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Cognitive values, theory choice, and pluralism: On the grounds and implications of philosophical diversity

Axtell, Guy Stanwood, Ph.D.
University of Hawaii, 1991

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COGNITIVE VALUES, THEORY CHOICE, AND PLURALISM:

ON THE GROUNDS AND IMPLICATIONS OF PHILOSOPHICAL DIVERSITY

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ABSTRACT

This dissertation focuses on the development of a pragmatic account of normative discourse. The approach taken to the subject of study is metaphilosophical. Prevalent contemporary treatments of norm governance and cognitive evaluation, are examined in light of background metaphilosophical views adopted by such schools as logical empiricism, pragmatism, and schools associated with contemporary sociology of scientific knowledge. I examine metaphilosophical assumptions that pre-structure treatment of conceptual issues such as belief-modification, theory choice, and explanation, pursuing these issues across a wide range of disciplines including ethics, jurisprudence, and philosophy of science. The pragmatist account of ‘inquiry as essentially normative’ is developed, and utilized as grounds for a double-edged criticism of both contemporary objectivist and relativist treatment of these conceptual issues. This pragmatist account differs substantially in its conception of the relationship between facts and values --or the natural and the normative dimensions of meaning-- from both logical empiricist and radical historicist accounts. Explicating these significant differences, I argue, leads towards confronting objectivism and relativism on the ‘higher level’ of values and meanings on which their polarity is motivated. The pragmatic conception of normative discourse that I develop as grounds for reconciliation, accounts for philosophical diversity by way of the possible variability in valuative orientations taken towards explanation of human decisions and discourse. Yet the methodological pluralism of the pragmatist school, I conclude, instantiates a philosophy of explanation in the human sciences that differs substantially from those that noted recent proponents of objectivism and radical historicism have embraced.
## CONTENTS

<table>
<thead>
<tr>
<th>ABSTRACT</th>
<th>iv</th>
</tr>
</thead>
<tbody>
<tr>
<td>PREFACE</td>
<td>vi</td>
</tr>
<tr>
<td>CHAPTER ONE</td>
<td>BELIEF AND ATTITUDE</td>
</tr>
<tr>
<td>1.1 Values and the analytic/synthetic distinction</td>
<td>4</td>
</tr>
<tr>
<td>1.2 From Hume to Stevenson on reason and logical connections</td>
<td>16</td>
</tr>
<tr>
<td>1.3 Intrinsic values as unconditioned interests</td>
<td>30</td>
</tr>
<tr>
<td>CHAPTER TWO</td>
<td>THE DEVALUATION OF VALUES</td>
</tr>
<tr>
<td>2.1 The norms of thought and action</td>
<td>41</td>
</tr>
<tr>
<td>2.2 ‘Normative’ science and ‘positive’ ethics</td>
<td>62</td>
</tr>
<tr>
<td>2.3 Instrumentalism and relativism</td>
<td>71</td>
</tr>
<tr>
<td>2.4 Pragmatism and instrumentalism</td>
<td>81</td>
</tr>
<tr>
<td>CHAPTER THREE</td>
<td>LOGICIST METASCIENCE</td>
</tr>
<tr>
<td>3.1 The Carnapian metaphilosophy</td>
<td>92</td>
</tr>
<tr>
<td>3.2 After syntax: the logicist legacy</td>
<td>98</td>
</tr>
<tr>
<td>3.3 A dogma of contexts: using and choosing a standard</td>
<td>103</td>
</tr>
<tr>
<td>3.4 Pragmatic decisions and rationality</td>
<td>108</td>
</tr>
<tr>
<td>CHAPTER FOUR</td>
<td>THE BRANCHES OF THE HISTORICIST SCHOOL</td>
</tr>
<tr>
<td>4.1 Diverse reactions to the received view</td>
<td>126</td>
</tr>
<tr>
<td>4.2 Science as social practice: the historicist’s challenge</td>
<td>132</td>
</tr>
<tr>
<td>4.3 The myth of the framework: Popper’s challenge</td>
<td>138</td>
</tr>
<tr>
<td>4.4 Lakatos’ objectivism and Popper’s ‘third world’</td>
<td>153</td>
</tr>
<tr>
<td>4.5 Lakatos’ historiography and the demarcation problem</td>
<td>159</td>
</tr>
<tr>
<td>4.6 Intuitionism and the ‘scientific elite’</td>
<td>163</td>
</tr>
<tr>
<td>CHAPTER FIVE</td>
<td>RECENT OBJECTIVE HISTORICISM</td>
</tr>
<tr>
<td>5.1 The rational, the social, and the normative</td>
<td>174</td>
</tr>
<tr>
<td>5.2 Laudan’s normative naturalism</td>
<td>179</td>
</tr>
<tr>
<td>5.3 The causal dualist thesis</td>
<td>192</td>
</tr>
<tr>
<td>5.4 The explanatory asymmetry and primacy theses</td>
<td>200</td>
</tr>
<tr>
<td>CHAPTER SIX</td>
<td>EXPLANATION IN THE HUMAN SCIENCES</td>
</tr>
<tr>
<td>6.1 Towards a pancritical non-foundationalist metaphilosophy</td>
<td>216</td>
</tr>
<tr>
<td>6.2 Pragmatism, logic, and learning</td>
<td>220</td>
</tr>
<tr>
<td>6.3 The explanation of normative decisions reconsidered</td>
<td>233</td>
</tr>
<tr>
<td>6.4 The human reasoner and the sciences of man</td>
<td>246</td>
</tr>
<tr>
<td>6.5 Competence and performance in norm governance</td>
<td>252</td>
</tr>
<tr>
<td>BIBLIOGRAPHY</td>
<td>263</td>
</tr>
</tbody>
</table>

v
The normative dimensions of knowledge and social practice have received attention in the past few years from philosophers and social scientists alike. Whenever one mentions a "norm" or "standard," one cannot help but note the multiple senses which common language associates with such terms. One may attempt to describe processes of change or alteration of norms in human practices, or one may attempt to say what constitutes improvement in norms; these projects undoubtedly are not the same, as common language attests. One may speak of norms and standards as actually existing regularities and dispositional attitudes taken by human individual or groups, or one can speak of "value itself," what the "good" is, what one "ought" to do, etc. Philosophers have typically divided inquiry into two, a division reflected in the problem commonly referred to as the Is/Ought problem, or the problem of the relationship between the "natural" and the "normative" dimensions of thought and meaning.

But there are philosophical reasons as well why one must resist letting the theoretical differentiation of these senses of meaning become hardened into either an epistemological or a metaphilosophical dualism. This dissertation is an examination into the relationship of the natural and the normative dimensions of thought and meaning. Its subject is what Frederick Will has recently called "norm governance," where governance is a concept that encompasses aspects both of the empirical study of change or alteration of normative systems, as well as philosophical grounds for criteria of criticism and improvement of normative systems. Perhaps what is most distinctive about
my approach is my treatment of the intellectual influences on dominant twentieth-century schools of thought on this issue. The polarities of objectivism and relativism, philosophy and sociology, reason and cause, etc. which are so troubling today are in my view deeply conditioned by persistent, unacknowledged assumptions about the relationship of the normative and the natural.

My focus in addressing normative discourse is metaphilosophical. The conceptual issues that come to light stretch widely across ethics, juridical theory, and philosophy of science. Treatments of issues like conceptual change, methodology, and theory appraisal are typically pre-structured by valuative judgments; getting beyond the polarities mentioned therefore requires confronting such oppositions directly on this 'higher order' of valuative meanings and discourse on which they are motivated.

One example of the pre-structuring that will be examined here is the dominant metaphilosophical orientation which I call selective foundationalism, and which pre-structures much of the epistemological and ethical theory twentieth-century logical empiricists have developed. Focusing on this metaphilosophical orientation is the first step towards reconstructing the complex relationship between the logical empiricist's epistemological objectivism and their ethical "non-cognitivism"; it is here that one finds the intellectual motivations for the quite disparate and dualistic attitudes they took towards norm governance inside and outside of science.

The path of intellectual influence I trace is from early Modern sources to the highly influential Carnapian/Reichenbachian metaphilosophy, and from there into both the "objectivist" (Lakatos/Laudan) and "contingentist" (Kuhn, Barnes, Bloor, Rorty) branches of historicist thought. One assumption I trace, for instance, is the Carnapian division of questions into the "internal" and the "external," each requiring separate and disparately
conceived criteria. This is echoed in the sharp divisions of internal and external history, rational reason and social cause, etc., which is a tenet of Lakatos/Laudan objectivist historiography (and Mertonian school sociology of science). It is also echoed in the notions of meta-epistemological subjectivity and of the relativity-to-framework of all cognitive claims and standards, notions that one finds profuse examples of in works representative of historicist relativism.

This reconstructive reading of the tradition makes possible a double-edged critique of contemporary objectivism and relativism, one that shows the "cognitive," "ethical" and "institutional" forms of these general attitudes have a common philosophical origin. To argue effectively for a middle ground between objectivism and relativism, we must confront the real and perceived repercussions of such philosophical movements as have been witnessed in the past twenty-five years, particularly what has been termed the "historical turn," and the related rejection of epistemological foundationalism and justificationism. If we recognize that a fragmented conception of reason is our Cartesian inheritance, we may be led to significantly re-construe both the "naturalistic" and the "normative" questions concerning the formation, criticism, and improvement of the norms of social practices. It is for these reasons that the main focus of inquiry will be on the metaphilosophical assumptions underlying what I take to be the two dominant twentieth-century schools of thought on these matters, namely the "logicist" and "historical" schools.

In my last chapters I attempt to draw out some of the conclusions of my double-edged critique, pointing to the need for a reconstruction in metaphilosophy. This is a non-foundationalist yet "pancritical" reconstruction that rejects 1) the comprehensive foundationalism of the Kantian rationalist tradition, 2) the selective foundationalism of
twentieth-century logical empiricism, and 3) the radical anti-foundationalism of historicist relativism. I argue that the pancritical metaphilosophy, as a fourth option, best represents the spirit of Dewey's pragmatism, and leads to concern for developing non-vicious "bootstrapping" methods of warranting of belief in all normative practices. It thereby moves towards resituating norm governance in science into a broader and more varied class of social practices, and by the same token towards mending the fragmented conception of reason that was one consequence of the selective foundationalist metaphilosophy. The methodological pluralism I defend in my concluding chapter is not the same as framework relativism. My version of pragmatism about norm governance differs substantially from the 'criterionless muddling-through' Rorty (though not Dewey!) finds apt to describe the consequences of pragmatism.

This research area has been of long-standing interest for me. Yet it is only in the last several years that a more serious effort has been made in the literature to deal with a metaphilosophy of human cognitive interests. While the development of my chapters preceded my discovery of some recent works which bear striking affinities with my approach, I have attempted in my final revisions to situate myself in relation to a number of these contemporary figures. Frederick Will's (1988) Beyond Deduction: Ampliative Aspects of Philosophical Reflection, Steven Fuller's (1989) Philosophy of Science and its Discontents, and Stephen Stich's (1990) The Fragmentation of Reason: Preface to a Pragmatic Theory of Cognitive Evaluation are among the most important of these recent contributions. It should also be noted, of course, that I owe a great philosophical debt to Larry Laudan for his extensive writings in the area of science and values, and a great deal of intellectual debt and gratitude to my chairperson, Lenn Goodman, and to all my committee members for their patient and thoughtful criticisms of earlier drafts.
CHAPTER 1

BELIEF AND ATTITUDE

Introduction

Meta-ethics in the twentieth-century has felt the effects of a general skeptical challenge to the grounds for sound meta-logical standards, as testified by the difficulties of finding criteria to adjudicate among competing moral principles. This skeptical problem came to be viewed as acute when the foundationalist\(^1\) and realist assumptions of the turn of the century moral "intuitionists" came to be questioned in the light of what Reichenbach called 'the rise of scientific philosophy.'

But the issue of philosophical substance may be why, for scientifically-oriented philosophers like the logical empiricists in the 1930s, 1940s and 1950s, this skeptical challenge was treated as being confined to such "non-scientific" fields as ethics, aesthetics,

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\(^1\) "Foundationalism holds that one's belief \(p\) is justified just in case \(p\) is either (a) foundational (i.e., noninferentially justified or self-justifying) or (b) based on the appropriate kind of inference from foundational beliefs." -David Brink (1989) *Moral Realism and the Foundations of Ethics*, p. 101. Of course, the foundation of all knowledge for empiricists is the senses, so that appeal to observations is the foundation for logical empiricist epistemology.
politics, social policy, etc.² For it is in the metaphilosophical policy of containment of the skeptical challenge, and of a foundationalism for scientific knowledge contrasted with anti-foundationalism in ethical philosophy, that I believe we discover the crux of the contemporary antithesis between epistemological objectivism and relativism.

My discussion of philosophical motivations underlying the logical empiricist's adoption of a noncognitivist account of value judgment can be separated into three sections, in each of which I will examine how a particular eighteenth-century view came to influence discussion of belief and attitude in the twentieth century. Each has had significant impact on the conceptions of norm governance in a wide range of disciplines. These are assumptions I parenthetically characterize as influences upon logical empiricist understanding of valuative "judgment," "knowledge," and "meaning," respectively. They are assumptions that tended to be exploited by logical empiricists, and lent support for their ethical noncognitivism at the time when the logical empiricist program was reaching its highest degree of support among scientists and philosophers.³ I focus at several points on G.E. Moore's debate with the noncognitivists in order to draw out these shared assumptions.

The first influential view is the interpretation of meaning through the model provided by analytic/synthetic distinction. These interpretations by Kant and, at the beginning of our own century, by G.E. Moore, drew criticism from the positivists, who although also committed to

² "In part under the influence of then dominant metaphysical, epistemological, and semantic assumptions, moral philosophy in the 1930s, 1940s, and 1950s became highly critical of intuitionism and assumed an antirealist, noncognitivist form." D. Brink (1989) Moral Realism and the Foundations of Ethics, p. 3.

³ Intuitionism was elucidated in the early twentieth-century by writers like, Moore and Ross, and before them by Sidgwick. These men generally accepted 1) Moral realism, the claim that there exist moral facts whose nature and existence are independent of our moral thinking; 2) Foundationalist epistemology, according to which our moral knowledge is ultimately based on self-evident moral truths; and 3) Non-naturalism, the view that moral facts and properties are sui generis, metaphysically independent of and irreducible to natural facts and properties (Brink 1989 p. 3).
analyzing knowledge claims via this distinction, rejected the notion of synthetic a priori
knowledge and effected a return to a more Humean conception of synthetic knowledge (as
descriptive, "matter of fact" knowledge). The perceived importance of the analytic/synthetic
distinction together with the linguistic categories and analytic methods associated with it, led
for many respected thinkers to a skepticism concerning metaphysics, generally regarded by
Vienna Circle members as inclusive of claims about a valuative form of knowledge.

The second influential view is the deductive characterization Hume gives to the idea of
reasoned connectedness. This characterization has the effect of restricting a "reasoned"
connection to a formalizable relationship between premises and conclusion. Most important
here is the associated division in Modernist tradition between "logical" and "psychological"
processes, as well as the separate explanatory conditions each type of process was thought to
entail. An explicit part of his empiricism, these views led Hume to reason that if value
judgments are not derivable deductively as a relation of ideas, then attitudes are only
"causally" (psychologically/habitually) related to beliefs. This view directly conditioned
discussions of ethical "knowledge" and the prospects for a "science of ethics." Hume’s moral
skepticism can be seen in this way as intimately connected with his own deductivist
assumptions, combined with an honest denial that there can be legitimation or reasonableness
in ethics when these assumptions are granted. The psychological glossing Hume gives to non-
deductive inference leads to a persuasive contrast of reason and the passions (or reason and
habit), a contrast upon which twentieth century emotivism would develop.

The third influence I will focus on is Kant’s treatment of the distinction between
conditioned (or extrinsic), and unconditioned (or intrinsic) judgments of value. Although the
logical empiricists took a radical turn away from Kant in the understanding of ‘intrinsic’ value

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4 Cf. Stevenson Facts and Values, p. 170
claims, claiming them to be cognitively "non-sensical," this distinction was accepted in important ways as the basis for their linguistic analysis of the 'language of morals.' This acceptance constituted more than a dialectical concession on the part of the logical empiricists to their moral realist and intuitionist adversaries. Rather it marks a crucial shared assumption in the debate between Moore and his emotivist critics, one with a reverberating effect not only on meta-ethics, but on metascience as well. As I develop it further in Chapter Two, this set of assumptions conditioned the rise and character of instrumentalist conceptions of reason or rationality.

1.1 Values and the analytic/synthetic distinction

Ethicists at the turn of the century continued to grapple with the enticing idea of a "science of ethics," both in the Kantian (deontological/a priori synthetic) sense and in the very different sense of a utilitarian naturalism. Kant, like Hume, had applied his central epistemological distinctions to value judgments in order to clarify their status. For Kant, the predicate of a judgment such as "X is good/right" is not contained in the subject, "X"; Hence, seeing it as a statement, it is informative. In calling such statements synthetic, Kant's definition implies that the denial of the judgment implies no formal contradiction.

That moral judgments are expressible in the imperative mode did not disrupt this synthetic characterization, since Kant claimed to be able to identify a class of imperatives expressing moral principles with absolute, categorical necessity. The natural expression of the moral law in imperative form was consistent with its status as a truth claim when expressed declaratively. It is the recognition that moral imperatives have knowledge behind them that separates following rationally (as a matter of duty) from following blindly. No experience
can explain such a law with its absolute necessity, implying that it must have its basis in truths known a priori. The moral law "is conceived as objectively necessary, only because it holds for everybody that has reason and will." Moral laws for Kant issue a priori from man's practical reason and are self-evidently apprehended by rational intuition. "Since moral laws ought to hold good for every rational creature, we must derive them from the general concept of a rational being." 

Now the characterization of "X is good" as synthetic is a formal one and pertains to declarative claims about both what Kant called intrinsic and extrinsic goods. In this sense all value judgments were generically characterizable as synthetic; it is the a priori character of moral knowledge that provides Kant with a standard for demarcating moral from non-moral content within value judgments. For Kant the a priori character of moral laws meant they were independent of interests. Non-moral ones were a posteriori, depending upon "inclinations" --given psychological interests or desires. 

G. E. Moore likewise applied the analytic/synthetic distinction to ethical judgments in *Principia Ethica* (1903). Like Kant he concluded that they had to be synthetic, and made this characterization of them explicit:

> To readers familiar with philosophic terminology, I can express their [Moore's own arguments'] importance by saying that they amount to this: That propositions about the good are all of them synthetic and never analytic; and that is plainly no trivial matter.

5 *Critique of Practical Reason*, i. i. 8 (v. 36; Abbott, p. 126).


7 Moore comments on this himself in *Principia*: "It has been commonly held, since Kant, that 'goodness' has the same relation to Will or Feeling, which 'truth' or 'reality' has to Cognition: that the proper method for Ethics is to discover what is implied in Will or Feeling, just as, according to Kant, the proper method for Metaphysics was to discover what is implied in Cognition." -p. xx.

8 *Principia Ethica*, pp. 6-7. Discussed in Kuklick, p. 93.

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An ethics which would make this knowledge analytic seems capable only of explicating meanings and not of supplying directives. The naturalists—at least the utilitarians and other consequentialists Moore took as his target—understood "X is good" in a way that appears to render it analytic. But this, in part, is the fallacy which the naturalists were said to commit. For Moore this fallacy was made evident by their failure on the "open question test." Moore's test depended heavily on the analytic/synthetic distinction and its associated accounts of definition. It is because he like Kant held that moral judgments are synthetic, that he insisted it remains an open question whether "A is good," means the same as "A has natural properties X, Y, and Z." If synthetic, then on Kantian grounds the denial involves no contradiction. It always remains open to deny this relation because the relation is not one of synonomy.

Moore's account of the "naturalistic fallacy" is much broader than this of course, but both Moore and his critics were disposed to understand definition through the analytic/synthetic distinction. If a synthetic statement is one in which the predicate is not contained in the subject, then this should be the case also for the predication of "good." The conclusion "X is good" depends upon, but is not deducible from a set of empirical premises.

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9 Of course there are a variety of naturalistic theories, very few of which are strictly descriptivist.

10 One plausible interpretation of Moore's open question test is as follows:
   D: "X is good" means "X has the Property P."

Then, we formulate two questions as follows:
   A: X has P, but is it good?
   B: X has P, but does it have P?

Now, the open-question argument is simply this:
   If D is correct, then A and B have the same meaning.
   But A and B do not have the same meaning.

Therefore, D is not correct.

This is why Moore emphasized the difference between the "open" but valid form of ethical inference in

1) A has qualities X, Y, and Z; therefore A is good

and the presumptively fallacious form,

2) A is good; therefore a has properties X, Y, and Z.\footnote{See Olthuis, \textit{Facts, Values and Ethics}, especially Ch. 2. Moore assumes that everyone understands what is meant by "good," and that such understanding constitutes genuine knowledge. For Moore both empirical beliefs about natural qualities and valuative conclusions about the good must be synthetic. But they were nonetheless divided as separate categories, such that to attempt inference from the one class to the other was to commit the naturalistic fallacy.}

In the years following the publication of \textit{Principia Ethica}, Moore's insistence upon the independence of "goodness" from good-making-properties distinguished him from traditional forms of naturalism. His insistence on knowledge about "non-natural properties" furthermore drove a wedge between himself and some other Cambridge philosophers such as Russell, who in \textit{Science and Religion} is seen to be quickly turning away from naturalism and towards a radically noncognitivist view of ethics. Moore might have been willing to embrace the Cambridge epistemology imbedded in Russell's distinction between knowledge by acquaintance and knowledge by description, if only Russell could have accommodated ethical knowledge on this model, as Russell himself had originally hoped to do.

