to discover other naturalistic criterion (i.e. realizability). Since these two naturalistic criteria are independent of one another, following Stump, one (e.g. empirical testability) can be used to ground the other (e.g. realizability). In this way, the naturalist can avoid the charge of circularity.

This response to the circle argument may be more compelling than the other; however, as I stated earlier, it is problematic in that it motivates an altogether different criticism. The new criticism is just this: The normative naturalist, we concluded, is to derive an axiology by imitating scientific axiology. Yet, as we have just seen, there is a plurality of methods and aims in science; 'science' itself is made up of many subdisciplines. Whose methodology, whose axiology, the objection goes, is the normative naturalist is supposed to replicate? That of physics, biology, chemistry, anthropology, or medicine?

What this objection rightly draws our attention to is that, apparently,

³⁷ Stump, 1991, p.457.

³⁸ Stump, 1991, p.468.

Laudan's normative naturalism presupposes a unity of methods and aims in science. This, according to David Stump, is one of the two dogmas of naturalized epistemology.³⁹ As Stump states,

The attempt to privilege one science as a model for philosophy of science shows that the naturalists share essentialism with the traditionalists. But the assumption that there is one unified scientific method that applies everywhere has been refuted.⁴⁰ Furthermore, we have been given no reason to think that only one method will be successful in philosophy of science, and using only one method where several may apply amounts to a reduction and distorts the philosophy of science by leaving out these other methods of inquiry.⁴¹

As I said, it seems to be the case that Laudan's normative naturalism assumes a unity of both methodology and axiology in science. What needs to be determined is whether Laudan should fear Stump's fiat regarding the abandonment of this so-called dogma. In other words, does Laudan's normative naturalism, in presupposing unity of science, distort either the enterprise of science, or his philosophy of science? I think the answer to both these questions is a resounding 'no'. In the first place, in order to make sense out of the scientific enterprise, in order to construct a philosophy of science, the naturalist *must* assume a unity of science. Otherwise, 'science' would be too enormous a concept, and hence impossible to copy. Moreover, one could argue that the sort of unity presupposed by Laudan's meta-methodology already exists in science. Take, for instance, Laudan's methodology. One would think that the methodological rule of means/ends connections (e.g. instrumental rationality) is prevalent in all scientific activities, that it is a universal and

³⁹ David Stump, 1992. The other dogma is that a naturalist epistemology cannot be normative; or, as Stump says, "accepting the 'naturalistic fallacy' as real". p.458.

⁴⁰ The footnote here is to Stump 1991.

⁴¹ Stump, 1992, p.458.

general principle of all science. Next, take Laudan's axiology; it is also plausible that realizability is a rational constriction on aims in more or less all areas of science. In other words, since Laudan borrows from science only very general principles of method and axiology, there is little worry that he is guilty of distorting the machine of 'science'.

Again, Stump's warning is an important one, but it is not one that Laudan needs to be overly concerned with. Stump is right that "any method of science could be applicable *in principle* to the study of science."⁴² This may very well be true, but it does not entail that every method *is* equally applicable, nor does it entail that there aren't certain general principles in science that are especially dominant in all scientific endeavors, and hence especially prominent in a naturalized philosophy of science.

I said at the start of this chapter that I was going to determine whether Laudan's normative naturalism is, in fact, naturalistic. We have now resolved that Laudan's meta-methodology, in particular the main criteria guiding aim change within the reticulated model of scientific rationality, has a perfectly naturalistic foundation. As we will soon see, the criticisms in the following chapter force an even more detailed examination of Laudan's reticulated model of scientific rationality.

⁴² Stump, 1992, p.458. Italics mine.

Chapter Four

In the last chapter I set out to dispell the worry that the main criterion guiding aim change in Laudan's reticulated model of scientific rationality had a non-naturalistic foundation. In this chapter I look at three more criticisms aimed at Laudan's axiology. These criticisms have a common thread running through them: They all question the viability of Laudan's reticulated model of scientific rationality, in light of his descriptive view on aim variance in the history of science.¹ The first objection I examine states that Laudan's reticulated model of scientific rationality results in underdetermination, at potentially all three levels (i.e. theory, methodology, axiology) of the triad. Laudan's critics argue the way around the problem of underdetermination is to posit the existence of a transhistorical goal of science. After I respond to this criticism I look at the objection that Laudan's account of the notion of 'progress', as it is understood in light of the reticulated model of scientific rationality, betrays an implicit commitment to an enduring, transhistorical goal of science. Finally, I explore the objection which states that in the absence of a transhistorical goal of science, the concept of 'progress' is rendered essentially meaningless.

Although I argue that there is a fairly easy answer to each of these criticisms (which is available to Laudan), they are nevertheless important objections; not least because they are common criticisms of Laudan's normative naturalism, which have been raised in various incarnations by some of his most eminent critics: Philip Kitcher, James Robert Brown and

¹ See chapters two and three for an elaboration of Laudan's descriptive and prescriptive axiologies.

John Worrall.²

In order to gauge the full force of these three criticisms, we need to start by reflecting more closely on Laudan's reticulated model of scientific rationality. In particular, recall that according to this model aims, methods and theories are in a triad of reticulation, and are each subject to change. There is a *prima facie* problem with this view, which although easily overcome, sets the stage for the more substantial criticisms which follow. The problem is just this: If goals and methods and theories are constantly liable to change (and in fact do change), then, within any historical triad, there will be no standard by which we can say either that progress has been achieved, or that aim change is rational,³ *even if* that standard is only a relative one.