Although conceding that the goodness of an object A "depended" on its having certain natural qualities X, Y and Z, goodness in an important sense had to be separately existing. Moore never gave up this independence thesis, or the metaphysics of the \textit{sui generis} character of ethical truths. This marks an important difference between Moore and many later cognitivists including epiphenomenalists and naturalists about ethical properties, who focus attention on goodness as a "supervenient" property. The very claim that goodness was a "non-natural property," seemed for Moore to entail this (somewhat Platonic) sense of
independence. Moore, in other words, insisted that there was a kind of synthetic "necessity" that characterized true judgments of goodness [(1) above]. But he rejected the attempt to construe this necessity definitionally [(2) above] --whether as an identity of goodness with a specified set of natural properties, or as a specifiable set of "supersensible" properties. The former was the fallacy of utilitarians and consequentialists, the latter the analogous form of the naturalistic fallacy committed by "metaphysical ethics."  

The problems of characterizing such a "necessity" are large however. They are all the more troublesome when one assumes the dichotomy between the analytic and the synthetic as a basis for analysis. The status of definitions such as "A has qualities X, Y and Z" was always problematic on the analytic\synthetic distinction. This no doubt exacerbated the tension in Moore's account between the necessary dependence of goodness on natural good-making-properties, and his notion of moral knowledge as sui generis. Moore does not appear to have viewed the "synthetic" label as interfering with the kind of necessity moral judgments have. But this may be because he was dealing with an essentially Kantian understanding of the distinction. Wittgenstein's influence on empiricism was towards conceiving the only kind

12 See Principia, Chapter IV, esp. Sec. 72.

13 "Historically, intuitionism has been motivated by acceptance of foundationalism and the existence of an is/ought gap. Roughly...because of the existence of the is/ought gap, the inferential justification of a moral belief must always involve another moral belief (the latter belief must be a more general belief under which the former may be subsumed). By foundationalism, however, all justification must terminate in foundational beliefs. Therefore, the justification of any moral belief must terminate in a foundational moral belief." -Brink p. 134.

This however points to a clear difference between coherentist versions of ethical cognitivism and the representational or correspondentist accounts of the intuitionists. "Defenders of a coherence theory of justification in ethics have replied to the charge of intuitionism by pointing out that considered moral beliefs are revisable (e.g. Daniels 1979). But since at least some versions of intuitionism ... treat foundational moral beliefs as revisable (e.g. Ross 1930), the revisability of considered moral beliefs does not establish that a coherence theory of justification in ethics is not intuitionist. Another consideration, however, does show that it is not intuitionist. If considered moral beliefs were the foundational moral beliefs of intuitionism, as the objection alleges, considered moral beliefs would have to be self-justifying. But according to coherentism, considered moral beliefs are not self-justifying; they are justified by their coherence with both moral and nonmoral beliefs." -p. 134.
of necessity as *logical* necessity. But by the time of Wittgenstein's *Tractatus* (1921), the leaning among empiricists was strongly towards a rejection of all kinds of synthetic necessity and a return to Hume as the one who had the distinction right.

The Schilpp volume on Moore's philosophy records the concessions and deep revisions of *Principia* that Moore made over the course of several decades. One of the most significant of these is seen in his attempt to save synthetic necessity by softening both his correspondence notion of truth and the independence of the two kinds of "properties." This softening is not easily compatible with Moore's stronger a priori synthetic view of moral principles, and hints at equivocations in his account of "non-natural properties." 14

Moore's later views put him far closer to more contemporary critics such as Toulmin, Nowell-Smith and Hare, who speak of "good" as a "consequential" or "supervenient" property. 15 Yet the main onslaught against Moore in the early twentieth-century was not by the naturalists who were cognitivists, but by those who agreed with Moore in his opposition to the traditional forms of naturalism, yet drew far different consequences. 16 In an important

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14 Originally Moore had said that a non-natural property was unable to exist "by itself in time" but must rather exist as "a property of some natural object." But in later years Moore further qualified his insistence that what was designated by the "non-natural" label was a property in the full-blooded sense.

15 There is nothing particularly troubling about consequential properties; Nowell-Smith and other later writers allow ways of speaking about "logical oddity" in the "fit" between beliefs and attitudes, and about "implicational meaning" which works as a non-a prioristic synthetic necessity. But for Moore it is quite a large concession, since the explication of the naturalistic fallacy is so closely wrapped up in the *Principia* with the account of definition.

16 Is ethical naturalism a reductive metaphysics, or can it be understood in a non-reductive way? Brink defends a non-reductive form of ethical naturalism, with close affinities to what is usually called descriptivism. "A non-reductive form of ethical naturalism claims that moral facts and properties are constituted by, and so supervene upon (or vary in a lawlike way with), natural and social scientific facts and properties even if moral terms are not definable by natural terms. Moreover, this sort of nonreductive ethical naturalism parallels the sort of nonreductive theory that we accept (or should accept) about a number of other, nonmoral disciplines in the natural and social sciences." (p. 9). Note that Moore's later concessions to naturalism move him closer to this position of non-reductive naturalism, while at least some naturalists have also conceded the falsity of reductive
sense, Moore’s distinction between natural and non-natural properties prefigures the use of the
descriptive/prescriptive meaning distinction by the noncognitivists. Both logical atomists such
as Russell and those who would follow Wittgenstein’s lead were dubious of Moore’s special,
non-sensible kind of intuiting. The notion of a non-sensible mode of apprehension which was
to mirror sensible intuition in many respects, yet which had for its object a sphere of value
properties, fit ill with the escalating empiricist program.

Moreover, while the new methods of linguistic analysis to which Moore appealed were
highly influential, most Wittgensteinians wanted to divorce ethical language from attachment
in Moore’s thought to a strongly referential account of meaning. In particular, they wanted
to expel the notions of a special intuitive faculty and a mysterious non-sensible kind of
fact. Moore’s own polemic against naturalistic definitions of "good" had made it apparent
to the majority of empiricists of his day that goodness could not be a natural property of
things, or a property cognized or confirmed through the senses. But the insight would be
turned back against Moore in the form of argument that what non-sensible intuitions purported
to represent could not be a form of synthetic knowledge. This era in which verificationism
emerged to recast the referential theory of meaning was also one characterized by a narrowing
approaches (whether of strong descriptivism or strong prescriptivism).

17 See Principia Ethica, pp. 140, 114, 110, 21 and 36 on this referential account and how it flavors Moore’s
definition of ethics. Of course the Vienna Circle advocates mostly took exception to Wittgenstein’s own "picture
theory" of meaning, which they held was still too referential. See Hudson on the rise of the verificationism and the
importance of changing background theories about meaning on the understanding of value judgments.

18 “Suppose we accept moral realism. How could we have moral knowledge or justify our moral beliefs?
Traditional moral realists (e.g., Price, Reid, Sidgwick, Moore, Broad, and Ross) were intuitionists; in particular,
they combined moral realism with a kind of foundationalist epistemology. But intuitionism has seemed to many a
mysterious and, hence, suspicious view. How could any moral claim be self-evident? Surely there is no special
faculty of moral perception. Although ... intuitionism [may not] deserve all of the scorn it has received, ...any
kind of foundationalist epistemology faces general problems that force us to defend a coherence theory of
justification. A coherentist moral epistemology claims that a moral belief is justified insofar as it coheres in the
appropriate way with other beliefs, both moral and nonmoral, that we hold or might hold.” -Brink, p. 8.
of the notion of the synthetic. The understanding of the "synthetic" realm must reflect the central thesis of Humean empiricism that all knowledge that is not a question of relations of ideas, has its roots in the senses.

The notion of a real but non-natural property thus had a self-contradictory ring to the verificationist's ears. *Pace* Moore, "non-natural" must refer to the "supersensible" properties and to *metaphysical* postulations which he had claimed to be critical of. Whether one opted for physicalism as did most logical empiricists, or for phenomenalism as did A. J. Ayer, what observation and experiment can reveal was restricted to only "natural" objects. Ayer put the new empiricist's perspective well when he said in *Language, Truth and Logic* (1946), "There is nothing that counts as observing the designata of the ethical predicates, apart from observing the natural features of the situation" and "There is no procedure for examining the value of the facts, as distinct from examining the facts themselves."19

Carnap, Ayer and Reichenbach all utilize the consideration that ethical judgments are neither analytic nor synthetic to establish that a third way of analyzing these utterances is called for. In proclaiming a "third option" between naturalistic psychology and objectivistic intuitionism, the verificationists held themselves to be applying the analytic/synthetic distinction to value judgments more consistently than either Kant or Moore. Indeed they viewed noncognitivism rather than traditional consequentialism as the correct implication of a "naturalistic" ethics. Analytic and synthetic judgments exhaust genuine knowledge, but if

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19 Ayer, *Language, Truth and Logic*, Chapter VI. This line is already suggested by Hume in the third book of the *Treatise*: "Take any action allowed to be vicious: Wilful murder for instance. Examine it in all lights, and see if you can find the matter of fact, or real existence, which you call vice. In whichever way you take it, you find only certain passions, motives, volitions and thought. There is no other matter of fact in the case. The vice entirely escapes you, as long as you consider the object. You can never find it, till you turn your reflexion into your own breast, and find a sentiment of disapprobation, which arises in you, towards this action. Here is a matter of fact; but 'tis the object of feeling, not of reason. It lies in yourself, not in the object." -*Treatise*, bk. II, pt. I, sec. 2.
analytic, such judgments could not serve the normative function expected of them; and if synthetic, they ought to be verifiable by experience like ordinary scientific statements, but are not. As Ayer put it, they lack the "criteria of validity" characterizing the latter expressions.

The analytic/synthetic distinction also provided the ground for the "method of analysis of modern science" which Reichenbach applies to the language of morals. Reichenbach's (imperativist version of) non-cognitivism expressed in *The Rise Of Scientific Understanding* (1951), like Carnap's and Ayer's emotivist accounts deeply reflects the problem of the status of value judgements in light of the analytic/synthetic distinction and the verificationist criterion of meaning. To have cognitive meaning, an utterance must have a truth value that at least in principle is discoverable by empirical or analytic means. Linguistic expressions capable of meeting these requirements are those that are "either empirically verifiable or else analytic."

The theory of *meaning as use* is intimately connected with the non-cognitivist's novel characterizations of ethical judgment. Reichenbach purported to infer non-cognitive meaning directly from normative function, taken as exclusive of the informative function. But a crucial premise which goes unnoticed by all the early non-cognitivists is that we expect the normative function to be performed not only by moral judgments but by a wide range of non-moral value judgements as well. The issue is not merely whether we should include valuative principles, norms, standards and the like as a contribution to knowledge instead of something standing over against knowledge. We all know that to serve as a standard is to do more than

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20 A confusion is also evident in the assumption common to both sides of the dispute that informative and directive functions were to be taken as exclusive of one another. This no doubt developed out of the earlier convention of dealing with different kinds of judgments as resulting from distinct mental faculties. It is implied already by Hume's statement that "Morality is more properly felt than judged of." *Treatise*, p. 470.
serve as a contribution to empirical knowledge. The question is about the very character of what passes for knowledge in the sciences and elsewhere.²¹

Moreover, the doctrine of meaning-as-use is notoriously vague and when explicated is seen to presuppose a fallacious notion of the "factual understanding" of function, one that was supposed to be determined in an "above-the-battle" manner by assignment of the task to "meta-ethics," conceived as a strictly analytic project and only tenuously connected with normative ethics.²²

In this way the doctrine of meaning as use²³ led Reichenbach and other non-cognitivists

²¹ Accounts such as that of Reichenbach make informative and normative, or again "assertive" and "expressive" uses of language, the defining issue in the cognitivist/non-cognitivist debate. Our interpretation is supported in Prasad's extensive historical overviews of the controversy through the 1950s. Yet the more specific ways in which the thesis of non-cognitivism has been put depends heavily, as Hudson showed on the changing background of theories of meaning. The linguistic analysis suggested by Moore was developed in later years on views of meaning quite different from his own. "Meaning as use" became a byword for emotivists as well as for early critics like Austin and Urmson who nonetheless identified with the linguistic turn in philosophy. But for Urmson and the common language philosophers the study of use or the "functions" served by utterances could no longer be identified with meaning, nor lead to conclusions about meaning independently of examining the particular linguistic or communicative context for standards and language conventions implicitly at work.

²² This raises an extremely vexing problem that both the descriptivists and the non-cognitivists were confused about. How does linguistic function relate to mode of expression? In the case of value judgments—as for that matter any other "kind" of judgment, the same or similar meaning can be expressed in a wide range of linguistic modes and manners. There is always an element of analogy in identifying normative judgments with the characteristic use of one mode of speech. But this is what Reichenbach's characterization of value judgments in terms of "imperative" function or meaning achieved. The imperative generally designates a mode of speech in contrast to interrogative and indicative. This characterization must be suspect from the outset. For if imperatives form a natural class it is a class delineated by linguistic mode of utterance. But it is highly hazardous to say that a linguistic mode denotes a certain type of meaning. Even the notion of "characteristic uses" of modes or "characteristic meaning" attaching to utterances in a certain mode of speech leads to obscuring less typical yet still linguistically correct usages.

²³ The opposition between G.E. Moore and the emotivists, for instance deeply involves the intuitionist's representational accounts of meaning and the emotivist/prescriptivist theories of meaning as function or as use. Prasad notes that the thesis of a primary linguistic function identified with "kinds" of utterances emerged after early emotivists had toyed with the stronger theses about singular function (strong descriptivism and emotivism). Stevenson acknowledged the existence and breadth of the class of "mixed mode" terms, while Ayer appears to treat all terms as fitting exhaustively into what Stevenson would call the "pure modes" of descriptive and emotive meaning. But even in the later debates between moderate descriptivists (Forrester) and prescriptivists (Hare), the
to situate the explication of meanings within a scheme of linguistic categories derived from the analytic/synthetic distinction.\textsuperscript{24} As with the earlier logical atomist tradition, the positivists sought to isolate the meaning of a sentence within the sentence itself. The view was attractive in shutting out the open-endedness and ambiguity surrounding the problems of context and the interpretation of a text. Indeed the appeal to models of artificial languages, which the ordinary language philosophers decried, was motivated in part by the desire to expel such ambiguity. In an ideal artificial language each sentence "component" that is not a logical connective refers to something discrete and observable or is reducible to such terms. The analysis of a sentence is thus complete when it is broken into parts and the syntactic and semantic roles of each part are discovered. The non-cognitivists thought that Ogden and Richards' distinction between "symbolic" and "expressive" uses of language, --a distinction they took as exclusive and exhaustive-- provided a framework adequate to analyze the semantics of language.\textsuperscript{25} Perhaps connotations were something added in the hearer/listener context, they supposed, but the core meaning of a sentence could be determined by function, which was logically distinct from intension.

\begin{quote}
\textsuperscript{24} I am indebted to Lenn Goodman's article "Context," PEW., Vol 38 no. 3, July, 1988, for much of this discussion of meaning and context.

\textsuperscript{25} Even Stevenson cites Ogden and Richard's book Meaning as a strong influence for all the emotivists.
\end{quote}

Dewey's pragmatist response was that the notion of meaning as use involves "a radical fallacy." See ESML, 285 and Rachels (1977), p. 156.
But the analysis is naive on several fronts, and depends crucially on a myth of meaning related directly to the philosophical adequacy of the analytic/synthetic distinction. If we follow the criticisms of Quine (and Morton White (1951)), the sentence is not adequate as a unit for discovering meaning; one must look beyond the sentence and its mode of expression to the context of the community presupposed by use of a language. One must go beyond the sentence in order not only to effect an interpretation, but to find what "core meaning" there may be. A core meaning is not there to be discovered apart from such a context.

Interpretation reaches out to context, but context may be said to remain just beyond the reach of any particular interpretation. Ambiguity is bound to arise with interpretation; but neither of these can be dismissed by the appeal to an ideal artificial language as containing its own implicit rules for construction and analysis. This doesn't do away with interpretation but only makes it authoritative. Carnap's notion of analysis governed by rules of the logical syntax of a language is just one such failed attempt.

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26 "In popular lore and philosophic tradition, meanings are given by definitions -by analyses, as philosophers came to say. Non-cognitivists like Ayer, Stevenson and Hare broadened the tradition: an analysis need not say the same thing in other words. It can describe a use, saying directly what the person who uses a term is doing with it. Still (this) tradition now seems dated. The problem is not just fashion; major developments in the philosophy of language seem to discredit analysis. Where do meanings and analysis fit in the best picture of thought and talk? ...When a term is central to our conception, the lesson seems to be, a search for definitions is misguided. To expect analysis to do serious philosophical work is to succumb to a myth of meaning. The myth may take the form of accepting a sharp analytic-synthetic distinction—a divide between truth in virtue of logic and meaning alone, and truth in virtue of the ways things substantively are. Quine challenged the myth in this form. He despaired of tying meanings straight to empirical tests, and suggested 'It is nonsense, and the root of much nonsense, to speak of a linguistic component and a factual component in the truth of any individual statement. Science has its double dependence on language and experience; but this duality is not significantly traceable into the statements of science taken one by one.'—Alan Gibbard (1990) Wise Choices, Apt Feelings: A Theory of Normative Judgment, p.30-31.

27 Bruce Kuklick's article "The Analytic-Synthetic and Descriptive-Valuative Distinction" sees the prescriptivist's dependence on the descriptive/valuative distinction as leaning upon empiricist construals of the analytic/synthetic distinction; this he argues is their vital flaw. Indeed he cites Hare to the effect that "the problem about the distinction between the descriptive and valuative is an offshoot of the distinction between synthetic and analytic." Kuklick, p. 97. See also Hare, "Descriptivism," Proceedings of the British Academy, 49 (1963). pp. 115-116, 130.
of meaning and its related causal construal of belief/attitude relations, it becomes possible also
to move beyond the myth of context-independent meaning.

It is more than a little ironic that Moore’s natural/non-natural distinction gave
unintended impetus to the rise of a descriptive/valuative distinction which threatened to
undermine the objectivity he saw in value judgments.\textsuperscript{28} Certainly not all cognitivists are
naturalists, and not all naturalists are descriptivists in the strictest sense of conceiving moral
judgments as descriptive claims, testable in ordinary empirical fashion. Certainly also, not all
that can be castigated as "non-naturalism" must be a form of foundationalist intuitionism as in
Moore. The oppositions between the descriptivist and prescriptivist that have been the
contemporary forum for meta-ethical debate have served no better than their progenitors in
clarifying the traditional ‘twin gaps’ between belief and attitude, and again between attitude
and action.

1.2 From Hume to Stevenson on reason and logical connections

The influence of Hume’s views on 20th-century logical empiricist treatments of value
judgments is well known. Hume’s influence on the logical empiricists’ treatment of value
judgment is easily seen from the contrast he forged between reason and the passions. This is
not to say that Hume, the philosopher who sought to ‘give the passions their due,’ would have
approved of the condemnatory treatment the "passions" were sometimes given by the early

\textsuperscript{28} Moore’s own reconsidered view, in the aftermath of extensive criticism, leans towards formulating the
natural/non-natural distinction more closely to a descriptive/valuative distinction. See Olthuis, pp. 21 and 22.
ethical emotivists.²⁹ Hume’s own writings have been an important influence upon both 20th-century consequentialist and emotivist ethics.³⁰

Of course the antagonism of reason and the passions, or knowledge and emotion, is longstanding in the history of philosophy. Hume makes this sharp juxtaposition along with rationalists like Plato and Descartes, and all three appear to have been under the sway of a kind of faculty psychology which we can now see was simplistic. Whether one distinguishes the basic human faculties as Reason, Will and Sentiment, as did Hume, or as Cognition, Volition and Feeling, as did Kant, the guiding assumption of this faculty psychology is that human behavior is explained by analysis of discrete human faculties that perform discrete functions.³¹

Under such a characterization of the human faculties, it is no surprise that the functions served by these faculties were also conceived as discrete and mutually-exclusive. The

²⁹ Among Vienna Circle associates, Schlick was known to be a dissenter from the emotivist theory. But Schlick wrote little about ethics and how he intended to make his more naturalistic views of ethics consistent with the strict empiricism of his colleagues was never clear.

³⁰ Hume’s era, of course, came before the Darwinian revolution, and "evolutionary ethics" in its modern sense is considerably more recent. Yet as a defender of human nature against rationalism, Hume’s own intentions have often been treated as leading more correctly to evolutionary naturalism than to emotivism. The troubling relationship between beliefs and attitudes, (which extends to discussion of facts and propositional contents) was centrally at issue for Hume. Whether there is an interpretation of Hume’s ethics which shows it as self-consistent has often been questioned. The reason I focus only on Hume as a predecessor of the emotive theory, is the centrality of the emotive theory to the logical empiricist’s understanding of value judgment.

³¹ “It has been customary, since Kant’s time, to assert that Cognition, Volition, and Feeling are three fundamentally distinct attitudes of the mind towards reality. They are three ways of experiencing, and each of them informs us of a distinct aspect under which reality may be considered.” G.E. Moore, Princípios Éticos, p. 129-130. Hume of course did not hold the Cartesian account of personhood as a disembodied agent that many Continentalists criticize. But the faculty psychology appears to be a deeper shared assumption.
traditional 'twin gaps' mentioned above were of major concern to Hume, who insisted that there are no bridges of "logical connectedness" between them.\textsuperscript{32}

At least as far back as John Locke, one finds in the empiricist tradition an emphasis on the priority of descriptive over valuative assessments of an object; in order to evaluate something, one must first be able to describe it. Hume takes the dichotomy between reason and the passions one step further in the second book of the Treatise by representing reason as restricted to the descriptive type of assessment: reason is capable only of informing us of matters of fact and their inferential relations. Evaluations hence presuppose facts or factual judgments, but are insufficient to determine them. Reason concerns only the determination of means to ends, and the ends are said to be determined by the separate faculty of Sentiment.