Even some of Laudan's critics agree that this problem is easily overcome.⁴ In Laudan's meta-methodology, as he describes it, change within any triad is *not* wholesale (e.g. as Kuhn would have it), but rather piecemeal.⁵ When we look at the history of science, we see only a large scale global transformation; for example, we see that a triad *t*₁, consisting of theory *T*₁, methodology *M*₁, and axiology *A*₁ has been replaced by another triad *t*₂, consisting of theory *T*₂, methodology *M*₂, and axiology *A*₂. But it is a great mistake, Laudan claims, to think that *t*₁ was replaced in one fell swoop by *t*₂; this, he argues, is neither historically accurate nor epistemically attractive.⁶ What actually happens, according to Laudan, is

² Brown, 1989, esp. pp. 124-126; Philip Kitcher, *The Advancement of Science*, (Oxford University Press), 1993, esp. pp. 157-160; John Worrall, "Fix it and be Damned: A Reply to Laudan", in *British Journal of the Philosophy of Science*, Vol. 40, 1989, esp. pp.376-388.

³ i.e. If our theories and methodologies change concomitantly with our axiologies, then, for example, there will be nothing to ground the claim that an aim is unrealizable.

⁴ Kitcher, 1993, p.158.

⁵ Laudan, 1984, p.65.

⁶ Laudan, 1984, p.78 and p.82.

that one or two aspects of the triad remain (temporarily) fixed, while one other is being challenged for revision. Laudan wants us to imagine, for example, that within t_1 , both M_1 and A_1 remain stable, while T_1 is being challenged by T_2 . In this scenario, we are able to judge T_1 and T_2 against the standards set by M_1 and A_1 . As Laudan puts it,

changes in values and changes in substantive ontologies or methodologies show no neat isomorphism. Change certainly occurs at all levels, and sometimes changes are concurrent, but there is no striking covariance between the timing of changes at one level and the timing of those at any other.⁷

Laudan's insight that change within any triad happens in a piecemeal fashion is a critical aspect of his reticulated model of scientific rationality. Because change is piecemeal, a revision in one aspect of any triad can be measured against the other, then stable, aspects. This allows for (temporarily) fixed standards by which judgements of rational change and progress make sense. As Laudan states, "Because these changes are not always concomitant, we are often in a position to hold one or two of the three levels fixed while we decide whether to make modifications at the disputed level. The existence of these (temporarily) fixed and thus shared points of perspective provides a crucial form of triangulation."⁸

Although this analysis makes it seem that the reticulated model of scientific rationality can account for (relative) progress and (relative) rational aim change, the general tone of the problem facing reticulation is explored at a deeper level in the criticisms mentioned above. First, I look at the criticism raised by Kitcher that each element in Laudan's reticulated

⁷ Laudan, 1984, p.84.

⁸ Laudan, 1984, p.84.

model is at risk of underdetermination.⁹

As the examination above illustrates, with Laudan's reticulated model of scientific rationality, theories, methods and aims change in a piecemeal style. The problem of underdetermination facing this view is typical: Though there may be good reasons motivating change at one level in any triad, e.g. the rejection of T1 and adoption of T2, there are (according to Laudan's critics) equally good reasons motivating other sorts of change at that same level, e.g. the rejection of T1 and adoption of T3; and, in these cases, A1 and M1 equally support both T2 and T3. In other words, there are potentially a number of equally good ways to resolve tension within any triad, even though there may be stable levels against which competing choices can be measured. Even Laudan concedes this point; he claims "the uncontested levels will not always resolve the controversy, for underdetermination is an ever present possibility."¹⁰

In the example above, there is underdetermination at the theory level, i.e. T2 is underdetermined by T3. Following the same line of argumentation, both axiological change and methodological change are at risk of underdetermination. For example, imagine that within t1, M1 and M2, according to T1, are both the best means of achieving A1. In this case, which methodological rule are we to chose? This example is again

⁹ Kitcher, pp.157-160,1993. Doppelt, 1986, pp. 238-242, also launches a similar attack on Laudan, but his argument contains an enormous weak link. Doppelt's argument that Laudan's reticulated model is open to underdetermination is premised on the claim that unless the reticulated model can accommodate a number of key assumptions (p.239), any change which maximizes epistemic gain will also experience an epistemic loss; hence, he concludes, Laudan's account leads to underdetermination, and to what he calls 'moderate relativism'. Doppelt is certainly right to draw this conclusion, *if* in fact Laudan's account cannot accommodate the key assumptions outlined by Doppelt. But Doppelt never shows *why* Laudan can't adopt these assumptions, and in fact it seems plain that he would have no problem at all fitting them into his account of change within the reticulated model, hence undermining the strength of Doppelt's criticism.

¹⁰ Laudan, 1984, p.84-85.

easily replicated with respect to axiological change; for instance, where within triad t1, we have two competing aims - A1 and A2 - which, according to T1 and M1, are judged to be equally good.

According to Kitcher, the way to circumvent the problem of underdetermination facing Laudan's reticulated model is to posit the existence of a transhistorical goal of science (the favorites continue to be: knowledge, truth, or truth-likeness). In fact, Kitcher argues that because Laudan deems rational certain principles of change, and therefore thinks some ways of resolving tension within a particular triad are better than others, Laudan must implicitly hold such a position.¹¹ Why else would Laudan advocate adjustment within triads, or deem certain principles of change the *right* principles?