Hence,

\begin{quote}
...the understanding can neither justify nor condemn (feeling)... a passion must be accompany'd with some false judgment, in order to its being unreasonable; and even then 'tis not the passion, properly speaking, which is unreasonable, but the judgment.\textsuperscript{33}
\end{quote}

\textsuperscript{32} Since my focus is on the relationship of beliefs and attitudes, I focus on the treatment of "Cognition and Feeling," or "Reason and Passion," and have said relatively little about the other "logical gap" that has traditionally concerned meta-ethicists. What if anything is required beyond a disposition to move one to action? The question is interesting, but only peripherally relevant for us here. What is important here for me is more the fact that ethicists of very diverse traditions have often agreed that it is necessary to recognize a logical gap for one of the two relationships. The disagreement often arises as to which of the two is a closed or strictly causal relationship, and which one a logical gap is evident in. Now in the Treatise Hume makes it basic to his account that the will must fill the logical gap between belief and movement to action. In opposition to rationalist ethicists of his day, Hume argues that rules of morality cannot be conclusions of reason. They have no automatic reason-giving force as is often alleged. "Reason alone can never be a motive to any action of the will." Furthermore, a sentiment or feeling "depends not on the will, nor can it be commanded at pleasure." The sentiments, Hume holds, must instead be excited by nature. Hume's more radical statements made early in the Treatise have the effect of rendering reason wholly inactive in motivation to action. At other times however, the claim seems to be that reason is active but not primary. Hume's view was that reasoning can affect behavior only through modifying beliefs. This may be enough to support the thesis of the primacy of sentiment in human conduct. Yet even on this latter view, which makes both reason and desire requisite to action, the desires themselves (and by extension, ends,) are closed to reasoned decision. See also Enquiry, Section III, 6. From Hendel, ed., Hume, p. 138.

\textsuperscript{33} Treatise, p 416.
The point was put by Hume in terms of the Aristotelian contrast between reason as "speculative," and the "practical" nature of moral propositions and rules. The latter are supposed by Hume "to influence our passions and actions, and to go beyond the calm and indolent judgments of the understanding." On Hume's view this difference leads to the radical conclusion that one can maintain any attitude in the face of a given state of affairs, without logical fault. The familiar Humean notion that reason does not "rule out" any object of desire was already intimated in the view that we can take any set of facts and combine them with any attitude without transgressing against reason. Both depend crucially on empiricist assumptions about logical disconnectedness between beliefs and attitudes. It led to consequences that Hume was prepared to accept, however unseemly to some.

'Tis not contrary to reason to prefer the destruction of the whole world to the scratching of my finger. 'Tis not contrary for me to chuse my total ruin, to prevent the least uneasiness of an Indian or person wholly unknown to me. 'Tis not contrary to reason for me to choose my own acknowledg'd good to my greater, and have a more ardent affection for the former than the latter.

In fairness to Hume, this was for him more the dispelling of category errors and reductive equations of reason with egoism or altruism, than a distasteful conclusion he was "forced" to accept. But are the categories Hume employs not also a relic of his suspect faculty psychology? There is, as Hume rightly sees, no sense to a model which portrays a man holding belief B1 to be logically compelled to some "corresponding" attitude A1. The

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34 Treatise, Bk. III, Part I, Sec. I. For discussion see P. Foot "Morality as a System of Hypothetical Imperatives," in her Virtues and Vices, p. 78.

35 Cited from Nathanson p. 101. The view here is argued by Hume through his distinction between "real existences" and beliefs. Choices such as those in question are conceived as real existences and divided off on this basis from beliefs, which alone can be judged rational/irrational. But this radical rift between separate faculties is part of the inadequacy of Hume's view on my account.
appropriateness of attitudes, or even of emotions like guilt and shame, do not have this logical
collection to descriptive knowledge. But what this lack of logical or deductive connection
meant for Hume, as for certain of the emotivists, was that the connection must be either
radically contingent, or else determined by the forces of evolution and custom. For Hume the
appeal to custom or "habit" is the basis for a proper response to the problems of the lack of
logical connections between beliefs and value judgements. By contrast, I think Hume's
skepticism, while rightly targeting the audacity of reason to dictate any one 'justified' attitude
to take towards a set of facts, should not extend to treating "reason" as assimilable to
everything that is of evidential or warranting import in attitude formation and preference.

Hume nonetheless developed a view about pan-human moral sentiment. This account and
his insistence that factually informed people will have approbation for the same objects would
seem anomalous, was not custom taken to perform the lead role in the determination of
attitudes. Hume's view had the implication that a high degree of uniformity of moral
sentiment among people of diverse religious and cultural beliefs should be expected. Yet this
view was arguably as far at odds with the cultural reality of Hume's day as it would be with
our own. For us it is enough to note that both the naturalistic and the emotivist strains of
Hume's ethics were reworked into different and mutually-exclusive systems in the first quarter
of the twentieth century.

The issues raised here bring the empiricist notion of "logical connection" to the fore.
Indeed a deductivist construal of this notion appears to be at the root of the sharp division
between logic and psychology that logical empiricists inherited from Hume and continued to
lean upon. For many empiricists the notion of a "logical connection" connoted no more than

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36 By rejecting this cultural determinism out of hand, I imply merely that the force of culture cannot be
established a priori. There is a great weight of evidence I think against this thesis when treated as an empirical
a deductive relationship between propositions; at its strictest the empiricist is skeptical about
the grounds both for (enumerative) induction and for inference from "Is’s" to "Ought’s," and
bases this skepticism on a view of evidentiary relationships too narrow to encompass more
than deductive relationships. Hume's skepticism about inference from belief to attitude, or
from Is to Ought, was closely related to the issue of whether such inferences were
formalizable. If valuative judgments are something over and above matter-of-fact judgments
and are not logically entailed by them, then there is a fallacy --or at least a logical
strangeness-- involved in inferring valuative conclusions directly from non-valuative premises.

This problem about the relationship of facts to values, beliefs to attitudes, returns in the
mature form of emotivism developed by C.L. Stevenson in the 1940s. Stevenson mentions a
"striking debt to Hume" in the development of his own version of the emotive theory.
Stevenson's *Language and Ethics* (1944) and *Facts and Values* (1963) attempt to overcome
the disparaging of moral reasoning and the role of emotions in human thought that was
evident in the versions of emotivism suggested by A.J. Ayer (1940; also 1954) as well as
proponents of the Vienna Circle.37 Stevenson was himself deeply influenced by logical
empiricism; but his interest in developing an account of moral reasoning that could stand on
its own merits, his rejection of verificationism, and his refusal to construe metaphysical
questions as non-sensical, are among the differences that mark him off from the positivists
and what he viewed as their "naive" emotivism.

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37 My emphasis thus far has been on the determinants of attitudes as distinct from emotions and feelings. Few
meta-ethical theories have attempted to collapse attitude and emotion as the early emotivists did. It would be a
side issue for us whether even man's conative-affective nature is adequately dealt with by the emotive theory. But I
note in passing that I do not find that the emotive theory does justice even to the emotions. There is more
appropriateness or "fit" between the emotions and the life-situation than the emotivists were apt to recognize. And
there is more cohesion or interdependence between emotionality and cognitive development than they were prone
to admit. I agree with criticisms sometimes made along these lines, but I can state my claim independently of this,
once we recover genuine differences between emotions and attitudes.
Against the tradition of these earlier non-cognitivists, Stevenson insisted that we must hold on to a notion of "justification" of valuative judgments that is not merely a euphemism for saying that a belief is "persuasive" or causally efficacious in bringing about the holding of a certain attitude. But the simultaneous affirmation of a theory of "good reasons" along with emotivism hides an inconsistency, I believe, that has not been satisfactorily resolved either by Stevenson or by any of his contemporary defenders such as Stephen Satris or Bruce Waller.\textsuperscript{38}

One major reason for this was Stevenson's deference to Hume in his understanding of reasoned connectedness; like Hume and the empiricists of his own time, Stevenson explicitly denied that there can be any "logical" connection between factual reasons and valuative conclusions at all: "The truth of the reasons themselves can be tested by logic, but their bearing on the valuative conclusion is neither logical nor illogical. It is simply nonlogical."

Here is the first juncture at which we see the problem of ampliative inference and its treatment by logical empiricists.\textsuperscript{39} I will later relate Hume's influence on separating the Is/Ought problem from the problem of inferring universal claims from particulars, to deep-seated contemporary misunderstandings of norm governance. Hume's separation I believe reflects the biases already discussed above, and marks an arbitrary narrowness in his highly

\textsuperscript{38} Hence Urnson held that Stevenson wrongly contrasted persuasive influence with logical relevance and negated the discrimination of validity in reasoning to attitudes only by his overemphasis on the causal picture. Stevenson defines himself as a \textit{defender of the nomological-deductive view of explanation}. See S. Satris' book \textit{Ethical Emotivism} and Waller's article "The Virtues of Contemporary Emotivism," \textit{Erkenntnis} 25 (1986), p. 61-75, for contemporary defenses of emotivism.

\textsuperscript{39} Stevenson, \textit{Facts and Values}, p. 85. Ampliative inference, as the pragmatists use the phrase, is sometimes conceived as identical to inductive inference, overlapping with induction. Usually it is thought of as involving movement between belief and attitude. Answering this question depends both on how narrowly one conceives of inductive methods and how broadly one construes ampliative inference. While I will not engage in these definitional matters here, I will use the phrase as indicating inferential strategies which overlap with induction 'proper,' understood as enumerative and other more or less formalized inferential patterns.
influential presentation of the problem of induction. The impact of this narrowness in Hume's characterization of the induction problem, which Frederick Will (1988) in particular has drawn attention to, cannot be over-emphasized. For it is in his presentation of the induction problem as one restricted to inferences to descriptive or "matter of fact" knowledge that we see a source for the tendency, still so common today, to treat the norms of thought or knowledge, and the norms of action or practice, so disparately!

Stevenson's mature form of emotivism went far in criticism of both the "naturalist" and "metaphysical" ideals of ethics as a science. Yet a basic problem persists in the account he gave about the relation between beliefs and attitudes. The problem is clearly seen where Stevenson attempts to correct in his own work what he sees as a deep inadequacy in Ayer's emotivism: the notion that reasons for a moral judgment are "reasons only in the sense that they determine attitudes." Stevenson, I believe, doesn't have the resources to avoid this problem because he makes the connection between beliefs and attitudes explicitly "causal" in contrast to "logical." "Empirically verifiable reasons, when used to support or oppose an ethical judgment, are always related to the judgment psychologically." "The supporting reasons mentioned here have no sort of logical compulsion."

His reasons do not 'entail' his expression of approval, of course, or make it 'probable.' An expression of attitude cannot stand in these logical relationships to descriptive statements, but only in causal relationships. But the reasons do make a difference: they help to determine whether the man will continue to make his judgement, or qualify it, or

40 As Will (1988) notes, the objectivism/relativism debate is evident in divergent attempts to construe ampliative discourse. The field divides between those who attempt to squeeze ampliative inference into a pre-conceived and formalizable model of induction, and those who portray the failure of ampliative inference to be formalized as an indication of the relativity-to-framework of the normative commitments that are the end-product of ampliative reflection.
replace it by an unfavorable one. So they can be called 'reasons' in a perfectly familiar sense of that term. 41

I take exception to Stevenson's claim that 'reasons' as he employs it is used "in a perfectly familiar sense of that term." It is clearly used in a causal sense, in explicit and even definitional contrast to rational warrant. 42 Like many others of empiricist persuasion, Stevenson used reason, argument, and validity as closely inter-related terms, and as definitionally linked to the empiricist contrast between logic and psychology. 43 Stevenson prefers to use the term "psychologically related" rather than "causally related," although passages other than the one cited show that he often uses the two indiscriminately. Indeed he gives every indication that what he means by a "psychological" connection is one that is merely causally efficacious in bringing about or changing a belief or attitude. 44

When Stevenson says that reasons "support the (valuative) judgment in the way that reasons support imperatives," he draws attention to a contrast with the inference of factual beliefs from factual premises; this latter relationship is one which he apparently takes as befitting the model of logical entailment. There is reasoning in ethics only where logic and facts are used. But persuasion relies on emotive force rather than either of these. The context of contrasting imperatives is one of persuasive force, and "It is cognitively non-sensical to

41 Facts and Values, p. 145. See also Urmson, who makes this objection to Stevenson's account in his 0 The Emotive Theory of Values

42 Ethics and Language, p. 113.

43 "The way in which the reasons support the imperative is simply this: The imperative is used to alter the hearer’s attitudes or actions...More generally, reasons support imperatives by altering such beliefs as may in turn alter an unwillingness to obey." Ibid, p. 27.

44 It is perhaps the nomological deductive method or view of explanation to which Stevenson like the positivists appeal, which represents this commitment to treating the explanation of psychological relations in this way.
speak of either 'valid' or 'invalid' persuasion."45 Here is the basic reason why Stevenson says "validity" fails to obtain in ethics. The point is to underline the conclusion that words like because and therefore don't function in ethics as in the parts of discourse whose subject matter is primarily descriptive.46

A valid argument as conventionally understood is one in which the truth of the premises guarantees the truth of the conclusion. Taken in this formal sense, the demand for validity in ethics would be a demand that moral principles be analytic. Stevenson took validity as a formal relation between premises and conclusion. Inferences from facts to evaluations are not candidates for validity or invalidity because "the reasons do not establish or call into question the truth of an ethical judgment's (descriptive) meaning."47 No clearer indication of Hume's influence on Stevenson could be asked for than the latter's statement that "Under the name of 'validity' he (the ethicist) will be selecting those inferences to which he is psychologically disposed to give assent."

Any statement about any matter of fact which any speaker considers likely to alter attitudes may be adduced as a reason for or against an ethical judgment. Whether this reason will in fact support or oppose the judgment will depend on whether the hearer believes it, and upon whether, if he does, it will actually make a difference to his attitudes.48


46 MacIntyre (1966) p. 259.


48 Ibid, p. 171 and 114-5. The descriptivist Mary Forrester thinks the need to sustain a viable notion of validity in ethical reasoning is an argument for presenting ethical language as primarily descriptive. She notes that few cognitivists today are moral realists, asserting that value judgments can be expressed as propositions which are true or false. But "For sentences that do not have truth values, it is difficult to see how there can be valid arguments containing them, and it is also hard to make sense of a kind of logic in which validity has no role." - Moral Language, p. 57.
What we have seen as an inheritance by Stevenson from Hume is an assimilation of the
deductive/non-deductive distinction with the reason/psychology distinction. This dissolves
some category errors, but creates as many others in its wake. For one thing it implies that
non-deductive relationships call for explanation only in terms of empirical-laws of a
psychological kind. It seems to be a largely semantic issue whether or not we should restrict
terms such as "logic" and "validity" to deductive relations alone. But the assimilability of
"argument," "reason" etc. to the class of potentially valid or invalid inferences is a substantial
issue where the non-cognitivist's case appears unjustified. These are the assumptions on
which "argument" and "reason" come to be definitionally contrasted with persuasive and non-
cognitive discourse. The central distinction in Stevenson's meta-ethics, the difference between
disagreements about beliefs from disagreements in attitude, is tied up with this contrast.49
But while the naive emotivist has surely reminded us that inference to valuative or normative
conclusions is non-deductive, even the sophisticated emotivist falls far short of showing that
such inferences are in-principle not matters that can be argued about.

On the other hand, Stevenson should be credited for his attempt, however undermined by
his epistemology, to drive a wedge between attitude and emotion. This is what Stevenson
intended in part by claiming to include a sense of "justification," and even of "truth" or

49 Stevenson's definition of emotive meaning as that which has a disposition to affect conative-affective
response parallels his account of cognitive meaning as that which has a disposition to affect belief. This
dispositional account of meaning is the root cause of Stevenson's inability to differentiate feelings from attitudes.
Even the later prescriptivists took Stevenson to task for failing to notice the far greater relevance of warranting
reasons in "attitude" formation than in the emotions. The dispositional account masks this difference because we
don't normally argue for dispositions. Stevenson can mark no logical differences between feelings and attitudes
that might save the demand for warranting reasons; he cannot do so because for him the difference is only one of
degree of "complexity." He saw "Attitude as a complicated conjunction of dispositional properties." Ibid, p. 60.
Urmson emphasizes how later prescriptivists attempted to distinguish attitudes from emotions. Expressions of
approval/disapproval they saw as logically distinct from emotive expressions like fear or anger, not just exemplary
of them.

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"falsity" of moral claims, while excluding "validity" from moral reasoning. But ambiguities are bound to be evidenced in his attempt,\textsuperscript{50} if justification can be no more than an appeal to arguments that actually work to change or redirect attitudes.\textsuperscript{51} Stevenson has been often praised by the later prescriptivists for re-introducing a close connection between factual beliefs and valutative conclusions, and for bringing the relevance of empirical reasoning in ethics back to the fore. But the rationality which Stevenson genuinely enough wanted to re-introduce into the relationship between the aptness or wisdom of value judgments and the veracity of the empirical beliefs on which they are based, is still an intruder. Hume's image, it seems to me, looms large in the reasons for this failure of emotivism.\textsuperscript{52}

A basic dogma of Stevenson and all the non-cognitivists is to be found in the "empirical method" that is advocated. This is the dogma that the distinctively valuative or ethical is beyond rational discussion. This goes considerably beyond the view that the soundness of

\textsuperscript{50} My crucial point is that the causal or "psychological" construal of this relationship Stevenson's empiricism drew him to, actually undermines any notion of justification with more than persuasive force. This is not to say that the notion of "psychological connection" could not be given a cognitive treatment. Indeed it is often said that we can give a psychological account of belief as well as of attitude formation. The later prescriptivists made progress in this respect, but Stevenson's view is still reductive: the "psychological" characterization is exclusive of and contrasted with "logical" relations.

\textsuperscript{51} Hume's two claims that reason is the slave of the passions, and that the passions themselves cannot be reasonable nor unreasonable, are usually taken to imply that we can only reason about how best to satisfy existing desires or ends; it is sometimes suggested that we cannot reason about these desires or ends themselves. Perhaps this is the source of the most curious presumption of modernism epistemology, to divide scientific explanations between those given in terms of normative constraints and those given in terms of psychological set. The Humean view has the effect of making all rationality instrumental rationality grounded on what came later to be regarded as a psychology of desires, and of denying that we can reason about the ends or desires that figure as the starting points of instrumental reasoning. Brink notes that we can deny the Humean view that reasons for action are desire dependent, without presuming a Kantian outlook. We can hold that we do argue and reason about ends, and can argue this without presuming Kant's extrinsic/intrinsic value distinction or his peculiar notion of the relation between belief and action.

\textsuperscript{52} It is a problem I see daunting even for more recent forms of non-cognitivism such as R.M. Hare's "prescriptivism." I expand on this below in a brief discussion of what Hare called ultimately arbitrary "decisions of principle" in ethics, somewhat related to Wittgenstein's notion of decisions to live a certain 'way of life'.

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valuative judgments depends upon the veracity of the factual evidence they’re based upon. It implies that there is one right view about any disagreement about belief. Stevenson says that "Supporting reasons have only to do with beliefs." A reasoned agreement in ethics "is theoretically possible only to the extent that agreement in belief will cause people to agree in attitude." 53

Stevenson is right that disagreement in attitude is not finally reducible to a disagreement over facts. He is right again that ethical agreement requires more than agreement on matters of fact. But he buys into empiricist dogma —and even into the scientism of the logical empiricists which he claims to disavow— in insisting that disagreement in attitude is rationally resolvable only to the extent that it has its "roots" in disagreement in belief. "Rational methods can resolve an ethical disagreement if and only if it is rooted in disagreement in belief.... The conclusion is perfectly general." 54 This theme —granting rationality in value-charged discourse but restricting it sharply to the use of formal logic or empirical reasoning— is presented as a central claim of Stevenson’s account, and another one he credits to Hume.

While Stevenson claims to make the relationship between beliefs and attitudes reciprocal, the epistemological assumptions he makes point instead to a one way street. The phrase "will cause people to agree in attitude" tips us off again to the strictly psychological explanation of the connection between beliefs and attitudes. What of disagreements in belief that may have roots in divergent attitudes? The whole notion of disagreement as "rooted" either in fact or beyond fact militates as much as does the causal account against the "reciprocity" of fact and value that the sophisticated non-cognitivists claim to recognize.

53 Ethics and Language, p 31.

Stevenson's account is not as differentiable from A.J. Ayer as he wanted it to be on this issue. Ayer held that one never really does argue about questions of value. Ayer presents himself as skeptical even of the view that disagreements of attitude may persist after matters of fact have been settled. He is here more the Humean in alluding to biological and social conditioning as insuring that like stimulus will produce like sentiment or emotive response in humans. Stevenson criticizes Ayer for this, saying that the latter reduces ethical disagreements to disagreements over belief, and hence misses the essential uniqueness of ethical disagreement. The substantial point here is that one can "hope" but not always expect that empirical reasoning will put an end to the disagreement. *Pace* Ayer, being rational about disagreements of belief does not mean that we will always agree what the facts are, let alone how to evaluate them.

Stevenson relies upon the same distinctions Ayer does in contrasting "speaking about" and "arguing about." We may speak about the emotive residue that remains after logic and fact have been exhausted, but we cannot argue or reason about it. Speech includes persuasion and emotive appeal, and disagreement that fails to be resolved by attention to the facts may be carried on as a matter of persuasion. Stevenson wanted to say that persuasion has its proper place and ought not always be condemned, yet he still uses "persuasion" to contrast with argument having a reasoned or argued basis. This goes far beyond contrasting persuasion with deduction alone. Arguing refers to "rational" processes, and for both men these are exhausted when empirical beliefs along with inductive and deductive logic are "exhausted." The "limits of reason" have now been reached, and what disagreement may persist is necessarily beyond rational resolution. There is no rational discussion about the essentially ethical or valuative aspects of the issues.
1.3 Intrinsic values as unconditioned interests

We must return briefly to Kant as the source of my final line of examination. For although the distinction between conditioned and unconditioned value judgments goes back at least to Greek inquiries about *phronesis* and *theoria*, the character of the distinction as it entered twentieth century meta-ethics appears due to the great influence of Kant.