To make his point, Kitcher asks (rhetorically) what, in Laudan's view, "makes the principles of rational transition among L practices¹² the *right* principles?"¹³ The most likely answer, he claims, is that "there are enduring goals that are not represented in Laudan's official picture"¹⁴. Otherwise, he states, why should relief from inconsistency be sought? Moreover, "why it should not be sought in the obvious fashion of hacking away at our L practices until they are so minimal as to avoid the contradictions."¹⁵ According to Kitcher, Laudan can only explain rational change within a triad by appeal to the promotion of an enduring goal of science.¹⁶

Although Kitcher argues that to avoid underdetermination Laudan must attach himself to the notion of a transhistorical goal of science, we

¹¹ Kitcher, 1993, p.159.

¹² L practices = Triad.

¹³ Kitcher, 1993, p. 159.

¹⁴ Kitcher, 1993, p.159.

¹⁵ Kitcher, 1993, p.159.

¹⁶ Kitcher, 1993, p.159.

are by now well aware that Laudan thinks this notion is abhorrent; again and again we have seen that Laudan explicitly disavows the idea of an enduring, constant goal of science. The question we now need to respond to is can he consistently maintain this view, in light of the problem of underdetermination? The answer here, most certainly, is yes. Laudan's reticulated model of scientific rationality is able to overcome this problem for two main reasons. The first reason is unique to the problem of axiological underdetermination. Kitcher (like Doppelt before him) has completely overlooked that Laudan offers a rational evaluation of aims, in which we learn that certain axiological changes are better than others, specifically those that are forced for naturalistic reasons. If within a triad there are competing aims, e.g. A1 and A2, Laudan can easily discriminate between A1 and A2 on the basis of naturalistic criteria, i.e. realizability of aims, and harmonization of internal/external aims. Thus, with respect to aims, not all potential ways of resolving inconsistency or tension within any triad are equally good; notably, aims which satisfy the naturalistic criteria will be the best ones. Because Kitcher ignores Laudan's naturalistic evaluation of aims, he fails to see this as a realistic option for undercutting underdetermination, at least at the level of aims.

That said, this response is only applicable to axiological underdetermination, and then even only partly. The naturalistic criteria placed on aim change will weed out some aims, thus narrowing the field of competing aims. But there may be more numerous aims which satisfy these criteria. Laudan readily concedes this point. After outlining the various naturalistic criteria, he admits that "doubtless a side range of cognitive goals or values can satisfy the demands laid down here."¹⁷ Thus,

¹⁷ Laudan, 1984, p.63.

the problem of axiological underdetermination still needs to be resolved, as does the problem of underdetermination with respect to methodology and theory.

This brings us to the second way Laudan can overcome the problem of underdetermination. Namely, what is needed is a closer analysis of the concept of 'underdetermination'. In particular, there needs to be a distinction drawn between two kinds of underdetermination: logical and empirical. Logical underdetermination states that there are an infinite number of theories which will fit with any given methodology/axiology pair, and an infinite number of methodological rules which will accord with any given theory/axiology pair, and correspondingly an infinite number of aims which will match a theory/methodology pair. Empirical underdetermination says that, in practice, what is available to the scientific community at any given time is a limited, hence finite number of realistic possibilities. This distinction between empirical and logical underdetermination is a common solution to this problem,¹⁸ and it is what rescues Laudan's position from a worse fate. Brown characterizes this crucial distinction like this:

It is only by blurring the logical sense of 'exists' (in which there do exist infinitely many different theories) with the available-at-hand sense of 'exists' (in which only a very few theories¹⁹ exist) that the argument from underdetermination gets off the ground.²⁰

To which he later adds,

The problem is laid to rest by the fact that there simply aren't infinitely many theories to choose from. In a logical sense there are, of course, but from any practical point of view,

¹⁸ Brown, 1989, pp.50-52 and pp.121-122. See also Giere, 1984, pp.7-9.

¹⁹ or, we can add, methodologies or axiologies.

²⁰ Brown, 1989, p.51.

there are only a very few live options on the table.²¹

Thus, the problem of underdetermination, when understood in the logical sense, would present a threat to Laudan's position; yet, importantly there is no threat when underdetermination is understood in empirical terms.

Hence, Laudan is *not*, contra Kitcher, forced to posit the existence of a transhistorical goal of science to circumvent this problem.

Next, I discuss the criticism, due to Brown, that Laudan's account of the notion of 'progress', as it is understood in light of the reticulated model of scientific rationality, betrays an implicit commitment to an enduring, transhistorical goal of science; after that, I examine the related criticism, as Worrall frames it, that without a constant, enduring goal of science Laudan cannot talk about 'progress' (or 'success') in science, at least in any meaningful sense of the term.

To start with, however, I will give a brief introduction to Laudan's view on progress. There are two important aspects of the notion of 'progress' or 'success' in Laudan's work. The first is that 'progress' is a relative concept, measured against a historically based set of goals.²² This definition of progress may be obvious from what we already know of Laudan's axiology. Since there are no universal, historically transcendent goals of science, in Laudan's meta-methodology progress will always be "progress relative to some set of aims", ²³ moreover "there is no uniquely appropriate set of those ends."²⁴ Thus, while "customary usage encourages us to fall into speaking of scientific progress in some absolutist

²¹ Brown, 1989, p.122. I should note that the intention of Brown's critique of underdetermination in this passage was *not* to save Laudan (i.e. what I am using it for), but rather to criticize him for accepting underdetermination at the level of methodology.