The identification of imperatives with "ought" statements together with the distinction between hypothetical and categorical imperatives, worked for Kant to delineate two classes of declarative claims corresponding to imperatives. Those having a priori grounding were distinguishable on this criterion from those the validity of which were grounded a posteriori. That you "ought" to do some particular action, or that some particular thing x is a good thing, was for Kant a synthetic judgment. But of course not all such claims were considered to be grounded a priori. How then is the distinctively moral sense of "good" to be marked? Kant's response was that the moral subclass of value judgments could be identified by the distinction between extrinsic and intrinsic value. Extrinsic value claims were

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55 "When he claimed that the fundamental principle of morality was a Categorical Imperative, Kant was not interested in any purely logical distinction between forms of what are literally imperatives. He was concerned with the recognition of an *I must* that is unconditional and goes all the way down, but he construed this unconditional practical necessity as being peculiar to morality. He thought it was unconditional in the sense that it did not depend on desire at all: a course of action presented to us with this kind of necessity was one we had reason to take *whatever we might happen to want*, and it was only moral reasons that could transcend desire in that way. ...Kant's idea of practical necessity is basically [a] familiar one, but it is given a particularly radical interpretation, under which the only necessary practical conclusions are those absolutely unconditioned by any desire. For Kant there could be a practical conclusion that was radically unconditioned in this way, because of his picture of the rational self as free from causality, and because there were reasons for action which depended merely on rational agency and not on anything (such as a desire) that the agent might not have had." -Bernard Williams (1985) *Ethics and the Limits of Philosophy*, p.189. See also his footnote #16.

56 "All imperatives command either hypothetically or categorically. The former present the practical necessity of a possible action as a means to achieving something else which one desires (or which one may possibly desire). The categorical imperative would be one which presented an action as of itself objectively necessary, without regard to any other end." -*Foundations of the Metaphysics of Morals*, Sec II.
dependent upon desires and were expressible as hypothetical imperatives: "You ought to do x if you want to achieve goal y." But intrinsic value claims were expressible as "You ought to do x, simpliciter."57 All categorical imperatives were moral imperatives, although some hypothetical ones might also have moral force in consequence of categorical imperatives from which they are derivable.58

The distinction between hypothetical and categorical imperatives has been highly influential upon 20th-century meta-ethics. We may parallel our work in the first section by seeing how G. E. Moore's work helped shape the distinction in its modern forms. As with earlier intuitionists like Sidgwick, Moore wanted to affirm the special kind of knowledge that moral principles supplied; naturalism (especially in its familiar utilitarian form) and subjectivism were among the views which Moore thought denatured this knowledge. Indeed he attacked both when he argued that the use of "Right" and "Wrong" does not consist in relation to any being's feelings, thoughts or will, nor does it consist in consequences. Judgments of goodness were ambiguous in that they could be instilled with both natural and non-natural meaning. But ethics was to resolve the ambiguity by concerning itself with the distinctly moral sense. In asking what the good is, one may be asking the descriptive question of what traits a person or group esteems highly; but the point was that there is always another, distinctly moral way of taking the question. This distinctly moral use of "good" was said to have its mark as an ascription of intrinsic value:

57 This uniqueness of intrinsic value claims in enjoying a priori grounding worked for Kant to neatly mark off moral from non-moral valuative judgments. The distinction is often marked among ethicists simply by employing a capitalized "Good" to express the object of moral theory, their alternative central principles being variations of this singular summum bonum. With Rorty I must agree that the capitalization of philosophical concepts helps little in establishing their transcendental status or philosophical importance.

58 "If the action is good only as a means to something else, the imperative is hypothetical; but if it is thought of as good in itself, and hence as necessary in a will which of itself conforms to reason as the principle of this, the imperative is categorical." -Ibid.
Everyone does in fact understand the question 'Is this good?' When he thinks of it, his state of mind is different from what it would be, were he asked 'Is this pleasant, or desired, or approved?' It has a distinct meaning for him, even though he may not recognize in what respect it is distinct. Whenever he thinks of 'intrinsic value,' or 'intrinsic worth,' or says that a thing 'ought to exist,' he has before his mind the unique object -the unique property of things-which I mean by 'good.'

In so arguing, Moore like Kant sought to separate questions of moral goodness from interests and to establish the autonomy of ethics. Now, the identification of judgments of intrinsic value was to be delivered through the intuitionistic "method of isolation." This Moore said allows us to isolate and identify each thing so that we can ask whether it is good, i.e. what degree of value it has in itself. Moore's test for intrinsic value was then the question, "...Would it be worthwhile that it should exist, even if...(nothing else did)."

This, he insists, we can surely do, and in so doing, "We are not merely making an assertion either about our own or about anybody else's attitude of mind towards the state of things in question." While the veracity of this intuitionistic test came under severe criticism from the logical empiricists, Moore never gave up the project of distinguishing intrinsic good or "good in itself" from what one ought to do, the "right" or "good as means." But neither would the non-cognitivists have expected this dichotomy to be questioned. They also were sure that the meaning of such judgments was something other than "stating facts" about attitudes or giving

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59 Moore from *Principia Ethica*, p. 17.

60 The autonomy claim is one that Moore also had the qualify in later years. *Principia* had stated it most radically: In the case of intrinsic value claims, "no relevant evidence whatever can be adduced: from no other truth, except themselves alone, can it be inferred that they are either true or false." -p. 14.


authoritative definitions of moral concepts. They too resisted the descriptivist’s reduction. Their criticism lay primarily on the interpretation of what it was that was "really" being done. Noncognitivism typically was presented as a proposal for clearing up certain prevalent misunderstandings about what people were "really" doing in their use of language. Hence it is important to recognize how heavily the logical empiricists leaned in their own account of value judgments on the Kantian distinction and on the distinctness of moral from non-moral valuative judgment. Rudolph Carnap for instance points out how within the Vienna Circle the distinction between intrinsic and extrinsic value was associated with the metaphysical-empirical distinction, and set the context for the analysis of the meaning and function of value judgments:

In our discussions in the Vienna Circle we were much concerned with clarifying the logical nature of value statements. We distinguished between absolute or unconditional value judgements, e.g., one that says that a certain action is morally good in itself, and relative or conditional value statements, e.g., one saying that an action is good in the sense of being conducive toward reaching certain aims. Statements of the latter kind are obviously empirical, even though they may contain value terms like "good." On the other hand, absolute value statements that speak about what ought to be done are devoid of cognitive meaning according to the empiricist criterion of significance.63

As we shall see more fully in Chapter Two, the positivists often drew attention to the difference between ethics and science by contrasting the "practical" nature of value judgments with the "theoretical" nature of scientific judgments. "Practical" nature here includes instrumental assessments of the utility of using certain means to achieve certain ends-in-view. And as Reichenbach insists, it includes at least one class of judgments that it was thought must be treated as absolute or transcendental: those which typically formulate norms

concerning ends rather than means. In Chapter Three I follow up on this issue in my criticism of instrumentalist conceptions of reason.

This question of the uniqueness of what Reichenbach calls the 'transcendental' sense of "good" has had an interesting history in America since the 1950s. I commend Stevenson for seeing Moore's emphasis as detrimental to the understanding of ethical discourse. Stevenson recommended a more "generic" sense of good as the focus of ethics, and uses this sense throughout his analysis of disagreement in attitude. R. M. Hare, in his prescriptivist writings from the 1960's to the present, continues to distinguish good/ought/right as "general" moral terms and contrasts them with "specific" moral terms. In one sense this misses Stevenson's point, since Hare's approach has the effect of placing the focus of meta-ethics back on explication of a distinctly moral usage of "good." But Hare's approach also helps to diffuse the emphasis on good/ought/right as privileged concepts in ethics. It is natural enough, indeed advantageous, he says, that language supplies words with both primarily prescriptive functions (like "good"), and primarily descriptive ones (like "chaste"). But the primarily prescriptive character of judgments of goodness, etc. doesn't mean that their normative force is somehow outside the bounds of reason.

Hare's prescriptivism is indeed highly Kantian in character; but especially in his most recent work, the "primarily prescriptive" character of terms like good/ought/right is no

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64 Yet J. O. Urmson was also correct to point out that Stevenson's understanding of the meaning of valuative terms proceeded virtually independent of context. In order to correct this deficiency, Urmson emphasized the cognitive nature of "grading" uses of "good" and of their informative function where acknowledged standards were in use. He insisted that standard-setting contexts were not without rationality, since the point of a standard-setting dispute was not merely 'to persuade' but to develop non-arbitrary and improving standards for use. Yet Urmson's sharp division between "standard using" and "standard setting" contexts still appears overstrong, and indeed is appropriated, as we will see in Chapter 3, by Carnap in his discussions of "internal" and "external questions". This in effect undermines the continuity of reason between the two contexts that Urmson had meant to emphasize.

longer taken as revealing a hidden non-cognitive nature of moral judgments. This shift is part of Hare’s attempt to reconcile what he takes as correct in both prescriptivist and descriptivist approaches. While Hare’s own focus remains on moral reasoning, there is no longer a dichotomy of kinds between this and the broader sphere of normative discourse. Although Hare no longer expresses the distinction in Kantian terms of categorical and hypothetical imperatives, he identifies *overridingness* along with *prescriptivity* and *universality* as the three formal or logical properties of moral judgments. But the formalist project of *Moral Thinking* (1981) does not avoid the problem of arbitrary "decisions of principle," and falls into a reluctant intuitionism.66 The crucial property that distinguishes moral from non-moral value judgments is only "overridingness," since non-moral value judgments can share the other two properties; yet when explicating overridingness, the arguments Hare uses are only from *de facto* rather than *de jure* usage.

My point here is that there is a widespread movement among ethicists who previously embraced forms of intuitionism, to make a "formalistic" turn and thereby deny that intuition any longer has a role to play in their systems.67 This makes an interesting base for

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66 "If pressed to justify a decision completely, we have to give a complete specification of the way of life of which it is a part...If the inquirer still goes on asking 'But why should I live like that' then there is no further answer to give him, because we have already ex hypothesi said everything that could be included in this further answer. We can only ask him to make up his own mind which way he ought to live: for in the end everything rests upon such a decision of principle. He has to decide whether to accept that way of life or not: if he accepts it, then we can proceed to justify the decisions that are based upon it; if he does not accept it, then let him accept some other, and try to live by it." *LM*, p. 69; emphasis is mine.

This opens up questions of influence from the Wittgensteinian ideas of "ways of life" and "language games". There is also an implied view that all justification is within a framework, and that questions about justifying the framework itself are non-theoretical and non-cognitive. These are questions into which we will expand in the next two chapters. For criticism of this line of thought in both ethics and epistemology, see W.W. Bartely, *The Retreat to Commitment*, and R. Trigg, *Reason and Commitment.*

67 In a statement underlying the main theme of "critical thinking" developed on the basis of universal prescriptivism, Hare says, "Critical thinking consists in making a choice under the constraints imposed by the logical properties of the moral concepts and the non-moral facts, *and nothing else.* This choice is what I use to
comparison of trends in jurisprudence and metascience, where writer’s like Rawls and Laudan, respectively, make somewhat analogous turns (Chapter Five below). Hare’s honorifically termed "formal property" of overridingness appears to dissolve upon critical examination, and this serves to raise further doubt about the prescriptivist’s linguistic categories and dichotomies. Of course this problem of separating moral from non-moral value judgments is a problem for any meta-ethical theory. But it remains important that the question why people "ought" to place moral goods over personal (e.g. egoistic) ones, is not coherently handled by Hare’s appeal to ‘formal properties’ in even his most recent versions of what he calls universal prescriptivism. Hare’s Kantianism (as well as the formalistic twist he brings to his utilitarianism\(^68\)) still stands at odds with his explication of prescriptivism. Perhaps any meta-ethics will find itself unable to explicate the worthiness of its favored prescriptions, so long as these are presented in terms of the comparative value of one class of prescription over another. One must, it seems, know how to evaluate different articles on a single scale before presuming to know how to compare that scale against another.

But we still want to ask how Carnap and the logical empiricists intended to analyze good/ought/right. As the passage from Carnap illustrates, it was all and only categorical or unconditioned uses of value-terms which were considered so troublesome in the Vienna Circle. And it was to such intrinsic value claims that the non-cognitivist accounts were intended to apply. This would be the class not ‘obviously empirical’ in Carnap’s words. Both

call a decision of principle." *Moral Thinking*, p. 40. This is clearly a contemporary form of the "empirical method" of Stevenson. But its dependence upon the decisions of principle have plagued Hare, particularly in his treatment of the problems of fanaticism, amoralism and weakness of will.

\(^{68}\) "What has happened is that the logical constraints have, between them, compelled us, if we are to arrive at a moral judgment about the case, to coordinate our preference into a total preference which is impartial between us. The claim is that this impartial preference will be the same for all, and will be utilitarian." *Moral Thinking*, p. 227. See also p. 42.
Carnap in *Logical Syntax*, and Ayer in *Language, Truth and Logic* take this to mark off the same class, namely "philosophical, or normative ethics."

But here we see that some of the same problems discussed in the previous sections reappear in the issue of a dividing line between moral and non-moral valuative judgments. Prasad's passage in the last section points out that most writers wanted to restrict their noncognitivism to "moral judgments" per se. But he was not critical of the common assumption of the possibility of identifying such a class on a formalistic basis, and therefore also was not cognizant of his own inconsistency in his claim to restrict the noncognitivist account to such a subclass of value judgments. Carnap and Reichenbach spoke as if the noncognitivist account fit only "moral judgments," but simply defer to the Kantian/Moorean characterization of such judgments as intrinsic value claims when characterizing this class. Ayer, on the other hand, was more aware that the non-cognitivist account could not be so easily restricted without involving the empiricists in the metaphysics of the ethicists they meant to criticize. Ayer was willing to use the broader functional attribute of normativity as the distinguishing mark of the non-cognitive. But he was also aware of the consequences of this move, and accepted the logical consequences. It means that

> our conclusions about the nature of ethics apply to aesthetics also. Aesthetic terms are used in exactly the same way as ethical terms. Such aesthetic words such as "beautiful" and "hideous" are employed, as ethical words are employed, not to make a statement of fact, but simply to express certain feelings and evoke a certain response.⁶⁹

Ayer's confession is quite significant for us here. The lack of objectivity which Ayer attributes to aesthetic judgments along with valuative judgments in the more traditional "moral sense" raises problems for the logical empiricists. It becomes crucial to ask how the logical

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empiricists are to clearly distinguish for separate treatment those value judgments that are involved in scientific practice and decision-making. For the acceptance of the equation of normative function with non-cognitive meaning further complicates the sharp distinction logical empiricists insisted upon drawing between "normative" and "empirical" types of judgments. In many of the borderline cases, the verifiability that is taken to distinguish the latter judgments cannot be verification apart from evaluation, i.e., cannot be logical or empirical in any straightforward sense. If the "vice" of an action is not an observable aspect of the action but an evaluation of it, as Hume insisted, Ayer may be led to conclude as much about a far broader range of virtues and vices --including those of scientific theorizing.

Consider also what Hare calls cognitive (primarily descriptive) but "secondarily valuative" predications like "industrious," and "honest." Are these clearly of one type of meaning or the other? Intuitions may vary. And more importantly, they may vary because such predications are often comparative, lack strict criteria of application, and depend upon valuations as well as upon observations. Clearly, such "secondarily valuative" terms are not taken as virtues in themselves, even by the most committed moral realist. To treat them as Stevenson does --for instance by rendering "industrious" as "works hard; I approve of this"-- actually cuts these characteristics off from the longer term ends they sub-serve. But to so constrict them is to obscure the sense in which they are human virtues at all. Here the moral realist and the moderate cognitivist agree that the prescriptivist analysis denatures the nature and logic of such terms.

In Chapter Two we will consider the broader effect of the positivist movement on other fields such as jurisprudence. The popularity of "legal positivism" around mid-century is another indication of some the same influences discussed here. But a central point can be formulated already. Few of the logical empiricists were willing to consider directly the status
of the kinds of predications that are the central focus of this dissertation. The terms of
cognitive evaluation are all what Hare calls "secondarily valuative" terms. Terms of cognitive
valuation must be characterized on their own model as being mixed-mode. *It will be
extremely important for my later chapters that Stevenson concedes that the "empirical method"
he advocates is not useful in analyzing examples of mixed-mode terms or the kind of
disagreements that are most common in the philosophical usages of such terms.* Logical
empiricists have been quite slow to recognize the extent to which their own account of
metascientific standards enact just the sort of "persuasive definitions" Stevenson says are
common in the use of mixed-mode language-use. Logical empiricist metaphilosophy, as I
argue in Chapter Two, actually develops on the shoulders of a kind of *persuasive contrast of
science and ethics.*

In the few places in contemporary meta-ethics where the hypothetical/categorical
distinction comes finally to be seriously challenged (as for instance in Philippa Foot’s
'Morality as a System of Hypothetical Imperatives'), the alternatives offered are usually
overtly relativistic ones. They are motivated by the idea that the rejection of the class of
intrinsic goods leaves ethics to begin and end with a descriptive psychology of desires. To
*retreat to "psychological set" explanations has been the common remedy for the symptoms of
failed foundationalism. This is exemplified even in Kant’s own skepticism about the openness

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70 Stevenson’s noting that terms of dual descriptive/emotive meaning are the most prone to manipulation by
"persuasive definition" is relevant as well. He concedes that disputes concerning this class of terms (though he
underestimates both the breadth of this class and the epistemic relevance of such disputes) are not amenable to
solution by the "empirical method".

71 Note that there has often been an assumed intrinsic/extrinsic distinction applicable to desires as well:
extrinsic desires depend on beliefs and hence on reasoning about these desires; the more primitive intrinsic desires
(desire to eat) do not depend on the agent’s beliefs and hence cannot be reasoned about. This distinction seems
valid enough, but is not particularly interesting here since what are called (somewhat misleadingly) intrinsic desires
are supplied by our empirical natures and are not claimed to be grounded a priori.
to reason of any goals related to actual human desires and interests in contrast to sole concern for rational agency. It is also, as we will investigate more fully in the next chapter, implicit in the logical empiricist's descriptive or "positive" approach to understanding the language of ethics.\textsuperscript{72}

\textsuperscript{72} "Rejection of the dualism of reason and emotion carries with it an abandonment of the dualistic theory of discourse, according to which emotive and cognitive meanings are logically (though not always causally) independent." -A. Kaplan, from Schilpp, p. 846.
2.1 The norms of thought and action

The intuitionist metaphilosophy of G. E. Moore took a largely realist, or for our focus it is best to emphasize "foundationalist," approach to both science and ethics. Moore took science as paradigmatic of genuine knowledge, and set out to show that ethics does or could possess the characteristic marks of scientific objectivity. Identification of "moral truth" was presented as proceeding scientifically through the special faculty of moral intuition and normal patterns of logical inference.

The logical empiricist (LE) metaphilosophy by contrast viewed the objectivity of science and of ethics quite disparately, leading to claims of a demise of the notion of 'moral science.' A radical selective skepticism towards metaphysics accompanied a metaphilosophy contrasting a foundationalist account of normative science, with anti-foundationalism about normative ethics. LE metaphilosophy offered to "eliminate metaphysics" and to scientize philosophy, but only by distancing it from most of its traditional concerns and redirecting it to a narrowed range of issues concerned with the "explication" of terms in the language of science. Given the importance Barry Stroud has attached to skepticism in the LE tradition, one should not be surprised to find this issue enmeshed with the re-emergence of the realism/relativism
debate. Logical empiricism at least provided a way to overcome this longstanding opposition, even if that way, as I contend, depended upon a highly problematic metaphilosophy. The question now in the realism/relativism debate as I would put it is whether the "post-positivist" philosophers have enough resources to deal with that issue themselves; or whether the crucial lesson of the LE’s failure remains lost on us, by our inability to establish our metaphilosophy on any less suppositious ground than the LE established theirs.

In Carnap’s classic statement in the forward of *The Logical Syntax of Language* (*LSL*, 1934), "Philosophy is to be replaced by the logic of science -that is to say by the logical analysis of the concepts and sentences of the sciences..." But there is a fundamental ambiguity, critics have noted, in Carnap’s notion of the logic of science. Carnap, Russell and others interpreted logical meaning as analyticity, but also seemed to imply, via the theory of meaning as use, that the explications of the norms of the logic of science were descriptive of actual scientific usage. Logic in other words was explicitly a *system of logical entailments*, but in the thought of logical empiricists usually doubled as a *theory of inferences*. The ambiguity is important, because the assumed distinction between normative judgment and logical entailment is basic to the LE metaphilosophy if anything is. If this difference is bridged in the case of the norms of science, then it should either have been argued explicitly or else appears to be fudging. But neither Carnap nor Russell appear to have had any explicit arguments for the slide, and indeed appear to have been initially rather insensitive to the problem. If this is correct, it leads us to view the technical term "explication" as something of an honorific label for a privileged treatment of the norms and language of theoretical science.

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1 "On Carnap’s view, the aim of the logician or methodologist of science is precisely that of providing an explication of the meta-scientific concepts used by scientists. Philosophical theories of "evidential support," "explanation," "confirmation" and the like were all regarded by Carnap as explications of existing scientific usage." -Laudan, "Some Problems Facing Intuitionist Meta-Methodologies," p. 120.

42
All the "special problems" with justification of methodological principles and appraisal norms claims had to do with normative practices that lay outside of science. Clearly, the selective skepticism of the LE metalphilosophy was manifested in its contrast of theoretical science and pragmatic discourse—between science and science policy. Any ambiguity between the normativism of LE metascience and their own acknowledged aversion for normative theory was taken to be avoidable by noting the unique foundations of scientific norms. For knowledge claims from fields that lacked the secure empirical footing of science, however, the LE metalphilosophy invoked an entirely different interpretive or explanatory framework.

These two metalphilosophical paradigms I will simply call comprehensive and selective foundationalism. While oversimple in some respects, such a classification of views will be useful for us because it cuts across differences between long-standing oppositions such as those between consequentialists and intentionalists, or again naturalists and intuitionists. The LE metalphilosophy was also deeply intuitionistic in certain respects, at least as concerned the norms of thought in contrast to those of action; nor is there agreement on what position on the objectivism/relativism debate a thoroughly "naturalized epistemology" leads to. But we would be remiss to consider foundationalist metalphilosophies as uncontested; we can contrast at least two non-foundationalist paradigms. One such basic outlook I will call skeptical non-foundationalism, the view that not only does the challenge to foundations extend even to scientific method and practice, that is to say, to the revered norms of thought, but that the

\[2\] "Comprehensive" is probably too strong a term here, especially to fit Moore. He pushed aside the idealists and neo-Hegelians he identified with metaphysics, and was as skeptical of their knowledge claims as the non-cognitivists were of his. I use "comprehensive" to refer to foundationalist views not about all knowledge claims, but at least science and ethics, and probably in most cases political philosophy as well. Moore is important as a transitional figure; he tried, quite unsuccessfully according to his noncognitivist critics, to walk a tightrope between naturalistic empiricism and the "metaphysicians" he criticized.
consequences of a successful challenge to foundationalism makes the case for cognitive relativism. Often in recent literature in the sociology of scientific knowledge (SSK), for instance, a radically constructivist and anti-normativist orientation leads to explicit acceptance of relativism with regard to norms and norm governance in all social practices, including science. I discuss these views more fully in relation to the "historical turn" in philosophy in Chapter Four.