²² Laudan, 1984, p.66.

²³ Laudan, 1984, p.66.

²⁴ Laudan, 1984, p.66.

sense,"²⁵ in Laudan's view, semantics aside, there is *unquestionably* no sense of progress which is absolutist or universal.

The second important aspect of the notion of 'progress' is that, in Laudan's view, it is a non-normative, descriptive notion. Basically, Laudan claims that uses of the terms 'progress' and 'success' amount to empirical assertions about the efficiency of means/ends action. As he puts it, "success, so conceived, is not a valuational or a normative concept. To claim that a certain action was successful is to make a contingent, empirical claim about the relation of that action and its outcomes to certain goal states."²⁶ Because success is a non-normative concept, to say that some activity has been successful, or has progressed, is not to endorse that activity. As Laudan notes, there can be successful bank robbers and wars.²⁷ Also, as he claims, "one and the same action may be unsuccessful or successful, depending upon the goals in question."²⁸

Both Brown and Worrall take issue with this account of progress. Brown's main objection is that the relative notion of progress explicitly embraced by Laudan masquerades his true attachment to a transhistorical notion of progress which is implicit in his reticulated model of scientific rationality. The most sustained argument Brown makes against Laudan is found in the following passage. Here, Brown argues that Laudan's relativism with respect to progress:

is to some extent undermined by a few simple observations. To start with, inside any given triad we can, as Laudan argues, recognize tensions; we want to overcome those tensions and we usually have ways of doing so. Does it not follow from this,

²⁵ Laudan, 1984, p.66.

²⁶ Larry Laudan, "Explaining the Success of Science: Beyond Epistemic Realism and Relativism", in *Science and Reality: Recent Work in the Philosophy of Science*, Ed.'s James T. Cushing, C.F. Delaney, and Gary M. Gutting, (University of Notre Dame Press), 1984 (b), p.87.

²⁷ Laudan, 1984(b), p.87.

²⁸ Laudan, 1984(b), p.87.

trivially in fact, that we must have a transhistorical goal? Why else would we tinker with any triad? And does it not further follow that we have a transhistorical method for doing so? If we do have such a goal and a way of achieving it then there is indeed a transhistorical sense of scientific progress; it is progress brought about by harmonizing the {T,M,V} triad. ²⁹

The main problem with Brown's argument is betrayed in another comment he makes. He states that "progress, after all, must be progress toward some goal, but no goal will stand still; the aims are evolving."³⁰ Brown apparently overlooks a dominant characteristic of Laudan's reticulated model of scientific rationality: Change within any triad happens in a piecemeal style; as we saw earlier, one or more levels within a triad remain stable, and they are the backdrop against which rational change on another level is measured. Because Brown fails to recognize this, he draws the conclusion that adjustments within a triad imply a goal beyond the triad; then, from this, i.e. the existence of a transhistorical goal of science, he infers a transhistorical notion of progress. But Brown's first inference is clearly faulty. The logic underlying this inference is that 'goals change' implies 'goals never remain (temporarily) fixed'; but of course, this is fallacious. Aims are subject to change (and do change), but they are certainly not in constant flux. Once this aspect of Laudan's reticulated model of scientific rationality is clarified, 'tinkering with triads', in the absence of a transhistorical goal of science, makes perfect sense.

Like Brown, Worrall thinks Laudan's account of progress is problematic. On this regard, he presents a number of arguments against

²⁹ Brown, 1989, p.126.

³⁰ Brown, 1989, p.125.

Laudan;³¹ the one argument I examine here is that if the notion of progress is to be at all meaningful, then Laudan must relativize it to the notion of an enduring, transhistorical goal of science. This argument is found in the following passage, where he asks:

Is the 'justification' simply that our present methods turn out better *when judged from our present point of view*? But, as Mandy Rice Davis might have said, our present point of view 'would say that, wouldn't it?' The question is whether our present point of view is *right* to say that our present methods are better than the methods of science of three centuries ago. And a positive to answer that question requires some principles considered as outside the historical process.³²

Though on the surface Worrall's remarks seem apt, his argument fails to withstand deeper analysis. Worrall's argument is that on Laudan's view, methods (and aims and theories) are going to be judged the best, *because* they will be judged from within our present point of view. This is a classic *non sequitur*. If our current methods, theories and aims were judged to be the most successful simply because they are ours, there would be no reticulation at all! But obviously judgements of success do not necessarily accompany standpoint theory. It is as likely that our present point of view will indicate negative aspects of our present triad.

The main thrust of this argument Worrall gives fails: He is mistaken that relative pronouncements of progress are necessarily positive ones. If he *were* right about this, then perhaps he would also be right that 'progress', in Laudan's system, is a meaningless notion. But he is wrong on both counts.