My own view falls into a camp other than those of Moore and his logical empiricist critics. Nor is it captured by cognitive relativist alternatives to foundationalist metaphilosophy. It indicates a non-foundationalist account of normative practices which is *pancritical* rather than relativist in orientation. I will use "pancritical non-foundationalism" to cover the whole range of non-relativist views indicating a need for transformation in our conceptions of the philosophy and epistemology.

My own particular version of pancritical metaphilosophy, what I call "pancritical pragmatism," is difficult to characterize; some may regard it as global anti-foundationalism, others as an attempt to work out a sophisticated cognitivism about both science and ethics. It says essentially that the attempt to conceive ethics as a science succumbs to certain "popular" conceptions of the objectivity of science that do not bear philosophical scrutiny. But although ethics cannot fit the popular picture of scientific objectivity, this has no overtly...

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3 The editors of *After Philosophy: End or Transformation* (Baynes, et al., 1987) organize the selections into three sections: 1) The End of Philosophy (where they include Rorty, Foucault, Lyotard, Derrida), 2) The Transformation of Philosophy: Systematic Proposals (Davidson, Dummett, Putnam, Apel, Habermas), and 3) The Transformation of Philosophy: Hermeneutics, Rhetoric, Narrative (Gadamer, Ricoeur, MacIntyre, etc.). What I call "pancritical" non-foundationalism might include all non-relativist reformism. Hopefully, my pancritical pragmatism is something of a systematic proposal. See also Cohen and Dascal, eds. *The Institution of Philosophy: A Discipline in Crisis?* (1989).

4 I here attempt to paraphrase the topology Brink lays out in his first chapter. Brink identifies himself as a defender of the first (what I call Moorean) metaphilosophy.
relativistic consequences if that picture, in its foundationalist and deductivist orientation, is not philosophically supportable or is at odds with actual scientific practice. Once we understand the naivety of the ‘received views’ of scientific objectivity, even some of the most cherished among the swarms of dichotomies and distinctions the LE drew lose their appeal. The original intention of James, Peirce and Dewey are to me among the best examples of American pragmatism, and my "pancritical" pragmatism is an attempt to develop some of the pancritical strains of thought that can be found in the pragmatist tradition. These strains I outline and briefly develop through discussion of Dewey's "theory of valuation" in section 2.4 below.

The present chapter examines some classic examples of the LE argumentative strategies with respect to their contrast of the "language of science" with the "language of morals." Section 2.1 addresses the construction of these two languages, while Section 2.2 draws upon this to underscore the contrasting logics of "explication" applied to each. What is most significant for us about the LE metaphilosophy is not merely that the LE viewed logical analysis as an "explication" of meanings, but that they were committed to contrasting logics for explication of these two presumptively exclusive and exhaustive kinds of meaning they recognized. This issue of explication cannot be treated completely independently of logical empiricist assumptions about explanation, in particular the N-D conception of explanation that logical empiricists often took as a defining characteristic of scientific method. The syntactical and semantic categories that the logical empiricists employed --the cognitive and the non-cognitive, etc.-- were accompanied by, if not pre-structured by, views about how the type of content within each category would be explained: explained, that is, in nomothetic or nomological science (I use these two words interchangeably) --the idea being that scientific explanation has recourse only to observable facts, universal empirical laws, and logic.

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If this is correct, then it may be suggested that *explication and explanation are two sides of the same coin within the LE metaphilosophy*. My primary focus concerns not the issue of the demarcation of science, nor the analytic/synthetic distinction, but this peculiar *explanatory framework* that accompanied the construal of these issues, as found in *LE* treatment of the content of beliefs. The dichotomous conception of explanation that accompanies the syntactical and semantical accounts in logical empiricism I will call *methodological nomotheticism*, and attempt to clarify in the sections below.

What I call methodological nomotheticism is closely related to the thesis that all statements neither analytic or empirical can only be scientifically investigated when reduced to (or reformulated as) the statements of descriptive psychology or sociology. This marks as well the importance of the logic/psychology distinction in the logical empiricist understanding of explanation. It was from this distinction that one might say the explanatory framework of methodological nomotheticism gets its dichotomous or dualistic character. The reasoning behind this assumption would seem to be reconstructible as follows:

(1) a nomological or nomothetic view of science demands that scientific statements be either analytic or synthetic; but

(2) all statements about value on the non-cognitivist theory of value have a status for nomothetic science only as descriptive psychological/sociological statements about the actually held beliefs or dispositions of a person or group of persons.

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5 Compare Rorty's (1967) classic discussion of "methodological nominalism," the view that all cognitive sentences about intensional entities can be reformulated into either questions about observable entities, or else questions of syntax. This reductionist tenet is itself one aspect of the nomological-deductive (N-D) conception of explanation that the logical empiricists held was co-extensive with *scientific* explanation: the reformulation or reduction Rorty focusses upon as a dogma of empiricism is one that is necessitated by the very need to exclude content not amenable in form to a nomological-deductive explanation. Hence, what Rorty discusses as methodological nominalism is not on my view the deepest dogma of empiricism; if I am correct the assumption I am reconstructing as methodological nomotheticism is a deeper dogma still.
(3) Therefore, the explanation of valuative utterances in nomothetic science must regard the needed universal empirical laws as laws of a \textit{psychological kind}.

We could of course expand the discussion of explanation to include not only the contrast of scientific discourse with the discourse of ethicists, but with that of juridical, aesthetic and other "non-scientific" inquiry as well. In these fields also, the effect of the \textit{LE} metaphilosophy has been striking.\footnote{For a contemporary treatment of norm governance in the juridical field, see J. Raz, (1991) \textit{Practical Reason and Norms}. "Legal positivism" was a strong movement for some time, and still exhibits influence on contemporary theory. The distinction between natural and positive law is an old one, however, and the term "legal positivism" is only loosely connected to the logical positivist school. In the sphere of politics one finds the influence of a distinction similar to that in ethics. It is sometimes said that the proper concern of political philosophy is with "second-order talk about politics" as contrasted with "first-order political principles" expressing basic political beliefs or judgments. -Gewirth (1960a), p. 187.} This influence is at least twofold: first, on the perceived need to distinguish sharply between "normative" and "meta" level discourse in any field\footnote{Many who draw this distinction, Gewirth says, "assimilate this distinction to such other distinctions as those between science and the philosophy of science (or, variously, epistemology), or between object-language and metalinguage, or between first-order and second-order statements, in each of which, similarly, the philosopher's business is said to lie in the second, 'meta' discipline." -Gewirth (1960a), p. 187.}, and second, on the methodological nomotheticist view that the content of normative theory has scientific status only as ascriptions of dispositions or descriptions of normative commitments or habits actually held by human subjects. Methodological nomotheticism as I have defined it is closely linked with the logical empiricist doctrine of "operationalism."\footnote{I certainly think this point would be accepted with regard to the logical empiricist view Rorty characterized as "methodological nominalism," and is part of the reason Hanna said Rorty's notion did not go far enough, but should be placed into a broader account which Hanna distinguishes as "ontological nominalism." The narrowness of Rorty's conception is I think reflected in his own positive metaphilosophy.} Operationalism is
the view that saying what an object is, giving the *Bedeutung* of an object sign, reduces to an operation of *stating the truth-conditions* for the sentences in which the sign occurs.⁹

These are some of the epistemological assumptions underlying methodological nomotheticism. But they are also those that have received the most attention among epistemologists, who have accordingly I think missed some of the big picture. This is why I will continue to focus on the semanticist and selective foundationalist assumptions at work. It is really in putting the semantic, epistemological and metaphilosophical views together that allows us to see how deeply seated in Modernist tradition is the view of scientific explanation I have called methodological nomotheticism. A dualist framework of explanation is as basic to Modernist tradition as deductivism itself. By raising concerns about this explanatory dualism, we can call into question to some extent the rules of the game of "unified science," particularly those rules which concern the methods of epistemology and the social sciences. In this way, the possibility of models of explanation in science far less restrictive than that which methodological nomotheticism represents.

The effort to place normative metascience back into a broader and more varied class of social practices is advanced by seeing research traditions in science as akin to those in other fields. Research traditions represent *normative systems of discourse*. The primary obstacle here is that, in the LE metaphilosophy, science was modeled as a kind of formal or *logical system*, a system containing only analytic and stipulative definitions along with descriptive claims. This model of science was explicitly intended to contrast with the a model of *normative systems*.

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⁹ "To say what an object is or, in other words, to give the *Bedeutung* of an object sign, is simply to give the truth-criteria for the sentences in which the sign occurs." *Aufbau*, p. 222. See Coffa, p. 149. This, in turn, was closely associated in logical empiricist thought with the basis of positivism, the verification theory of meaning.
Several factors involved with this contrast merit special notice. One is that such guiding values as "completeness" (or closure) and "consistency" play a rather different role in normative systems than in formal ones. Given the influence of the LE metphilosophy, it is not surprising that the current literature in jurisprudence is full of debates concerning whether there are any genuine 'normative gaps' (e.g. lacunae in the law) in functioning normative systems. But the existence of normative gaps, that is, of problems or conflicts not correlated with a solution (e.g. established statute law), was typically denied in science when taken on analogy with a logical system. Science on this account is modeled as a closed or "axiomatic" system.

This leads us to the important difference in the role of consistency in the two kinds of systems. In normative systems, there is rarely a clear hierarchy or ordering of reasons for issuing imperatives; the notion of a best possible reason for issuing a norm is somewhat vague. Furthermore, noting an inconsistency between norms within a normative system has a different consequence than noting inconsistency between indicatives.10

What is particularly revealing here is that normative conflict is an inescapable fact about normative systems, ethical and legal systems being prime examples.11 What is suggested in section 2.2 is that the importance of such conceptual issues in science was systematically de-

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10 This is one conclusion of H. Hill, in his dissertation entitled "Normative Conflict in Legal Systems: A Functional Analysis," Washington University, St. Louis, 1985. I am indebted to Hill's dissertation for much of this discussion of consistency and completeness. One of the primary insights of my pancritical pragmatism is that contradiction within a normative system differs from contradiction within a system of descriptive statements or purely empirical beliefs. As we shall see in Part III, this can be made one basis for distinguishing relativism from pluralism, and for developing a pluralistic account of explanation far different than the dualistic account of the logical empiricists.

11 A distinction is often drawn between strongly and weakly consistent systems. This is generally understood in terms of a "joint compliance test" that serves to reflect on obedience statements for a pair of norms, principles, or "rules." Differences over this often involve even the source and legitimacy of strong consistency as a goal for normative systems. Hill argues that consistency is a pragmatic rather than a logical requirement, and that on pragmatic grounds weak consistency may be more desirable than strong consistency.
emphasized through a rhetorical strategy on the part of logical empiricists to push a *persuasive* picture of science as an axiomatized formal system. The view that closure and strong consistency are requirements for rationality of discourse may even derive from the attempt to reduce normative systems to logical systems, and rationality to logic. Rejecting the ideals of closure and completeness, and being clear on the practical tradeoffs between theoretical consistency and theoretical competition would underscore the importance of ampliative processes in norm governance. It has been one of the primary contrasts of normative metascience with other normative practices has been that there is no reasoning not reducible to logic and evidence in the former, as there is in the latter. But "in juridical reasoning," as Will points out, "ampliative processes are especially prominent and unavoidable" (p. 181).

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12 "Viewing a scientific theory as an axiomatized formal system, a distinction is made between two parts of the formal system: the body of theoretical propositions which is formulated solely in the theoretical vocabulary, and a set of 'correspondence rules' which connect functions constructed out of the theoretical terms with observation terms." -H. I. Brown, *Perception, Theory and Commitment*, p. 46. The later work of Carnap and other logical empiricists constituted a major weakening of this program.

13 Note that while the Kantian tradition contrasted mathematical reasoning and logic, the logical empiricist tradition assimilates them. This is one important source for the operationalist reduction of reason or rationality to logic, a view which Continental thought has largely resisted.

14 "The main contrast for which Peirce employed the term 'ampliative' is that between kinds of argument (inference, reasoning)...Departing somewhat from Peirce's usage, his terms 'deductive' and 'ampliative' seem better than available alternatives (e.g., 'analytic' and 'synthetic') for drawing a contrast between two ways in which effects upon norms are produced in governance. One way is that of deriving effects as end products of a succession of steps each of which represents the application of prior accepted norms. This method is distinguished here as 'deductive.' ...Complementary to the deductive phase of the employment of norms, and here termed 'ampliative,' is a phase in which effects are wrought upon norms themselves in conjunction with their applicative use." -Will, p. 5.

15 Will avoids the term "argument" when referring to ampliative processes. But I see no reason not to use it for at least the subset of ampliative inferences he calls the philosophical reflective. An argument in common usage is just where reasons can be given or are given. Hence it is where claims about evidential relationship are present explicitly, and there need be no further requirement for the use of the term "argument" that the relationships in question are deductive ones. Indeed as I have held, if the inference involves positions on conceptual issues, then *ipso facto* it depends upon both empirical and normative considerations. Common usage is not a bad guide here, since the term "argument" is rarely restricted to deductive relations. If simple deduction can settle an issue,
Little wonder then that the selective foundationalist metaphilosophy since antiquity has instantiated so broad and deep a division between norms of thought and of action!  

Later in the dissertation we will have an opportunity to speak about norm governance and the problem of ampliative inference in somewhat broader terms. But in order to keep the development of my argument focussed on the LE metaphilosophy, I will confine this examination to the contrast of science and ethics, continuing to draw from Reichenbach, Carnap, Feigl and other logical empiricists, as well as from the ethicist Stevenson, because of his special place in discussions of these issues on the American philosophical scene though mid-century. In addition I will relate the work of several notable critics, such as Alan Gewirth, David Brink, and Stephen Nathanson, and one contemporary defender of a scientific objectivism conjoined with moral relativism, Gilbert Harman.

Hans Reichenbach’s discussion of the languages of science and ethics in *The Rise of Scientific Philosophy* is especially perceptive because of the historical viewpoint he brought to his development of the emergence of "scientific philosophy." For Reichenbach "There is no compromise between science and speculative philosophy" (1951, p. 73). The target of his criticism was "the ethico-cognitive parallelism, the theory that ethical insight is a form of cognition, that is, of knowing." The double meaning of "moral law" as divine command

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16 "[The] controversies over the application of causal categories to human action that pervaded philosophical thought during the past few centuries are no mere semantic ones that might possibly be resolved by agreement among the disputants about the usage of the word 'cause' and other cognate and closely related terms. Involved in this [division is] Kant's judgment that implicated in it is a division between two utterly different organons of 'reason'." For Will, by contrast, deductive and ampliative aspects of philosophical reflection should be thought of as abstractable phases out of concrete practices of norm governance in which both are present. -Will, p. 2.

17 Reichenbach, op. cit., p. 52.
and as rule of nature, he argued, bears witness to the construction of this parallelism. The liberal side of 20th-century non-cognitivism (including in the broad view much existentialist ethics) is seen in its force as a corrective to the errors of a long tradition of casuistry and ethical absolutism. Reichenbach and the logical empiricists are also reacting against the problems they saw in Hegelian humanist idealism. Objectivist forms of humanism were particularly strong in the 19th century, and it is in response to the robust metaphysical intuitionism of that century that 20th century empiricists made the turn towards a more Humean epistemology and a selectively foundationalist metaphilosophy.

Spinoza and Plato are also foremost among the targets of Reichenbach’s polemic. Spinoza’s axiomatic construction was meant to give his ethics a deductive form and a foundation in reason analogous to the footing of geometry. For Socrates in the *Meno*, virtue is a form of knowledge in a sense made analogous to that in which geometry is a form of knowledge. Here, if we can frame it in Kant’s terminology, ethical insight is an a priori and synthetic form of knowledge. Reichenbach’s polemic against the ethico-cognitive parallel is stated as follows:

One conclusion can be immediately drawn from the analysis of modern science. If ethics were a form of knowledge it would not be what moral philosophers want it to be; that is, it would not supply moral directives. Knowledge divides into analytic and synthetic statements; the synthetic statements inform us about matters of fact, the analytic statements are empty. What kind of knowledge should ethics be? If it were synthetic, it would inform us about matters of fact. Of this kind is a descriptive ethics which informs us about the ethical habits of various peoples and social classes; such an ethics is a part of sociology, but it is not of a normative nature. If ethics were analytic knowledge, however, it would be empty and could not tell us what to do either.18

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18 H. Reichenbach, op. cit., p. 276-7. Kaplan notes, "Russell and others have frequently pointed out that there is an imperative element in assertions, enjoining belief. In addition to the direct cognitive meaning of the proposition asserted, there is the derived cognitive meaning of the existence of good reasons for asserting and believing it. And this derived meaning relates to the normative function of the assertion. Overlooking this function in assertions is the complementary error of overlooking the cognitive meaning in statements of norms."
Reichenbach’s intention is to show that the cognitivists are on the horns of a dilemma: their principles could not be analytic, because then they could not perform the prescriptive tasks expected of them; but they could not be synthetic either, because then they should be adjudicable by empirically fact, but are not. Cognitivism, Reichenbach proclaims, is thus nothing more than "the two thousand year error that knowledge contains a normative part."

The modern analysis of knowledge makes a cognitive ethics impossible: knowledge does not include any normative parts and therefore does not lend itself to an interpretation of ethics... The two-thousand year old plan to establish ethics on a cognitive basis results from a misunderstanding of knowledge, from the erroneous conception that knowledge contains a normative part. 19

What Reichenbach may have shown is that Plato, Spinoza and Kant share in common suspect strong versions of value cognitivism. In the literature of meta-ethics these views have recently received attention under the title of "moral realism." 20 Moral realism is the affirmation of "value facts" to be discovered by one or another type of privileged representation. Moral facts can be stated and assigned truth value. 21 Reichenbach’s critique has historical perspective, and I think some force against the historical figures he is most

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19 Reichenbach, op. cit., p. 52.


21 Interestingly, Stevenson and Dewey both wanted to say "truth" and "justification" of moral claims, while Stevenson at least denies moral reasoning involves "argument" or "validity." I am likely to affirm just the inverse of this, since I think we can talk about "valid" principles and well or poorly "argued" ethical/juridical perspectives, without presuming that these represents actually existing state of affairs or "value-facts." Dewey’s notion of value-facts makes sense only when we see him as a coherentist. This gets into the difference between coherentism and correspondence, and Brink makes clear that much cognitivism is coherentist. This would be my position as well.
concerned with, though much less against a sophisticated moral realism such as that of David Brink (1989). But Reichenbach treats the thesis of cognitivism far too narrowly in arguing that the criticisms of Plato, Spinoza and Kant should lead us to reject cognitivism altogether. There are cognitivist accounts which make moral principles represent neither a priori synthetic truths, nor analytic truths, nor truths intuitively known. There are cognitivist accounts (both naturalist and intuitionist) that are aligned with what Brink calls moderate coherentism, and are not foundationalist at all. Part of the problem is, of course, that the cogency of any such alternative account will be dismissed because it will jibe with the particular interpretation of the analytic/synthetic distinction that empiricists like Reichenbach held: one that bloats the notion of analytic reason in claiming it able to structure the content of belief, and which shrinks the notion of the synthetic to ostensive facticity.

The conditions set into the dilemma Reichenbach posed are derived from foundationalist assumptions; they are no more open to question than is the logical empiricist’s interpretation of the analytic and the synthetic. Indeed they are identical with this, so that Reichenbach’s polemical statement above contains very little actual argument at all. Each horn addresses a different failure to fulfill the demand that the ethicist prove his/her axioms. This demand on ethics arises from a deductivist epistemological orientation together with a view of science as an axiomatic system containing stipulative and analytic definition, but not the persuasive definitions of ethics. The dilemma depends crucially on a view of practical and persuasive uses of language as functioning altogether different from theoretical (analytic and synthetic) ones. If analytic, ethical rules would be deprived of their "imperative character"; surely

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22 Aristotle pointed out that the subject matter of ethics is contingent in its concern with the effects of volitions, human habits, and institutions. Gewirth sees this as limited to a difference in the modality of ethical and natural-scientific propositions and arguments. Brink writes, "The practical character of morality is often thought to call for an anti-realist, especially noncognitivist, construal of moral claims. If moral judgments purported to state facts, it is claimed, they could not fulfill the action-guiding function they do. To fulfill this function, moral
not, responds Kaplan, unless prescriptive character (function) is identified with a special kind of imperative meaning. Medicine is knowledge, but this does not deprive rules of hygiene of their imperative force; rather, rules owe their imperative force to the reliability of the knowledge on which they're based as well as to the practical reasons for issuing a norm.

Although Reichenbach claimed not to assume a verification theory of meaning, his statement relies at least on the assumption that "knowledge divides into analytic and synthetic," and on assessing the legitimacy (if not the meaning) of knowledge claims on this basis. The only kind of "fact" the non-cognitivist will recognize about valuative commitment is just the descriptive psychological fact that such commitments are made, or that behavior appears to presuppose certain norms, habits, customs, etc. This view, as we will see in Section 2.2, is more a result of a peculiar dualistic scheme of explanation the logical empiricists adopted rather than a requirement of empiricism generally.

Finally, it is certainly true that non-western and pre-modern western philosophical traditions do not share with the empiricists of Modern tradition their radical opposition between man as a cognizer and as a profferer of value. But this may only make us more suspicious of the adequacy of Reichenbach's scientific philosophy. Twenty-century LE represents historically the high-water mark of this prosaic picture of man and the epistemology based upon it.

judgments must concern or express affective, fundamentally noncognitive, features of people's psychology." - Brink, p. 37. Brink argues that this argument from function to meaning is ineffective, being based on dubious epistemological and semantic assumptions. I see Hume Treatise III, i, is a forerunner of the argument Reichenbach uses.