³¹ See also John Worrall, "The Value of Fixed Methodology" in British Journal of the Philosophy of Science, Vol. 39, 1988, pp.263-274. The other main argument (which I think is obviously invalid) that Worrall gives is that there is a logical problem with the piecemeal change at work in the reticulated model of scientific rationality. For this argument, see Worrall 1988, pp.266-267, and Worrall 1989, pp.382-384. For Laudan's reply to Worrall (on this and other points) see Larry Laudan, "If It Ain't Broke, Don't Fix It", in British Journal of the Philosophy of Science, Vol. 39, 1988, pp.369-375.

³² Worrall, 1989, p.381.

'Progress', relative to a historically grounded triad, does not entail perfunctory, hence meaningless, judgements of excellence.

It is now clear that the concepts of 'progress' and 'success', although relative notions, are meaningful. But there is one final question which needs to be answered. Namely, it needs to be seen whether historical relativism, with respect to the notions of 'success' and 'progress', enables Laudan to still talk about 'success in science', in such broad terms. Throughout this thesis I have emphasized that one of the main motivations for the naturalistic turn is the success of science. Can Laudan, for instance, still justify the naturalistic turn by pointing to the success of science, if 'success' is a relativized notion? According to Laudan, this is no problem at all:

All this sounds rather "whiggish," and so it should, for when we ask whether science has progressed we are typically asking whether the diachronic development of science has furthered cognitive ends that we deem to be worthy or desirable. Great scientists of the past need not have shared our aims in order for us to ascertain whether their theory choices furthered our cognitive aspirations. For these reasons, a recognition of the fact that aims and values both change does nothing to preclude our use of a robust notion of cognitive scientific progress.³³

Worrall may be right that, if we accept Laudan's position, we will have no "basis for the judgement that the empirical sciences have become increasingly *sophisticated* as opposed to degenerately baroque."³⁴ But this is a consequence of Laudan's position that is he is not unaware of, or apparently unhappy with. In fact, it seems that it would only be problematic for Laudan if he thought of 'success' or 'progress' in normative terms. But, as we already know, he doesn't; to Laudan, these

³³ Laudan, 1984, p.65.

³⁴ Worrall, 1989, p.381.

are non-normative terms, which state an empirical assertion concerning the efficiency of means/ends. how efficient means/ends are. As Laudan puts it, "The thesis that science is successful (or unsuccessful) amounts to the empirical assertion that the actions of scientists have in fact brought about or otherwise promoted (or failed to promote) certain goals or aims."³⁵ Thus, he can consistently (continue to) talk about the success of science.

It is now evident that none of the arguments presented in this chapter raise insuperable problems for Laudan's normative naturalism. It has been clearly established that Laudan's descriptive axiology, i.e. his view regarding aim variance in the history of science, is not at odds with his reticulated model of scientific rationality. Now, it is time to turn to the final chapter of this thesis, where I determine to what extent Laudan's normative naturalism is, in fact, 'normative'.

³⁵ Laudan, 1984(b), p.87.

Chapter Five

The main thrust of criticism in the last chapter was that Laudan's descriptive axiology, i.e. his view on aim variance in the history of science, needs to be revised in light of his reticulated model of scientific rationality. We concluded there that Laudan's reticulated model of scientific rationality in no way demands the existence of a transhistorical goal of science, and in no way entails a transhistorical notion of progress.

In this chapter I investigate a final criticism of Laudan's axiology. This criticism, due primarily to Robert Shaver, charges that Laudan's reticulated model of scientific rationality cannot satisfy the demands of strong normativity.¹ I begin this chapter by reviewing two standard definitions of normativity: Strong normativity and weak normativity. Then, I will examine Shaver's criticism. After that, I try to answer whether, in general, a naturalized epistemology needs to be normative in one or the other way. Then I turn to the particular case of Laudan's normative naturalism; we will see that Laudan is committed to strong normativity. Once this is established, I will determine whether Laudan can fulfill the demands set by a strong normative criterion.

To begin with, then, we need to familiarize ourselves with the concept of 'normativity'. I mentioned in chapter one that a naturalized epistemology, if it is to fulfill its normative role, needs to tell us how we ought to act in a given situation. Though perhaps an initial step towards an answer, this criterion of normativity is too vague to be of any help. Everybody agrees that for a proposition to be normative it has to tell us how to act; normativity *means* providing guidance. What we need to

¹ Robert Shaver, private correspondence, 1996.

know is when does a proposition tell us 'how we ought to act'? How are we to interpret this claim? In other words, when, exactly, can we say of a proposition that it provides guidance? This is the tricky part: Getting some consensus on what 'guidance' consists in.

In today's philosophical discussions, there are two prominent definitions of normativity: Strong normativity and weak normativity. Put simply, the proponents of strong normativity say that for a proposition to be normative it has to express a categorical imperative of the sort 'you ought to do A'. It is important to note that once one has categorical normativity, hypothetical imperatives can provide guidance; categorical guidance is a necessary condition for hypothetical guidance.

The proponents of weak normativity, on the other hand, say that for a proposition to be normative it has to express a hypothetical imperative of the sort 'if your goal is B, and A is the best means to B, then you ought to do A'. Put plainly, with this account 'guidance' is cashed out in instrumental terms, as action taken relative to some goal.

To understand the attraction of strong normativity take this case, for instance: Imagine that person (P) has the goal A. Through her research, P came to the conclusion that B was the best means to achieving A, and consequently set her sights on working at B. However, it was later discovered that through professional sloppiness (and *not* social, economic factors, etc...) P got it wrong. That in fact C and not B was the best means to A. According to instrumental rationality (i.e. weak normativity), the appropriate judgement here is that in acting upon B, P acted irrationally. Conversely, still following instrumental rationality, P was obliged to act upon C. The proponents of strong normativity point out that what instrumental rationality ignores is the rationality or irrationality of the goal, namely A.