23 "After two hundred years of intermittent exposure and criticism, the attachment of philosophers in the Anglo-American tradition to deductivism remains strong. In ways that we are hardly aware of we are influenced in various steps we take in governance by the deep-seated preconception that to whatever extent issues of governance transcend the competence of deductive, applicative processes, they transcend the competence of philosophical governance altogether." -Will, p. 13.
Although expressly anti-positivist in certain respects, C. L. Stevenson nonetheless embodied some of the predominant LE conceptions of meaning and knowledge. This is evident even in the fundamental division of his meta-ethics, the distinction between "disagreement in belief," and "disagreement in attitude." His own contrast between the language of science and the language of morals follows closely upon this and indeed is tied definitionally to the two formally different forms of disagreement. "It is disagreement in attitude, which imposes a characteristic type of organization on the beliefs that may serve indirectly to resolve it, that chiefly distinguishes ethical issues from those of pure science" (EL, p. 26).

In his more qualified statements, Stevenson concedes some relevance of attitudes in science.

There is unquestionably a possibility that interests in knowledge should be opposed, and lead to valuative controversy, within science itself, about what is worth speaking of, or what classifications or distinctions are worth making. At times these issues are complicated enough to stand in the way of scientific agreement, and must be debated by many of the methods that we have illustrated for ethics. But there is no occasion for philosophical fear, on this account, that science totters. The valuative aspects of science involve only interests in knowledge, and these constitute a limited range of attitudes in which opposition is relatively infrequent.

It is not clear from this passage whether Stevenson holds the strong empiricist thesis of Ayer, that given enough time and empirical evidence, 'disagreements in belief' will always disappear. Perhaps Stevenson recognizes an unmeetable burden of proof which Ayer and the

24 *Ethics and Language*, p. 13. Compare p. 26, and 290. "To the question, 'what distinguishes ethical statements from scientific ones?' it has been answered: Ethical statements have a meaning that is approximately, and in part, imperative. This imperative meaning explains why ethical judgments are so intimately related to agreement in attitude, and helps to indicate how normative ethics can be distinguished from psychology and the natural sciences." -p. 13.

positivists bore for this claim. But with regard to what he says about "disagreement in attitude" and the valuative aspects of science, several points should be made.

First, Stevenson's claim that "the valuative aspects of science involve only interests in knowledge" (read 'norms of thought'), can hardly be construed as a straightforward empirical claim. It depends on a number of significant conceptual issues, such as the separability of a class of isolable "interests in knowledge." I have not been suggesting that empirical arguments could not be made to support Stevenson's views. The claim that disagreement among scientists can be known to involve only interests in knowledge appears quite arbitrary and unargued in Stevenson's account, a likely inheritance from the dominant epistemological and metaphilosophical climate of his time.

Second, Stevenson's claim that "these constitute a limited range of attitudes in which opposition is relatively infrequent," as an empirical claim, is probably false. There is no empirical or historical evidence offered to back it up. Third, questionable assumptions appear to be made in claiming an epistemic relevance for the "relative infrequency" of disagreement about valuative aspects of science. How much weight can an argument from comparative rates of frequency be expected to bear? Of course, a high uniformity of belief in our real social-political world can be the result of many factors, including simple uncritical acceptance of views handed down through a community. This is why Will develops an eight-fold topology of norm governance. In order to give Stevenson's contrast any significant force,

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26 In Will's account of norm governance, attributing independent "reflective/non-reflective," "deductive/non-deductive" and "philosophical/non-philosophical" qualities to particular normative practices (or instances of cognitive decision-making), is a contingent matter calling for case-by-case study. "The three characters, 'R' (reflective), 'D' (deductive) and 'P' (philosophical), taken as independent, contingent attributes of governance, yield in the abstract an eight-fold classification." Overview follows p. 158. Presumably such a topology addresses an intersection of functional and causal considerations, and hence yields a matrix for explanation far more pluralistic than the dichotomy between cause and reason implied by the operationalist philosophy and methodological nomotheticism.
not only the relative frequency but the severity and tractability of the two types of disagreement would seem to be epistemically significant.

Does Stevenson's meta-ethics reflect the same "contemplative" model of knowledge production that the logical empiricists are so often accused of? Stevenson like the logical empiricists was aware of objections to the effect that very little fits the models of pure descriptive knowledge and clear linguistic analysis. Stevenson was deeply concerned with the understanding of "mixed mode" utterances, and with developing rhetorical strategies applicable to resolution of disagreements in attitude. Various opponents of logical empiricism including the pragmatists have agreed on the point that these linguistic categories, in claiming to explicate independent "meaning components" in language, implies hypostatization of these units of discourse. Dewey for instance writes that the tendency towards hypostatization has "led to the intensification of the alleged dualism between the 'normative' and the 'descriptive' and the resolute, systematic identification, on the part of the human-moral studies, with what could be termed 'normative itself' and 'normative in authority' because based upon alleged inherent fixities and absolutes." Stevenson at one point conceded that such divisions as he utilizes are, in his own words, "artificial." He says that "Valuative and descriptive meaning...stand in extremely close relationship. They are distinguishable aspects of a total meaning situation, not 'parts' of it that can be studied in isolation."

It is at such points that Stevenson comes closest to a more contextualized account of meaning and function such as that of the pragmatists who were his contemporaries. Yet Stevenson immediately insists (for unspecified reasons) that the artificiality of his central linguistic categories and divisions does not render them suspect. The contrasts between two kinds of meaning and two kinds of disagreement, are the central distinctions of Stevenson's

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27 LW 16, p. 339.
philosophy. But how useful such (admittedly conventional) linguistic categories are as tools of logical analysis cannot be divorced from the adequacy of the persuasive picture of science they are used to support. Once accepted, these distinctions become the basis for a sharp contrast between science and ethics, each discipline now being one that pick up its cards after the selective foundationalist has shuffled and dealt.28

One might then ask if Stevenson's concession to the pragmatists is not mere lip-service. At least his epistemology could not support the reconstruction of ethics he had hoped to produce, as we saw in the first chapter, because it leaned heavily on many of the semantic, epistemic and metaphilosophical assumptions of the logical empiricists. His real differences from the pragmatist comes out in a related discussion of the theoria/praxis distinction, where he expresses strong skepticism of Dewey's notion of "pragmatic" reason. It is a notion we should be wary of, he says, because it is "half attitude, half belief." On my version of normative epistemology, however, the requirement that cognitive commitments29 fall easily into prescriptive and descriptive meaning components is not admissible. Stevenson himself has only utilized it on assumption that it will not be a distortive force. Let us hold Stevenson to his word that his "empirical method" is fruitless when issues are dominated by rhetorical strategies of persuasive definition. For it is certainly persuasive definitions of science and

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28 This contrast is evident even in the definitions of the two types of meaning he gives: "The emotive meaning of a word is the power that the word acquires, on account of its history in emotional situation, to evoke or directly express attitudes, as distinct from describing or designating them. ...A sign's descriptive meaning is its disposition to produce cognitive mental processes, where "cognitive" is to be taken as a general term designating such specific kinds of mental activity as believing, thinking, supposing, presuming, and so on." -ibid, p. 33.

29 On my alternative view, factual claims and attitudes are often differentiable only in context. Both are necessary to the construction and selection of knowledge claims. 'Background knowledge' is not a body of descriptive knowledge, but a complex unity of descriptive, ontological, and valuative assumptions. If this is an ontological implication of pancritical pragmatism, that view is still consistent with methodological pragmatism (cf. Rescher) and bootstrap methods of testing in science.
ethics which Stevenson presents. Despite his own brilliance in analyzing 'persuasive definition' in normative ethical disagreement, Stevenson does not see how far his own metaethical emotivism is still tacitly constructed and argued on the basis of persuasive definitions and contrasts. The circularity of Stevenson's scheme is apparent when he uses the linguistic categories as a basis for defining these disciplines. In insisting on the adequacy of the semantic schemes on which his "empirical method" depends, Stevenson in effect commits himself to fitting the actual character of ampliative reasoning processes in the sciences to a prefigured model of "disagreement in belief."

My conclusion is that the noncognitivist doctrine of primary descriptive and prescriptive meaning components has derived partly from a persuasive contrast of the discourse of science and ethics. By a persuasive contrast I understand the juxtaposition in a meta-theory of two terms defined in what Stevenson called a "persuasive" fashion. Evidence for this contrast has typically been found in considerations of differences between science and ethics in respect of cognitive tractability or decidability of the problems characteristic of each. Stevenson's model as on the positivist's, cognitive or "rational" decidability is made the hallmark of science, even if knowledge is not (as in the scientistic conceptions of the early positivists,) strictly identified with science. The decidability criterion, or as it was called by Feigl, the

30 Very late in Dewey's career (1949) he wrote, "'Persuasion' takes effect in selecting and ordering factual subjectmatter but is not itself any part of the subjectmatter...the qualification...is that 'persuasion' be limited to the intent and office of evaluative judgments, an not be treated as one constituent along with factual evidential constituents in the subjectmatter of the judgment...The function of persuasion and of producing conviction is so far from being peculiar to judgments conventionally recognized to be in the value-field that it is now better exemplified in 'scientific' inquiry and the propositions that result from it. It follows from what has been said that there is nothing whatever that methodologically (qua judgment) marks off 'value-judgments' from conclusions reached in astronomical, chemical, or biological inquiries. Specifically, it follows that the problem of 'the relation of value to fact' is wholly fictitious, since it rests upon and proceeds from assumptions that have no factual foundation." -Dewey, LW 16, p. 357.

"confirmation criterion" of scientific disputes, could be argued for in many ways. The presence of genuine experts, the incorrigibility of the axioms of science, or the tractability of its problems, and the unity of the "scientific method of confirmation," are just a few that we must cite before concluding this section.

Adequately investigating all the empirical issues raised by the notions of decidability or tractability would involve step-by-step comparison of the actual roles played by axioms, aims, warranting evidence, confirmation procedures\(^{32}\) and inferential strategies in contrasted fields of inquiry. These issues are too broad for me to investigate in this study, as are the conceptual issues that have influenced the development of the current divisions between academic disciplines. But neither value cognitivism nor a pancritical metaphilosophy requires symmetry in all respects; this would be to return to modelling ethics as a science.\(^{33}\) We can turn now to examine more directly the explanatory aspects of the LE metaphilosophy.

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\(^{32}\) "From the statement 'Killing is evil' we cannot deduce any proposition about future experiences. Thus this statement is not verifiable and has no theoretical sense, and the same is true of all other value judgments." - Carnap (1935), p. 24-25. The logicist conception of scientific confirmation of hypotheses was that they concerned logic and evidence alone. The background knowledge needed is a body of cumulatively developed empirical knowledge. Giving up this logicist view of confirmation means that background assumptions ("auxiliary hypotheses") are available in both ethics and science to help draw out both theoretical and empirical consequences of a theory. Of course, the criticizability of conceptual hypotheses differs from testing of empirical hypotheses, but neither are straightforward or unproblematic.

\(^{33}\) Compare Gewirth's conclusions: "What follows from this is not that ethics ought to be 'scientific' in the sense of being deduced from science or of using the same methods as science. The burden of the argument has been rather that there should be a parity in the approaches which philosophers take to ethics and to science insofar as those approaches delimit or recognize the respective fields as being constituted by the norms relevant to each." - Gewirth (1960b), p. 328.
2.2 ‘Normative’ science and ‘positive’ ethics

In his (1935) *Religion and Science*, Bertrand Russell wrote,

> In a scientific question, evidence can be adduced on both sides, and in the end one side is seen to have the better case --or, if this does not happen, the question is left undecided. But in a question as to whether this or that is the ultimate Good, there is no evidence either way; each disputant can only appeal to his own emotions, and employ such rhetorical devices as shall rouse similar emotions in others.\(^{34}\)

But are the disputes Russell compares here on a logical par? Is it correct to treat ethics and religion as haggling over hopelessly metaphysical predicates while science concerns itself with the surety of empirical predicates? Surely, at least, not all ethics are "monist" in the sense Russell presumes, that is, they are not all conflicting normative principles claiming to represent a moral *summum bonum*.\(^{35}\) What is important to us is the structure of Russell’s argument. For arguments of this kind have always been behind the *persuasive contrast* of science and ethics. One of the earliest and best criticisms of this contrast and the rhetorical strategies it involves was Alan Gewirth’s (1960) article ‘Normative "Science" and Positive "Ethics."’ He wrote,

> Questions of ‘ultimate’ principles in any field, including science as well as ethics, are not on the same level as intermediate questions in regard to which the relevant principles are assumed; for in the former case one cannot appeal to higher principles in order to answer them. Consequently, in comparing scientific with moral disagreement, Russell should have balanced his moral example about ‘the ultimate Good’ with a scientific example of disagreements and the undecidability of moral ones.\(^{36}\)

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35 William James, "The Moral Philosopher and the Moral Life" argues vigorously against monistic ethics.

36 Gewirth, 1960, p. 312. "The same contrast is found, in principle, also in such noncognitivists as Hare, Urmson, and Nowell-Smith when they emphasize the differences between the meaning of 'good' and the varying criteria of its application, or between moral and empirical predicates. For here too this difference involves the
The Russell passage like those of Reichenbach and Feigl indicate what Gewirth as early as 1960 saw as the great "disparity" in the logical empiricists' dual attitudes toward inquiry: their "normative" approach to science, concurrent with an equally strong insistence on a descriptive or "positive" approach to ethics. For logical empiricism in its heyday, the radical dualism of normative science and positive ethics characterizes its conception of scientific explanation! For the logical empiricists, the demarcationist project of distinguishing science from non-science was closely associated with contrasting explanatory frameworks for each. Gewirth tries to steer between extreme views of ethics or politics as a strict science, and of philosophy of science as strictly politics. But the positive approach to ethics, Gewirth objects, "makes it impossible to do justice to distinctive features of ethics in the way in which the normative approach in philosophy of science does justice to science" (p. 329).

Gewirth was perhaps the first seriously to challenge the rhetorical strategies involved in the LE's defense of this explanatory dualism. These rhetorical ploys involved at times even a standard fallacy Gewirth calls the fallacy of disparateness --"the fallacy of discussing one field on one level or respect and the other field on a quite different level or in a quite different respect" (p. 313). When the logical empiricists categorized types of "science" or characterized "scientific judgments," the interpretive or explanatory framework called for a normative interpretation/explanation. When they categorized types of ethics or characterized "ethical judgments," the demand was to proceed in a "positive" fashion (p. 313). Hence,

\[\text{contrast between the cognitive decidability of scientific disagreements as to whether an empirical predicate is to be applied to some object, and the undecidability of moral disagreements by reference to cognitive considerations alone, as to whether an ethical predicate is to be applied to an object.} \] -p. 312.

Certainly the faulty construction of contrast of Russell's contrast above represents an error too elementary for a logician of his caliber. Gewirth charges that the non-cognitivness of the principles of ethics is itself a consequence of the disparate explanatory logic applied to science and ethics. The fallacy he alleges is, I think, a genuine fallacy, though it is much less clear how to construe its relation to the rest of the LE metaphilosophy.
"They use 'science' on the level of the fulfillment of certain regulative or restrictive norms, while they use 'ethics' without restriction to the corresponding norms" (p. 314).

The LE selective foundationalist metaphilosophy represents a high-water mark in the dualistic program of explanation. Terms like "science" and "ethics" are themselves 'mixed-mode,' and thereby open to persuasive definition. Logical empiricists employed terms like "scientific" and "science" in a regulative or restrictive denotation, while "ethical" and "ethics" are quite explicitly employed positively or non-restrictively. These persuasive definitions and the explanatory disparity that attended them are a direct reflection of the selective foundationalist metaphilosophy. Confronted with the perceived choice between absolutism and relativism about human ends the LE school did what I affectionately call 'a Vienna Two-step': They utilized rhetorical strategies of persuasive definition and contrast to insulate a preferred objectivistic characterization of science and "scientific judgment."

It is clear that in common language, "science" is typically something of an "achievement" term. This is what often makes it appealing to the progenitors of a discipline that is (a) in its infancy, or (b) for other reasons suspect in the general public's eyes, to apply the honorific title of "science" to what they are doing. Witness (a) "cognitive science," and previously, of course, (political science), and (b) "Creation science." In established fields such as physics and biology, by contrast, there is rarely a felt need to reiterate the field's "scientific" status. What is interesting is how the logical empiricist the project of demarcating science could be

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38 "For example, such philosophers do not regard both Christian Science and neurology as examples of 'science,' but they do regard the discourse of both Al Capone and Albert Schweitzer as examples of 'ethical' language." Hence the question arises: why does the philosopher refuse to be neutral as between the 'logics' of the neurologist and of the Christian Scientist, while he insists on being neutral, qua philosopher, between the 'ethics' of the democrat and the Nazi? It seems to be because he regards the normative difference between the 'logics' as a cognitively ascertainable one, while he holds that the normative difference between the 'ethics' is not cognitively ascertainable." -Gewirth (1960b), p. 317.
taken as identifying a restricted range of studies in which achievement is possible. This, as we will see further in section 2.4, is not the direction of Dewey and the pragmatists.  

The problem of the demarcation of science needn't have been identified with other distinctions such as the theoretical/metaphysical or cognitive/noncognitive meaning distinction; many of the Vienna Circle members came to see this either before or after their relocation to American around 1938, prior to the outbreak of the world war. But the project of demarcating science from non-science remained vital to some of the logical empiricists as well as to critical rationalists like Popper. Part of the reason it is important is that a response to a demand for demarcation commits the responder to a normative position as to what constitutes the genuinely scientific. Defining ethics positively meant describing them without regard to whether the product is genuine or counterfeit. This account includes the presumption that the normative difference between good and bad science is an ascertainable one, while the difference between moral philosophies is not. Bereft of tacit persuasive contrasts and the rhetorical strategies Gewirth calls examples of the fallacy of disparateness, a clear argument is difficult to find for the foundational and anti-foundation status of the axioms of science and ethics, respectively. Common language supports a sense in which Nazi 'ethics' are not

39 "Error lies in restriction of the domains of value in which achievement is desirable. It is folly rather than wisdom to include in the concept of success only tangible material goods and to exclude those of culture, art, science, sympathetic relations with others...As John Stuart Mill said 'some things called expedient are not useful but in reality are one branch of the harmful.' To due reflection, things sometimes regarded as 'practical' are in truth highly impolitic and shortsighted. But the way to eliminate preference for narrow and shortsighted expediences is not to condemn the practical as low and mercenary in comparison with spiritual ideals, but to cultivate all possible opportunities for the actual enjoyment of the reflective values and to engage in the activity, the practice, which extends their scope." -Dewey, LW 7, p. 209 (1932).

40 Such normative and conceptual positions are the source of persuasive rhetoric mitigating the competition of research traditions. This indicates to me why we need to better understand the character and logic of normative discourse.
"genuine" ethics, just as much as it supports the sense in which Christian 'science' isn't "genuine" science.

The disparity fallacy is nowhere instanced more clearly than in the line of argument that supports differences in axioms as the distinguishing factor. Reichenbach asks whether ethics can be conceived deductively, and answers negatively; for it is incorrect to say that the final ends of ethics are either self-evident truths or again axioms derivable from more general principles. For Reichenbach the division rests upon the difference between the cognitive axioms of science and the non-cognitive axioms of ethics.

In order to prove that virtue is knowledge, that ethical judgments are of a cognitive type, we would have to prove that the axioms of ethics are of a cognitive nature. The question of the nature of ethics is thus reduced to the question of the nature of ethical axioms.41

The view of science as an axiomatic system is implied by Carnap and Reichenbach. Ayer places greatest emphasis on his claim that forward-reaching consequences are derivable from scientific hypotheses but not from ethical ones.42 In Reichenbach's statement, it is the

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41 Reichenbach, 1951, p. 58.

42 I have responded to this claim above. Even when values are excluded from analysis by definition, their is a form of 'ought', the ought of rationality or expediency, that helps us find logic in "practical" language. The form of the hypothetical imperative calls for an "ought" judgment, though it is an empirical rather than a valuative ought in the instrumentalist scheme. The LE sought to take solace in this meager traffic across the is/ought divide. But normative questions about knowledge production are impoverished on this view. This seems something peculiar to Modern philosophy; Pre-modern and non-western traditions do not share this same radical conflict between man as a knower of fact and as a profferer of value. And how we came to honor it is revealed to some extent by the reductionistic fashion in which the natural and normative questions concerning practices of knowledge production have been posed. A. Kaplan claims to find 'derived cognitive meaning' through the instrumental aspect of an imperative. Ayer responds by saying this only puts off the difficulty: We must either come to final ends or involve ourselves in an endless regress; and the axioms of ethics cannot be proved. The judgment that one ethical system is correct or better than others is itself a value judgment: here we find the charge of vicious circularity that was so crucial for Ayer and which is apparent in Reichenbach's argument as well. It is the basic reason why Ayer, in dealing with metaphysics or with ethics as contrasted to science, argues "We cannot bring forward any arguments to show that our system is superior"; There is "No way of determining the validity of any ethical system." -Ayer, Language, Truth and Logic, p. 111-112.
'unconditioned' claims of ethics, represented in moral principles, which are the axioms indicated as non-cognitive; my opposition to this line of reasoning should be apparent since I have already argued against the radical division of two levels of imperatives, the hypothetical and the categorical, that Kant saddled modernism with.  

Herbert Feigl puts the case for the difference of axioms by claiming that "The validating principles of deductive and inductive logic do not seem at all to have any plausible alternatives or competitors" (1952 p. 676). Closure isn't had (or can't be guaranteed) on ethical disagreements because there are alternative systems of morals, but "given the purpose of language and knowledge, there are no genuine alternatives for fulfilling them." Here of course is the idea that there is comparatively far greater latitude in the 'way of life' of a community, than of ways of knowing the world. But the grounds for cognitive adjustments to the world are not deductive either. The real question instead seems to be whether the implicit suggestions presented here --that only in science is some logic used in every piece of research, and that logic is not used (or not routinely used) in non-scientific forms of inquiry-- are adequate grounds for defense of the LE's selective foundationalism.