What if, say the advocates of strong normativity, A were 'build a nuclear bomb'. Following weak normativity we are forced to call P irrational since she failed to take the most effective route to this, a morally heinous goal. The point is perhaps even clearer in a case that does not smack of moral repugnancy; say, for example, A is 'find a cure for the common cold'. It still seems funny, according to the proponents of strong normativity, to call P irrational because she failed to take the best road to this goal. Bluntly put, the advocate of strong normativity says that unless we can say that P ought to have gone for A (build a nuclear bomb, find a cure for the common cold), it makes no sense to say that taking the means B was irrational, or that P ought to have taken C.

The proponents of weak normativity may agree that, as it is construed above, it does seem peculiar to say P ought to have done C. But only, they will say, if we have already presupposed a strong sense of normativity. If instead we think of rationality instrumentally, we can say - and there would be nothing funny about it - 'P ought to have done C, and was irrational to act on B, *since* A was P's goal'. The advocates of instrumental rationality suggest that the above case, when seen through the eyes of weak normativity, makes perfect sense. Moreover to see it this way we need only to adjust the meaning of rationality so that it never translates as anything but 'the best means to ends'.

Now that we have a good understanding of what sort of propositions may count as normative propositions, we can elaborate on Shaver's criticism. The objection he raises, simply put, is that the only valid normativity is strong normativity, and, he argues, Laudan's normative naturalism is an instance of weak normativity; hence, he concludes, Laudan's normative naturalism is not normative, after all. To respond to this

criticism, we need to see what the normative demands are on naturalized epistemology in general, and determine if Laudan's normative naturalism is in fact a version of weak normativity; I will answer those two questions shortly. First, I want to examine Shaver's justification for claiming that strong normativity is the only acceptable form of normativity.

Shaver's justification for ruling out weak normativity as a viable form of normativity comes in an appeal to common sensical, fundamental presuppositions; he does not actually present an argument, at least not one that would convince someone who is not already on the side of strong normativity. Basically, he claims that if there is no reason why one ought to pursue this end *x* and not that end *y*, then the fact that, say, *z* is the best way to achieve *x* is a fact which has simply no normative force. Shaver's criticism says that hypothetical (i.e. weak) normativity does not count as 'guidance'. The conclusion he draws is that Laudan's view has to be able to give categorical normativity with respect to our cognitive ends. Yet, Shaver argues, Laudan's meta-methodology is *not* capable of strong normativity with respect to our cognitive aims.² Hence, he concludes, Laudan's normative naturalism is not 'normative' after all.

The challenge this objection raises to Laudan's normative naturalism is great and cuts to the heart of his meta-methodology. It forces us to speculate on the very plausibility of an instrumental theory of rationality. And it goes one step beyond merely questioning the legitimacy of a philosophy of science which is only concerned with the proper means to more or less arbitrary ends. It states that to have any bite, normativity *must* be categorical.

Our first task in responding to this criticism is to determine whether, in

² This argument was elaborated in a series of private correspondence. I have extrapolated from those conversations, and I take full responsibility for any misconstruals of Shaver's position.

general, an epistemology of science demands the sense of normativity presupposed by Shaver, namely that guidance about ends is a necessary condition to obtaining guidance about means, i.e. strong normativity. Or, whether a weak normativity can satisfy the standards of an epistemology of science. Specifically, we now need to know if there is any reason, any good argument to convince us that a normative naturalized epistemology *has* to be normative in specifically one or the other sense.

Unfortunately, there are no such good arguments. The most obvious arguments for either side are (as the case on p. 56 above foreshadowed), inevitably circular. For instance, the supporter of strong normativity will say - categorically - that a naturalized epistemology demands strong normativity. On the other hand, the champion of weak normativity will say - hypothetically - that if we want a naturalized epistemology to be normative, then we ought to adopt weak normativity. In other words, any proposition that tells us which propositions count as normative, must (to have any normative force) itself be that form of proposition.

In chapter three we came face to face with the inescapable futility of circle arguments: Though not without some force, circle arguments are utterly unconvincing to anyone unwilling to step into the circle. Certainly, I have not shown that there are no other, non-question begging responses to the question at hand. However, in the first place, Shaver has yet to offer one; furthermore, in the end that sort of inquiry may be misdirected. At least, for the scope of this chapter, it seems less important to solve the dilemma over strong or weak normativity in the general case of all naturalized epistemologies than it is to figure out if Laudan's normative naturalism is implicitly committed to one or the other position.

Thus, we will confine our search for an answer regarding the

normative requirements on a naturalized epistemology to a specific meta-methodology, i.e. Laudan's normative naturalism. To start with, we need to remind ourselves how normativity is supposed to fit within Laudan's system. According to Laudan, his normative naturalism is a species of instrumental rationality (or means/ends connections);³ as such, it is normative *only* hypothetically; that is, it guides us to take the best means to achieve our end *x*, given that *x* is in fact our end. What Laudan does not emphasize often enough is that this account, clearly an instance of weak normativity, is specifically about the *methodological* aspect of his normative naturalism. And, as we are well aware, methodology is only a part of Laudan's normative naturalism, and a rather insignificant part compared to axiology. Certainly it would be faulty to infer that since his account of methodological rules is a case of weak normativity, then his normative naturalism, on the whole, is a version of weak normativity.

Quite the contrary: Laudan's normative affinities go beyond mere weak normativity. A purely instrumental rationality is concerned only with the proper means-to-ends, and is mute on the subject of ends-in-themselves. But, of course, we know that Laudan is not silent on the subject of ends. He says a lot about normativity with respect to aims. Importantly, he says that some ends are not rational, specifically those ends which are not realizable. Implicit in Laudan's views about rational constriction of aims is that certain means/ends connections provide no prescriptive value. Thus he claims that "before a purposive action can qualify as rational, its central aims must be scrutinized."⁴ Statements like this, which smack of strong normativity, are scattered throughout his writings, and they are not at all ambiguous: Laudan is claiming if an end is not rational, then telling us

³ Laudan, 1990(b), p.316.

⁴ Laudan, 1984, p.64.

how to get from means-to-ends does not provide any guidance, i.e. it is not normative.

Therefore, in Laudan's meta-methodology normativity with respect to ends is a necessary condition for normativity with respect to means. This is an unavoidable consequence of Laudan's reticulated model of scientific rationality. Doppelt sums up this point nicely. He says "Laudan in effect must grant that scientific epistemology needs to establish the rationality of scientific aims," and "without that, showing that a methodological rule is an effective means to realizing a given scientific aim will not establish the rationality of accepting the rule."⁵ Although Doppelt draws a different conclusion,⁶ his comments fit well here; they confirm the thesis that weak normativity is *not* the only normativity at work in Laudan's normative naturalism (although there is weak normativity with respect to methodological rules). In point of fact, in Laudan's meta-methodology every use of a hypothetical imperative is contingent on normativity about ends.

Before going on, it is important to clarify why normativity about ends translates into strong normativity. This is important because guidance about ends could conceivably be hypothetical. It could be Laudan's view, for instance, that particular methodological rules are contingent on particular ends, and those ends (in turn) are contingent on other ends. The problem with this picture is not hard to see. The above suggestion could only possibly work in one of two ways, and in the end both are untenable. One way is if there was a single end of science in reference to which all other ends were hypothetically justified. But, first of all, we saw in both

⁵ Doppelt, 1987, p.231.

⁶ Namely, as was discussed in chapter two, the (in)ability of a naturalized epistemology to account for a rational evaluation of aims.

chapters one and two that Laudan thinks the idea of 'one end of all scientific inquiry' plainly false. Furthermore, even if there existed that 'one end of all scientific inquiry' it would somehow have to be justified, and since it is the 'end of all ends' it could only be justified categorically. The other possibility is that each particular end is justified hypothetically in reference to again some other particular end. This picture is as unattractive as the first, however, as it sets up Laudan's position to a typical infinite regress. And, the response to that is plainly typical as well: Either the position is suspect because it leads to a regress, or the regress is stopped by none other than a categorical imperative. Thus, once Laudan admits that the consequent of a hypothetical imperative itself needs justification he has committed himself, inevitably, one way or another, to strong normativity.

It is now clear that Laudan is committed to strong normativity.⁷ Thus, Shaver's claim that Laudan's normative naturalism is an instance of weak normativity is off the mark. But his criticism has not yet been fully answered. We now need to see if Laudan's strong normativity with respect to aims is *strong enough* to satisfy *all* the demands set by a strong normative criterion.

Strong normativity, recall, states that we cannot have normativity in the absence of categorical imperative propositions of the sort 'you ought to do x'. For our purposes, the most important things to note about this characterization of strong normativity is that justification about ends does

⁷ It should also be clear is that this adherence *necessarily* mirrors normative commitments in science, at least if the judgement that Laudan's account of the rational evaluation of aims is naturalistic is correct. To understand this one only needs to recall the conclusion reached in chapter two, namely that Laudan's account is naturalistic in virtue of it imitating science. Justification of aims (i.e. the realizability criterion) is demanded by Laudan's normative naturalism just because this very sort of aim justification exists in science. And since it is aim justification (i.e. the reticulated model of scientific rationality) that betrays Laudan's implicit dependence on strong normativity, that same aim justification in science betrays a commitment there to strong normativity.

not necessarily entail propositions of this kind. This is important because, as we saw in the last chapter, in Laudan's meta-methodology lots of aims are going to be justified, namely all those that are realizable. Laudan's naturalistic criterion of aim change can limit the choices of aims, but simply put, the reticulated model does not uniquely pick out aims. Thus, if strong normativity demands categorical propositions of the sort mentioned above, then Laudan's axiology will fall short.

But does strong normativity require categorical propositions of the sort 'you ought to do x'? Perhaps not. Although earlier (pp.69-70) this is how strong normative propositions were formulated,⁸ conceivably, there are at least two forms of categorical propositions: Those that express an obligatory act, and those that express a permissible act, where in both cases the act in question is a justified one. The kind of categorical proposition we have seen thus far is of the obligatory sort; it states that 'you ought to do x'. But it is certainly possible that a categorical proposition states only a permissible act, for instance 'you can do x'. In both these cases, the declarative is categorical, and in both these cases there is a justified act; only in one instance we are categorically obliging that act, and in the other we are categorically declaring its permissibility.