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43 In this regard Philippa Foot (1972) is right in pointing to Kant's own "hedonism" concerning conditioned ends as a contributing factor to this impasse. According to Kant all actions not done out of duty were governed primarily by personal pleasure and self-interest. When this view is dismissed, there is little need to establish the uniqueness of the unconditioned imperatives or the autonomy of its logic.

44 Feigl 1950, p. 135. "The vindication of the principles of meaning and knowledge is so trivial precisely because, given the purpose of language and knowledge, there are no genuine alternatives for fulfilling them. But we do know of alternative systems of moral norms. An aristocratic ethics such as Nietzsche's and a democratic one such as Jefferson's are clearly incompatible with each other." See Waller (1986), "The Virtues of Contemporary Emotivism" for a contemporary defense of this view.

45 "The Ideal of Science may also get reinforcement from an element of the cognitive part of the 'frame': in their foreknowledge about science logical empiricists are impressed by the fact that some logic is used in every piece of research. They seemed to have turned this observation into one of their basic principles." -Radnitzky, Contemporary Schools of Metascience, p. 58.
If these grounds are not adequate, and Feigl's tacit identification of science and science alone with deductive and inductive logic is unfair to the ethicists, then the conclusion to be drawn is one quite opposite to Feigl. I conclude that what has happened here is that a preconceived deductivist orientation in the epistemologies of these men has driven them to misconstrue the role of empirical axioms in science, and even to incorrectly characterize science as an axiomatic system. The demand that we begin from final ends or from 'proven' axioms in order to validate our decisions or to allow cognitive meaning to subsidiary value judgements is entirely inapt, a reflection of what Bernstein calls the Cartesian Anxiety. The restriction of postulational methods, techniques of consequential and counter-factual reasoning, etc. to science was highly artificial and carried on only by misunderstanding the tasks of both traditional naturalist and intuitionist ethics. In science, it can be shown, consequences do not follow merely upon postulation of a hypothesis, but only subsequent to being combined with pertinent background knowledge ("initial conditions"). *Pace* Ayer, future oriented consequences are routinely derived in ethics, and one only has to move beyond the positivist's faulty conception of scientific confirmation in order to recognize the point. Furthermore, the *LE* themselves used postulational methods only selectively, and were falsificationists only about empirical hypotheses, not about their normative commitments. Falsifiability concerned primarily the formulation and confirmation of empirical hypotheses, but not the normative criteria by which hypotheses were evaluated.

Both utilitarians and emotivists are self-proclaimed 'naturalists,' yet they are divided by the more substantive division between cognitivism and non-cognitivism. Ayer for instance certainly does *view himself* as a naturalist about values. But what we are beginning to find is that, upon examination, Ayer and other logical empiricists are radically inconsistent about their naturalism. This is an inconsistency recognizable in the disparity in the way that facts
are thought relevant to theoretical conclusions inside and outside science. The form of
naturalism they advocate is one that rejects the notion that there are values with *intrinsic
worth*, but which unhesitatingly asserts the view that there are *facts with intrinsic meaning*.
Moreover, as we will see more fully in Chapter Three, the logical empiricist's actual
treatment of norms in science shows them to be dyed-in-the wool intuitionists about this sub-
class of values!

The considerations and arguments adduced in this section should not be taken as implying
that the demarcation problem can be merely set aside; nor that there may not be significant
differences, between various disciplines with respect to their fact-sensitivity, and the
objectivity of their methods of inquiry. The argument is rather than these differences fail to
justify the radical disparity in the way the logical empiricists held factual considerations to be
relevant to theoretical conclusions. Gewirth argues that there is a grave circularity between
the noncognitivist's linguistic categories and the characterization of disciplines analyzed on the
basis of linguistic categories. Here the circularity in choice of moral principles alleged by
Ayer and Reichenbach comes back around. "It is circular to say that the basic principles of
science are themselves cognitive," Gewirth responds, "for it is these principles or norms
which determine whether anything else is to be called cognitive" (p. 318).

We cannot pursue an extended discussion of each of the many proposed bases for the
contrast of the languages of science and ethics. But a few conclusions about the rhetorical
strategies involved in the persuasive contrast of science and ethics are warranted at this point.
If the case for the exclusive validity of restrictive definitions of science is made on the basis
of different axioms, we are entitled to ask what is so special about the axioms of science, and
why we should accept the positivist notion of science as an axiomatic system. If the case is made on the basis of the alleged ‘unity of science’, we are entitled to question the physicalist and reductionist theses which were basic to the project of unified science. If the case is made on the basis of a unique method of confirmation, we are entitled to inquire whether background assumptions are any less important in science than in other fields, and whether, this granted, confirmation practices in both fields don’t make good use of postulational methods and strategies of consequential reasoning. If the case is made based on the notion that we all know what the ends of science are, while those of ethics are variable, we are entitled to ask how disputes of such magnitude as that between scientific realists and non-realists can be so easily set aside. The contrast of languages can no longer rest on notions of decidability and tractability, if by these we must assume agreement on the norms or criteria of scientific belief and decision. The central terms of metatheory are normative,

46 The cognitive sphere is also involved in the sharp contrasts of basic content and method which has typically been alleged of the normative sphere by the noncognitivists. One could argue that there are different ways of making cognitive adjustments to the world, just as surely as there are different ways of ‘getting along’ with other moral agents.” If the nonrestrictive approach to ‘ethics’ be justified on the ground of the plurality of the ways of “getting along” with people, a nonrestrictive approach to ‘science’ may be similarly justified on the ground of the plurality of the ways in which men have actually made ‘cognitive’ adjustments to the world” (p. 326).

47 “Every sentence of psychology may be formulated in physical language...all sentences of psychology describe physical occurrences, namely, the physical behavior of humans and other animals. This is a sub-thesis of the general thesis of physicalism to the effect that physical language is a universal language, that is, a language into which every [material model] sentence may be translated.” -Carnap, 1932. Compare 1962, p. 896, 904 and 951.

48 If the current state of debate over the realism/anti-realism issue in philosophy of science is any indication, the problems involved in positing one unique cognitive end for science appear equally as daunting as they are for ethical monists. Of course, Carnap claimed that this dispute was metaphysical and cognitively non-sensical. A more qualified argument along these lines might be developed independently of Carnap’s selective skepticism towards metaphysics, so I do not fault Carnap over his stance on this classical antithesis.
implying that the skeptical challenge to the foundations of knowledge bears far more evenly across the range of human normative practices than the LE were willing to concede.49

2.3 Instrumentalism and relativism

Gilbert Harman’s provocative defense of scientific objectivism and moral relativism deserves special focus, since it represents a use by a major figure of the selective foundationalist metaphilosophy.50 Harman’s arguments for scientific objectivism and moral relativism are based on some of the same persuasive contrasts we have previously discussed. In examining these arguments more closely through his recent work, I will attempt to draw out the sources of Harman’s relativism and show their inheritance from received metaphilosophical views.

Harman’s argument in The Nature of Morality (1977) leans upon differences in the observability and verifiability of the predicates typical of scientific discourse. As his main example, he contrasts the judgment ‘There goes a proton’, with the judgment ‘Those children

49 “The reason why scientific questions can be decided solely by cognitive means is that, since we (and I here exclude the Christian Scientist, and so forth) agree on the norms or criteria of scientific method, the chief thing that remains is to determine the facts by the use of this method, and such determination is hence a purely cognitive process. But this is also true in respect of ethical and political issues. If men agree on ethical or political norms or criteria, then they can come to agreement...” -Gewirth (1960b), p. 321. On a coherentist view, there is no logical requirement for ultimately grounding our axioms for ethics any more than for science.

50 “Moral Relativism Defended” (1975) and Harman (1984). Harman usefully distinguishes four sense of "ought," the 'ought of expectation', the 'ought of rationality' the 'normative ought to be' and the 'normative-ethical ought to do.' Harman’s relativism concerns the last of these, and is limited to what he calls "inner judgments as contrasted with "outer" judgments. Basically, this contrasts "the judgment that someone ought or ought not to have acted in a certain way or the judgment that it was right or wrong of him to have done so" with the (outer) judgment "that someone is evil or the judgment that a given institution is unjust." To say of a contented employee of Murder, Inc. that he "ought not" kill Orckett, Harman argues, is a "misuse of language," since that would imply that our own moral considerations carry some weight with him. This Harman says is merely a thesis about logical form, but a deeper sort of relativism I think pervades his defense of "implicit agreement theories".

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are wicked.' But when telling why the former calls for a realist interpretation and the latter a non-realist one, he appeals straightaway to the explanatory value empirical fact has for the one type of judgment but not for the other.

The observation of an event can provide observational evidence for or against a scientific theory in the sense that the truth of that observation can be relevant to a reasonable explanation of why that observation was made. A moral observation does not seem, in the same sense, to be observational evidence for or against any moral theory, since the truth or falsity of the moral observation seems to be completely irrelevant to any reasonable explanation of why that observation was made. The fact that the observation of the event was made at the time it was made is evidence not only about the observer but about the physical facts. The fact that you made a particular moral observation when you did does not seem to be evidence about moral facts, only evidence about you and your moral sensibility.\footnote{The Nature of Morality, 1977, p. 7.}

Harman’s central argument for a radical disanalogy in the explanation of scientific and ethical judgments, is that in the explanation of moral judgments we do not need to make reference to moral facts, but only to nonmoral facts and the appraiser’s moral beliefs; whereas in the explanation of scientific judgments, we depend crucially on the explanatory value of empirical (natural) facts.

David Brink (1989), a cognitivist about values, argues that Harman fails to establish his disanalogy between the scientific and moral cases. Brink points out that Harman’s approach leans on the difference between appeal to brute fact and appeal to psychological set in the explanation of change-in-belief. Brink argues that Harman’s unguarded appeal to brute fact as a final, reliable guide that science has and ethics does not, indicates question-begging. For,

If we make no assumptions about the truth of Spike’s moral judgment and theoretical commitments, then it is true that we can provide a certain kind of explanation of Spike’s judgment by appeal to his "psychological set" alone. Spike judged that the children were doing something wrong, because he believed both that they were needlessly setting a cat

\footnote{The Nature of Morality, 1977, p. 7.}
on fire and that pointless cruelty is wrong. This explanation contains no commitment to moral facts, only commitment to Spike's beliefs or psychological set. But if we make no assumptions about the truth of the scientist's observational judgment and theoretical commitments, then it is equally true that we can provide a kind of explanation of her judgment by appeal to her psychological set alone. The scientist judged that a proton was passing through the cloud chamber, because she believed, among other things, both that there was a vapor trail in the cloud chamber and a scientific theory about the behavior and observational consequences of protons in supersaturated atmospheres. This explanation contains no commitment to protons (or cloud chambers), only commitments to the scientist's beliefs or psychological set.\textsuperscript{52}

Brink's point is that explanations in terms of psychological set alone are a perfectly general possibility, not restricted to events for which we might initially offer moral explanations. The real issue is over the adequacy of competing explanations, an issue which involves a great deal of cognitive evaluation. True, the inferential relationship between reasons and attitudes is not necessary in any formal sense. But it would be absurd to assert that the relationship between doxastic states and their (non-analytic) evidential ground ever has this kind of necessity. Psychological or physiological explanations of belief-formation seem a general possibility; but this has rarely tempted epistemologists or social scientists to treat them as adequate, or in effect to collapse epistemologically-couched (or ideographic) explanations into nomothetic ones.\textsuperscript{53}

\textsuperscript{52} D. Brink, 1989, p. 185.

\textsuperscript{53} The view Hempel gives in "Science and Human Values" (1960) appears to be considerably more adequate than that proposed by other LE we have thus far considered. The basic difference is that he draws a parallel between value assumptions and empirical postulates rather than attempting to differentiate them strongly. Some value judgments have a status similar to observation statements we use as one means of testing a theory. But Hempel already realizes that such observation statements aren't themselves finally proven but are just statements presently not questioned. Others are like axioms chosen; neither are final judgments to be conceived as closed questions. Instead of saying the questions are non-rational he says merely that "if the process of justifying a given decision or a moral judgments is ever to be completed, certain judgments of value have to be accepted without any further justification, just as a proof of a theorem in geometry requires that some propositions be accepted as postulates, without proof." (p. 87). Hempel emphasizes that "unconditioned judgments" of value are not absolute or irrevocable, but only judgments about ends considered not in need of further scrutiny at the present time. Still, Hempel understates the role of values in science and still employs a strong distinction between the explanation of
To accept Brink's parallel is not, of course, to say that what have been called psychological set explanations are adequate in science, or would be deemed so by professionals in the field. But it is to attempt treating question of the adequacy of these explanations uniformly in the two cases, rather than begging it outright. To the extent that Harman's case is made on a presumed lack of parallel between conditions of explanatory adequacy, it reflects the fallacy of disparity discussed above. *Harman at least should have attempted to deal with the fact that ethicists are as likely to reject the adequacy of the psychological set explanations of change in moral attitudes as scientists are of "change in belief".*

Brink insists that psychological set explanations are not adequate for the moral case, just as Harman insists they are not adequate for the scientific one. But Brink's argument here supports a return to the comprehensive foundationalist metaphilosophy. It makes the appeal to moral as well as scientific foundationalism (he also counts himself a "realist" on both scores,) the cure to the explanatory puzzle. Explaining respectively why either the scientist or Spike holds the beliefs they do ("unless we regard [their] training as in some way mistaken) will lead eventually to the existence of certain physical and moral facts. This is to accept the premises of Harman's argument, but to argue that we can make enough sense out of the notion of "moral facts" to justify a strong descriptivist account of moral language. By contrast, I would emphasize the naivete of Harman's appeal to fact as an independent determinant in scientific testing, and press further on Brink's point that testing in both ethics and science relies on holistic methods and consequential reasoning.  

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54 This point will become clearer when we discuss the relevancy of background knowledge to the derivation of empirical or rationally criticizable consequences of a hypothesis (discussed in relation to the "historical turn" in Part II).
themes of Will's earlier work, *Induction and Justification* (1974), was just this disparity in Modernist conceptions of the way in which facts are thought relevant to theoretical conclusions. On my view, as on Will's, the is/ought distinction is not grounds for skepticism; yet in order to maintain this we have to be clear that the establishment of norms does not resemble a deductive procedure. The demand that they be so established is entirely inapt.55

A second part of Harman's disanalogy is his argument that while nonmoral facts help explain moral judgments, putative moral facts are always "completely irrelevant" to explanation. Brink rebuts this assumption by arguing that the availability of psychological set explanations of moral judgments does not undermine the plausibility of moral explanations, much less show them to be irrelevant. He also points out that in epistemology we standardly recognize cases in which appeal to fact can explain events not explainable in terms of psychological set, as for instance when an agent is unaware of certain facts. Since this appears to be an 'in house' dispute between a selective and a comprehensive foundationalist, I am not concerned to further address this aspect of their dispute. But Brink's cogent response does convince me that some of the typical argumentative strategies by which selective

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55 "...Norms cannot be formally deduced from descriptions of historical events, nor is there any a priori proof that they ought to be followed, but there are no good reasons for making either of these demands necessary conditions for the acceptability of proposed norms. It is sufficient that we be able to recognize the kinds of behavior that have tended to help or hinder the development of science in order to make proposals as to how scientists ought to behave. Thus the question 'Is the philosophy of science normative or descriptive?' is not particularly illuminating if it presupposes that these are mutually exclusive alternatives." -H.I. Brown, "Normative Epistemology and Naturalized Epistemology," p. 165. Compare also Dewey, Vol. 16, p. 357: "Evaluative judgments cannot be arrived at so as to be warranted without going outside the 'value field' into matters physical, physiological, anthropological, historical, socio-psychological, and so on. Only by taking facts ascertained in these subjects into account can we determine the conditions and consequences of given valuings, and without such determination 'judgment' occurs only as pure myth."
foundationalists have divorced scientific from moral realism, are based on poorly and sometimes even fallaciously constructed disanalogies.\footnote{Harman’s position presupposes that moral factors (he uses the stronger term "facts") are not explanatorily relevant even if they are conceived as supervening on natural facts. Cognitivist views typically involve the notion of supervenience and do not treat moral facts as autonomous from nonmoral facts. On these views, the explanatory power of the natural facts ‘ensures the explanatory power of the injustice that they realize.’ Brink points out that appeal to moral facts is at least practically indispensable, "since it might often be extremely difficult to identify all those lower-order facts that actually constitute some moral fact that seems explanatorily relevant" (p. 192). Brink also attempts to go substantially beyond this in defending the legitimacy of moral explanations in cases where we can or could identify the nonmoral facts in which the moral explanans consists.}

Harman’s is one of the most challenging versions of moral relativism in recent years, since he unflinchingly accepts consequences sometimes considered to provide a reductio of the view. There are no cogent reasons for a person to accept principles that differ from those they already accept. One may speak of "reasons for changing one’s principles," but only in the sense of arguments that do in fact motivate a change in the person’s desires and behavior. Arguments that "would lead him" to such a change, are counted, but normative arguments that some evidence or consideration "should" or "ought" to motivate are discounted: moral argument is a practical use of language, and as earlier non-cognitivists put it, a matter of non-rational persuasion. On Harman’s view it seems, convictions about morality reduce to conventions, and reasons are defined only by their efficacy. "Hardened professional criminals," he claims "since they do not share our conventions, ... have no moral reasons to refrain from stealing or from killing us" (p. 112). Where argument fails to effect its intended psychological change in attitudes or beliefs, there is no sense of speaking of the argument as containing “good reasons” at all.

Harman’s view that desires and agreements require no justification but themselves provide the psychologically-given framework within which justification can proceed, is part of the same predominant myth that "reasons" follow upon the acceptance of a conceptual scheme
or framework, but do not and cannot help determine whether one framework is better than another. This myth of one-directionality, as we have seen, is already deeply imbedded in positivism, and supported by metaphilosophical views that go far back into the history of philosophy. What these metaphilosophies share in common is the view that in the end, Decision is king. Decisions as to "ways of life," or some other term for the norms of action, help shape contexts in which reasons are used, but reasons cannot help us make basic decisions. "At the foundation of well-founded belief," Wittgenstein held, "is belief that is not founded." At the base of decisions which are well-founded are those that are unfounded."

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57 "...One of the nagging and persistent questions has been: How are we to provide an ultimate justification for the rules, principles, practices, and norms that we actually employ? There are some who suggested that to ask this question is to exceed the bounds of linguistic propriety. Others recognized this as a legitimate issue but claim that the philosopher's task is done when he has described moral discourse. Still others, who reveal hidden affinity with the existentialists, have suggested that in the end, Decision is king. Even here there has been disagreement about whether such a decision is completely arbitrary (because there are no further standards by which we can evaluate these decisions) or whether such an ultimate decision is the most "rational" decision that can be made." R. Bernstein, *Action and Praxis*, p. 194. Bernstein’s statement comes in a chapter on Peirce and Dewey, and indicates tensions about which the pragmatists already had concern. It must not be supposed that the concern Bernstein raises for "ultimate justification" implies that there must be a foundational ground for normative commitments. This would be to commit an error teleological accounts are often prone to. This on a pragmatist account is the opposite yet complimentary error as that of the instrumentalists discussed earlier.

58 "Regularly, and indeed frequently in recent years, the discovery of the indispensability of ampliative process in the governance of norms in certain areas, the infeasibility in philosophical governing thought of replacing these altogether with deductive ones, has been taken to be a sign that at this point the limits of acceptable philosophical thought in the governance of norms have been reached, that the domains of life and thought affected have been revealed as to this extent inescapably determined by 'causes' rather than 'reasons,' permeated by influences that are not tractable in principle to acceptable philosophical procedure. And without question, a most important ground leading to this depreciating appraisal of these processes has been just their wide divergence in character from deductive ones, their relative intractability to the kinds of analysis that have achieved such great intellectual prestige through successful application to various widespread and fundamental forms of deductive activity." -Will, p. 43.

59 Wittgenstein, *On Certainty*: "At the foundation of well-founded belief is belief that is not founded (215)...Giving grounds...justifying the evidence, comes to an end; -but the end is not certain propositions striking us as true, i.e. it is not a kind of seeing on our part; it is our acting, which lies at the bottom of the language.
It is interesting in this respect as well that both Brink, the moral realist, and Harman, the moral relativist, claim to be "naturalists." No clearer contemporary example of the old antithesis between Kantian views of moral value as \textit{intrinsic value} and the view of all value as instrumental value (Ch. One) could be asked for. Indeed Harman even canonizes this antithesis about the status of values by contrasting "autonomous" ethics with his own "naturalist" ethics --conceived as the claim that all valuative judgments are \textit{hypothetical} imperatives, relative to "a specified set of purposes or interests or aims" (1984, p. 377).

Nathanson's pragmatically-oriented argument in \textit{The Ideal of Rationality} (1985) aims to show the failure of instrumentalist accounts of rationality which proceed from the \textit{de facto} authority of a psychology of desires, or of an "implicit agreement" on aims among the members of a group. What Nathanson characterizes as the "means/end account of rationality" are those forms of means/end instrumentalism which make the rationality of actions dependent on the likelihood of their satisfying a person's desires. It is an approach which as Nathanson shows has antecedents in Humean epistemology, and which goes back in American meta-ethics at least as far as to R.B. Perry's influential notion of value as "the object of any interest." Nathanson also takes Harman as one of his targets, and argues that the means/end account Harman holds can be shown incorrect by revealing 1) the mistaken

\textsuperscript{60} The treatment of moral statements as a species of psychological/empirical judgments is already imbedded in American meta-ethics in the interest theories of R.B. Perry and others, where "X has value" equals "X is the object of any interest". Stevenson, otherwise highly influenced by Perry, insists that this view misses the essential prescriptive component of value judgments.
relationship between reasons and motives which it relies upon, and 2) the relativism concerning ends which its acceptance has as a consequence.\textsuperscript{61}

The psychological view that grows out of the means/end account leads to viewing reasons as motivators: "To think that you ought to do something is to be motivated to do it."\textsuperscript{62} On this view something can be a reason for acting only if it provides some motivation for action; Something is judged "good" or worthy of being acted upon, only if in fact one wants that thing or action, or there is implicit agreement on this end among people sharing a language. According to Nathanson,

The means/end theory makes the rationality of actions dependent on the likelihood of their satisfying a person's desires. Because means/end theories hold that actions are made rational by their connection with an agent's aims or desires, it follows that there is no reason for a person to act on behalf of an end he does not desire. To have a desire, aim, or goal is to be in a certain kind of motivational state. We can, therefore, express this view by saying that something can be a reason for a person's acting only if it provides some motivation for the person to act.\textsuperscript{63}

\textsuperscript{61} The first error is seen in the way that it makes motivations underlie reasons and evaluations. This Nathanson shows makes all of the ways in which it might attempt to deal with the problem of "irrational desires" untenable. Nathanson drives his point home by arguing that means/end accounts fail to give satisfactory responses to the problem of irrational desires: "some desires fail to provide a person with any reason to act. Indeed, there are some desires that it would be irrational to act upon" (p 99). This is not something that can be conceded easily by an account that makes desires both the necessary and sufficient condition of value.