To be clear, notice that in neither case, is strong normativity being reduced to 'weak normativity' (as I've defined it), i.e. hypothetical normativity. In fact, this very distinction is paralleled in the case of weak normativity. For instance, a hypothetical proposition which expresses a permissible act would state, 'if your goal is x, and y is the best means to x, then you can act on y'. This is opposed to an obligatory hypothetical proposition, which would state, 'if your goal is x, and y is the best means to

⁸ Predominantly, because this is the more recognizable form of categorical propositions.

x , then you ought to act on y .

Once the distinction between permissible and obligatory categorical propositions is elaborated, it becomes clear that Laudan's axiology is able to satisfy the demands of strong normativity. Although some proponents of strong normativity may hope for a standard higher than permissibility, permissibility is nevertheless a solid enough foundation for a hardy, meaningful sense of strong normativity. It is now evident that obligatory categorical propositions are not necessary; hence, it no longer seems problematic that Laudan's account is unable to uniquely pick out aims.

This response has two merits: First, as we have seen, strong normativity, conceived of as categorical imperatives expressing permissible acts, manages to restore a perfectly robust sense of strong normativity to Laudan's axiology. Second, this kind of strong normativity is not just consistent with Laudan's reticulated model of scientific rationality, but it is one which he obviously endorses. Laudan is quite aware that (as we saw in chapter four) "a wide range of cognitive goals or values can satisfy the demands [i.e. realizability] laid down here."⁹ He readily admits that the preponderance of justified aims in his system may cause some critics to wonder "'how does the reticulational analysis tell us which among the surviving goals is the right one?'"¹⁰ However, in Laudan's opinion, there is no answer to this question, because it rests on an illegitimate assumption; namely, that there is (or should be) a single, unique aim which motivates action. Rather, he claims, "there is no single 'right' goal for inquiry because it is evidently legitimate to engage in inquiry for a wide variety of reasons and with a wide variety of purposes."¹¹

⁹ Laudan, 1984, p.63.

¹⁰ Laudan, 1984, p.63.

¹¹ Laudan, 1984, pp. 63-64.

We are now in a position to conclude that Laudan's normative naturalism is, in fact, 'normative' enough. There is weak normativity only in the case of methodological rules, which hypothetically guide action with respect to cognitive ends. Significantly, in Laudan's meta-methodology there is a robust sense of strong normativity in the case of aims: Cognitive ends are rationally evaluated, following the naturalistic criterion of aim change, and they supply us with a form of categorical guidance which states that is permissible to act towards any one (or more) of a number of justified aims.

Conclusion

Throughout the course of this thesis, my intention has been to show that Larry Laudan's normative naturalism can achieve success as an epistemology of science. I hope to have done just that. Although his meta-methodology is the target of much criticism, we have seen that none of the major objections raised presents an insurmountable hurdle for Laudan's position.

The main criticism in chapter two, namely that aim variance in the history of science led to the problem of relativization, was easily overcome. We saw there that methodological contingency on variant aims does not detract from the universality of the principle of instrumental rationality: Whatever your end, adopt whatever method which will best promote the attainment of that end. A preliminary conclusion reached in this chapter was that Laudan's normative naturalism is a species of instrumental rationality; in his system, methodological guidance is only hypothetical, and actions only rational if they are suited to attaining our goals in light of our beliefs about the world.

In chapter three we looked more closely at Laudan's account of aim change, as captured by his reticulated model of scientific rationality. First, we saw how critical Laudan's axiology is to his normative naturalism - that "methodology gets nowhere without *justified* aims". This examination set the stage for the main objection in this chapter, namely, that Laudan's main criterion guiding rational aim change, i.e. the utopianism of aims, is non-naturalistic. After citing two definitions of naturalism, the conclusion arrived at was that Laudan's account is indeed naturalistic in *both* senses: It is as naturalistic as science in one sense (i.e. 'copy science'), and even

more naturalistic than science in the other sense (i.e. empirical testability).

In chapter four the main thrust of criticism was that Laudan's descriptive axiology, i.e. his view on aim variance in the history of science, needs to be revised in light of his reticulated model of scientific rationality. However, once Laudan's account of both piecemeal change within a historical triad, and his notion of progress as relative progress are considered, it becomes clear that his reticulated model of scientific rationality in no way demands the existence of a transhistorical goal of science, and in no way entails a transhistorical notion of progress.

In the fifth chapter of this thesis I responded to the final obstacle facing Laudan's normative naturalism: Is the reticulated model of scientific rationality capable of meeting the demands of a strong normative criterion? We saw that there that once a distinction between two forms of categorical propositions was elaborated, Laudan's axiology gives us a perfectly robust sense of strong normativity.

Laudan's normative naturalism is a successful meta-methodology. It is capable of doing all that we would expect from an epistemology of science. It has a naturalistic criterion which enables us to rationally evaluate cognitive aims, and it instructs us, once we have those aims, to follow the methodological principle of instrumental rationality for most efficient means/ends realizations. As Laudan states, this is all we can ask from any meta-methodology:

But beyond demanding that our cognitive goals must reflect our best beliefs about what is and what is not possible, that our methods must stand in an appropriate relation to our goals, and that our implicit and explicit values must be synchronized, there is little more that the theory of rationality can demand.¹

The prospects for normative naturalism are good.

¹ Laudan, 1984, p.64.

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