\textsuperscript{62} From Nathanson, p.94. Harman had previously said "According to Hare, to accept a general moral principle is to intend to do something. If we add to his theory that the relevant intentions can be reached through implicit bargaining, the resulting theory begins to look like the one that I am defending." -1975, p. 14.

\textsuperscript{63} Nathanson, p. 94. This side of Nathanson's normative view is established in his discussion of the possibility of "nonmotivating reasons." "In evaluative contexts it makes perfect sense to talk of nonmotivating reasons, reasons that ought to influence a person but fail to do so" (p. 96). Reasons need not motivate in order to be genuinely relevant reasons. The conclusion he reaches is that reasons and motivations, though intimately connected, are not self-justifying. The contexts of warranting are evaluative ones, and the normative force of reasons ought not be equated with their felt persuasive force.
The essential point Nathanson makes that bears upon my own argument is that Harman's case for moral relativism is made to rest on the adequacy of the means/end or instrumentalist theory of rationality. This is closely connected with his defense of an "implicit agreement thesis," the time-worn thesis "that moral judgments make reference to and are made in relation to an agreement in intentions" (1975, p. 16). For Harman, beginning with the descriptive content of \textit{de facto} desires and implicit agreements ensures the objectivity of the inquirer; in quite Weberian fashion, the inquirer is said to be freed from "making any assumptions about value" him or her self.

Here we see the operationalist side of Harman's thought, for he believes not merely that moral judgment is relative to (or makes no sense apart from) such group agreement or consensus, but also that the only scientific sense that can be made of \textit{de jure} claims is captured in the operation of referring to some \textit{de facto} agreement. This I see as a result of the inadequacy of the selective foundationalist metaphilosophy. Harman's account, I must agree with Nathanson, rests on the dubious view that reasons must be motivators, and makes nonsense of the notion of non-motivating reasons. This connection between the reductionism of the instrumentalist philosophy and the urge to a descriptive psychology of desires or social consensualist account, is something that will be repeatedly emphasized in this dissertation. Later we will see how Harman's appeal to \textit{implicit agreement theories} is paralleled in radical historicist philosophies of science.

Harman defines himself as a "naturalist," although this self-labeling and his glossing of naturalism is one of the most contentious aspects of his view. Naturalism is presented in this

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64 "I would like to show how a commitment to means/end rationality implies an extreme version of ethical relativism. If one believes that this sort of relativism is false and if it is implied by the means/end theory, one will have reason to reject the theory." -p. 93. My only question here is why Nathanson focuses solely on ethical and not also cognitive relativism as implied by the means/end account of rationality.
way: "What a naturalist wants is to be able to locate value, justice, right, wrong, and so forth in the world in the way that tables, colors, genes, temperatures, and so on can be located in the world." His conclusion is that someone who takes naturalism in this way and actually attempts to locate moral properties in the world in order to be able to treat them as natural facts "will tend to become skeptical or relativistic" (1984, p. 370). Given my criticisms of Moore’s moral realism in the light of his correspondence theory of meaning in the last chapter, I have no doubt about Harman’s conclusion; but the question whether Harman’s premises represent the best, or even a self-consistent, form of naturalism, must certainly be raised.

This form of naturalism is one that depends on a radical asymmetry in the treatment of judgment: one that rejects the idea that there are values with intrinsic worth, only to contrast values with an unreconstructed positivistic view affirming facts with intrinsic meanings. Given the inconsistencies alleged above in Harman’s attempt to treat judgment in this way, I must conclude that Harman is a naturalist in the same "truncated" sense in which Ayer was a naturalist—that is to say, whenever it served a selective foundationalist metaphilosophy. But the question can be put again, what ‘naturalistic’ basis is there for this?

2.4 Pragmatism and instrumentalism

The previous section ended with the suggestion that a certain prevalent version of instrumentalism—the "means/end theory" which restricts all "cognitive" and "reasonable" aspects of value to the relationship of means to ends—harbors an implicit relativism in its refusal to acknowledge rational and cognitive aspects of the valuative selection of ends. Here of course one finds motivation for the denial by scientifically-minded philosophers that the
issue of the *ends of science* is a valuative matter—a matter of choice or selection. All throughout the logical empiricist era, any discussion of norms, where it was considered polite conversation at all in intellectual circles, was typically conducted in an equivocal sense; what mattered above all was whether you had in mind the *norms of thought* or the *norms of action.* But my use of terms here is perhaps contentious. The way logical empiricists and some so-called naturalists framed the issue was in terms of two other contrasts: the contrast, on the one hand, between theoretical and practical interests, and on the other, between cognitive and non-cognitive meaning. The claim is that if this is correct, my shorthand is appropriate: the "cash value" of combining these two sharp distinctions in the way that they do is to establish a kind of radical dichotomy between norms of thought or knowledge, and norms of action or conduct.

In Chapter Three I argue in detail that the logical empiricists fail not only in the 'scientistic' thesis identifying knowledge and reasoned thought with science, but also in meeting the burden they placed upon themselves to provide incorrigible foundations for the norms of science. For the LE is a "committed" believer that a lack of foundations for knowledge claims leads straight away to relativism. That is to say, the LE explicitly uses the non-empirical grounding of the axioms of ethics, as well as infinite regress/vicious circle arguments about normative ethical theoretical conclusions as evidence that a relativistic view should be taken. This places the burden of themselves to show it is otherwise with the axioms and normative posits of the sciences (Axtell 1991, forthcoming). They only distinguish their cognitive objectivism from their moral and institutional relativism on the basis of their claim to make good on foundationalism with regard to the aims and methods of natural science—that is, the norms of thought. But the focus of the present section is on this

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65 See also my "Logicism, Pragmatism and Metascience," *Philosophy of Science, FSA: 1990* Vol. 1.

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means/end instrumentalist outlook which the LE accepted with regard to the norms associated with practices such as ethics and jurisprudence. The question here is, 'Is the pragmatic theory of valuation a version of this particular form of instrumentalism?'

In introducing this focus, it is perhaps appropriate to speak in somewhat broader terms about the development of my argument thus far. I have attempted throughout not to beg the intuitionism/naturalism dispute on either side in my critique of the logical empiricists. But my own pancritical account of norm governance, I suggested as the outset, might support Will’s non-reductive account having ‘features of both intuitionism and naturalism’ (p. 140). One occasionally finds indications of a non-reductive account of norm governance from sources in line with the tradition of American pragmatism. It is not uncommon on the European continent either. Consider Marinus Doeser’s statement:

Whether it is indeed so that values and value judgments are not amenable to rational evaluation depends on the definition of rationality one wants to use. When rationality is defined exclusively in terms of methods enabling the determination of means-end relationships values must, by definition, be excluded from rational evaluation. This is also the case when rationality is identified with methods that lead to agreement among well-thinking people or among a majority of them. But why should these definitions be accepted?  

This statement is arguably consistent with naturalism, yet contains a sort of Moorean flavor. There is cognitive content or information in the various modalities of ‘ought’ judgments, but it isn’t exactly the same content stored up in the ‘is’ judgments that support it. Hence Moore is right to defend common language from the excesses of naturalistic reductionism that insist on the synonymy of "good" and natural good-making characteristics.

66 M.C. Doeser, "Can the Dichotomy of Fact and Value Be Maintained?".

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Still, and this Moore was not fully able to grasp, rejection of the synonymy relation does not exclude their joint occurrence.

Now, we know that Dewey termed himself an instrumentalist in a certain sense. This was his view that the object of knowledge is always an instrument; that knowledge is something to be sought for not for its own sake, but for the sake of action. Propositions for Dewey have meaning as means, means of conducting our relations or transactions with things. This he called a "conjugate" relationship. But Dewey's instrumentalism did not limit the search for instruments to some specific or pre-determined end. Hence the context of Dewey's instrumentalism differs from the means/end instrumentalism of Harman, with its consequent relativism arising from the view that all value judgments are hypothetical imperatives. Nathanson's critical focus on the connection between this kind of instrumentalism and a relativism about ends, this is to say, is well motivated by an understanding of Dewey's ethics.

The issues I am raising here are very broad ones, and it is important for us to keep the focus narrow enough to make a brief examination feasible. The means/end instrumentalist, as I understand him, insists on reducing values to an instrumental type when things as means are cherished. One way of demonstrating the difference between LE and pragmatist accounts of means/end relations is to pose the question of whether the pragmatist, like the means/end instrumentalist, reduces the "cognitive" aspect of normative judgments to values of an instrumental type. Fingarette (1951) tried to show a lack of parallel between Dewey and the means/end instrumentalists by arguing in effect that the latter's reduction of the cognitive aspects of the 'ought of morality' to the instrumental ought (often called the 'ought of
rationality'), is not mirrored in Dewey's pragmatism. Normativeness can be "cognitive" but not "descriptive" in Dewey's theory of valuation.\(^67\)

Dewey contrasted instrumentalism in the specific sense I am addressing --instrumentalism about goal-oriented behavior-- with foundationalism about goal-oriented behavior. Dewey's theory of valuation was as adamantly opposed to absolute values as was the logical empiricists'. Yet it presents an alternative which is very different in character from those of the LE. In further developing Dewey's pragmatism as a genuine alternative to both instrumentalism and foundationalism about goal-oriented behavior, I will focus primarily on the manner in which Dewey attempted to re-think the issue of means/end relations. Three themes I will briefly discuss in Dewey's theory of valuation are 1) the conditionedness of all human goals, 2) the mutual reciprocity of means and ends, and 3) the reflective character of genuine ends-formation.

We have already seen the differences between G. E. Moore and the noncognitivists. Some of these criticisms are shared by the pragmatist, but others are not. Dewey's own differences from Moore are starkly apparent in the difference between "the fallacy of confusing means and ends," which Moore campaigned against, and a nearly contrary error Dewey warned of --that of falling victim to the specialist's conception of means and ends. The "specialist's conception" separates out intrinsic goods, or goals treatable as ends-in-themselves, and makes them the special domain of intuition, revelation, or some other privileged experience. This conception depends integrally on the absolutism of final ends.\(^68\)

\(^67\) I would similarly charge that Harman's account implicitly reduces the "normative-ethical ought to do" to the "ought of rationality." See also Fingarette's "How Normativeness Can be Cognitive but not Descriptive in Dewey's Theory of Valuation" (1951).

\(^68\) In a Deweyan spirit, Kaplan writes, "The absolutism of final ends involves a reversal of the order of the order of a universal and existential quantifier. The situation is that, for every value judgment, there exist ends in relation to which the judgment has (derived) cognitive meaning; the position of a transcendental or [foundational]
According to Dewey, there are never any actual normative goals that are simply intrinsic goods. But traditional intuitionists and utilitarian naturalists decried such a suggestion. For the Moorean intuitionist, it is a direct affront to the autonomy of ethics, depriving it of its central focus. For the utilitarian naturalist, Dewey’s suggestion appears to render the principle of ‘greatest good’ unintelligible, and thus to reduce to instrumentalism.

The direction of my effort to clarify these key differences between pragmatism and logical empiricism can only lead back to Dewey’s original understanding of inquiry as essentially normative, and science as practical activity. This is also exemplified in Dewey’s thesis of the conditionedness of all normative judgments. For this thesis works against both the Kantian dichotomy between conditioned and the unconditioned judgments, and against psychologistic conceptions of value as the object of any interest. For Dewey valuation is never complete in itself; taking desires or interests as complete in themselves leads to the pernicious psychology of desires that Dewey insisted be abandoned. "The view which connects valuation (and "values") with desires and interest is but a starting point" (1939, p. 16).

intuitionist ethics is rather that there exist ends which give (direct) cognitive meaning to all value judgments. Empiricism, in rejecting this latter claim, is not thereby rejecting the former; it does not exclude all cognitivism but only the absolutistic sort. If the emotivist adopts the absolutistic premise of the existence of final ends, he is driven by his empiricism to deny cognitive meaning to value judgments. But if ends are not final, cognitivism can be made consistent with empiricism." -A. Kaplan, (1963) p. 838.

"Just as Dewey had long ago rejected the notion that there are facts independent of judgment, so he had long ago rejected the idea of values that are not the products of acts of valuation. Just as there are no facts with intrinsic meaning, so there are no values with intrinsic worth. Indeed, the whole notion of intrinsic values vanishes with the recognition of the superstition of necessity." -Ralph Sleeper (1986).

"Where there is more than one means available, or the one means available has a cost which may make us hesitate to draw our inquiries to a conclusion, we are faced with the need for a kind of practical thinking Dewey called ‘valuation’ (or ‘evaluation’)....If science is, as Dewey insists, practical activity, and practical activity has the features Dewey attributes to it, science will inevitably from time to time face the prospect of having to evaluate and revalue the traits of the methods, theories and experimental techniques it uses." -J. E. Tiles, p. 162.
Dewey does not use the distinction between hypothetical and categorical imperatives, and avoids Kant's overstated contrast of reason and inclination. But he does contrast "ideal" and "material" values. In Dewey's contrast, the separation of "ideal" goods is contextually, not analytically, determined. The mere ability of value judgments to be phrased as hypothetical imperatives is little comfort to the means/end instrumentalist unless he can show that this constitutes the entire "cognitive" content of the judgment. But for Dewey, "The notion that value is 'instrumental' because 'instrument-means' are what are prized hardly attains the dignity of a pun."

On pragmatist views, it is typical of norms to be embodied in action plurally rather than singly, and for the form of their embodiment to be affected by their complex relations with each other (Will, 1988 p. 33). There is often more than one means available to a chosen

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71 Dewey 1932, p. 221.

72 "The distinction is one between goods which, when they present themselves to imagination, are approved by reflection after wide examination of their relations, and the goods which are such only because their wider connections are no looked into. We cannot draw up a catalogue and say that such and such goods are intrinsically and always ideal, and such and such other ones inherently base because material...The business of reflection in determining the true good cannot be done once for all, as, for instance, making out a table of values arranged in a hierarchical order of higher and lower. It needs to be done, and done over and over and over again, in terms of the conditions of concrete situations as they arise. In short, the need for reflection and insight is perpetually recurring."

73 "If observation, not conceptual (actually verbal) abstractions be resorted to in this matter of 'instrumental' and 'final' values, it will be noted that things used as means (whether material or procedural means) are in fact prized, cherished, made subjectmatters of loving care and devotion, in every art or pursuit that has attained any desirable development. The notion that value is 'instrumental' because 'instrument-means' are what are prized hardly attains the dignity of a pun. Is there a special type of value, deserving a name of its own, in the case, say, of dogs or jewels being prized? If values are of an instrumental type when things as means are cherished, why not a type of dog-value or diamond-value in these other instances?" -LW 16 p. 349.

74 "In conformity with this they are plastic in various degrees, variable in their disposition to alter in response to different configurations of practices with which they come to be embodied. This is a most important clue to understanding the processes that, rather than primarily probative and, in consequence, applicative, are testive or critical with respect to norms, and, in their effects upon them, generative (molding and defining)." -Will (1988) p. 33.
end. This indicates for the pragmatist that value is attached not only to ends, but also to alternative means and their varying consequences. What happens when we must choose between the swiftest, most potentially powerful, most reliable, and most economical means? In Dewey’s conception, the relationship between reasons and the valuative judgments they support is not merely contingent or psychological in Stevenson’s sense. In order for a valuative judgment to be correct or appropriate, there logically must be empirical reasons in its support. But the reasons themselves cannot be expected to deductively entail valuative judgments such as those concerning choice of means.  

Formalistic explications of conditions for individual rationality fall short of providing a logic for such decisions. Even the point of carrying on the attempt to make ampliative processes of philosophical reflection fit a preconceived model of rationality becomes moot when we no longer believe that rationality is preserved only by demonstrating such a fit. In actual social contexts of decision-making, the valuative assessment of the consequences of various alternative means (rather than simple instrumental efficacy) is of primary concern. This of course does not mean that we shouldn’t pursue naturalizing these questions or bringing empirical reason to bear on them, but that instrumentalized interpretations of normative issues represents only an abstractive technique of reasoning that does not preserve all the meaning of the normative issues themselves.

The conditioned nature of ends, on the pragmatist view, --their embeddedness in social and communal practices-- does not of itself imply a lack of rational criteria by which ends-choices can be criticized and revised. Indeed Dewey distinguished between customary

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25 Logical empiricists will point out that Dewey appears to accept their tenet that supporting reasons have only to do with belief: “For in my opinion sentences about what should be done, chosen, etc., are sentences, propositions, judgments, in the logical sense of those words only as matter-of-fact grounds are presented in support of what is advised, urged, recommended to be done -that is, worthy of being done on the basis of the factual evidence available.” -ESML, p. 296.
(habitual) morality and reflective (principled) morality in order indicate how some of Hume's 'habits' are the result of learning, and a matter of conscious choice. To speak of the "conditioning" of ends by historical happenstance seems preferable in this respect, because the concept of conditioning is distinct from that of determination. According to Dewey, conditioned ends need not have the character, so often placed upon them by both these adversaries, of being ego-centered, merely arbitrary, or "purely conventional."

This leads me to the mutual reciprocity of means and ends as a second theme in Dewey's theory of valuation. An end is never genuinely separate from the whole constellation of human goals and interests out of which it emerges. Ends and means on earth are so entangled, that typically demands or invites changing the other as well. Dewey begins from this folk-wisdom in arguing that what may presently appear as one's end-in-view, once it is realized, becomes the means to other goods. The judgment that one intended action is a means, and another an ends, depends integrally upon perspective, and whether one is taking a short-term or long-term perspective. Short term goals are often frameable as long-term means. Goals are also often designated on an interim basis, until their realization allows other or more long-term goals to come into.

This recognition that such determinations are perspectival, however, need not be a prescription for allowing their conflation or non-discrimination in any particular context of inquiry or action. It means only that means and ends are abstractions from cultural contexts in which they are qualitatively continuous and functionally interactive. The discrimination of means from ends depends upon the inquirer's current endeavor and perspective. It depends, more specifically, on the kind of explanatory interests the inquirer brings to bear on his/her research.
A final theme in Dewey’s theory of valuation that deserves special attention is his emphasis on the generative or constitutive character of the formation of human ends. There is artificiality to any analysis which treats valuation in science as merely applicative and not also constitutive. The valuative reasoning involved in such constitutive judgments is contrasted to grading and measuring. "An end in-view thus differs on the one side from a mere anticipation or prediction of an outcome, and on the other side from the propulsive force of mere habit and appetite" (1932, p. 186). Dewey’s thesis of the ‘thoroughly reciprocal character of means and ends’ goes further in insisting on the necessary interrelatedness of ‘genuine instrumentality’ and questions of consummatory value. The consequences of alternative means and the rationale for ends-selection are intricately intertwined. Axiological decisions or assumptions constrain and provide standards of adequacy for means. Both the epistemic and practical consequences of means-selection lead back to reflection on and refinement of ends-in-view.

On the pragmatic understanding of all inquiry as essentially normative, the reflection upon what one should do, that is on the preferability of the ends, remains of central importance (1932, p. 185). For Dewey, reflection and active criticism of aims is a liberating force for human intelligence. Preferability of ends is no longer seen as testable by the mark of intrinsic value; for Dewey a much wider range of naturalistic considerations and evaluative

76 “To see what Dewey means by the reciprocal character of ends and means, it is necessary to add ... a caution about the artificiality of the distinction between instrumental and constitutive means. Means are never wholly external to the end which they are used to realize." -J.E. Tiles, p. 158.

77 In this sense pragmatism unlike logical empiricism has always emphasized ampliative aspects of philosophical reflection. Questions of cognitive and consummatory value are necessarily to be selected through ampliative phases of reasoning. An appreciation of the ampliative aspects of philosophical reflection is necessary for understanding what Will calls norm generation and "governance."
desiderata envelop this question. Similarly, when ends are taken as psychologically or otherwise given goals, inquiry is closed. But pragmatism has always stressed the openness of inquiry.

Although a fuller study of the pragmatist philosophy need not be undertaken here, it is important to note that Dewey’s theory of valuation is aligned with Pierce’s account in important respects. Both held that inquiry is essentially normative. This indicates that reduction of the valuative dimensions of inquiry to operational or instrumental concepts is a failed project; pace Ayer, axiology of science is as much a central concern as axiology in other areas of inquiry. It is active reflection on these issues of scientific axiology and on the prioritized applications of scientific knowledge, that enable responsible norm governance in our knowledge-producing practices. In an important sense, both the relativist and the absolutist influences upon epistemic concerns have the shared consequence of cutting off the openness of inquiry. In contrast, the pragmatic theories of Peirce and Dewey resist all forms of reductionism, including those of both the instrumentalist and the foundationalist about goal-oriented behavior. In order to understand the unique character of a non-reductive theory, I think we cannot do better than to put together and always hold together Peirce’s famous dictum, "Don’t block the road to inquiry!" with his philosophical claim that inquiry is essentially normative.

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78 On the distinction between ends and standards, see 1932, p. 245-6. On consequentialism and intentionalism in ethics, see p. 173.

79 "We cannot do better than apply what Dewey called the ‘method of intelligences’, that is, to examine as broad a range of positions and supporting considerations as possible. The process leads, not to structures with foundations, but, using Peirce’s metaphor, to supporting ropes composed of numerous independent and evolving threads." -D. Christie (1989), p. 116.
3.1 The Carnapian metaphilosophy

In this chapter, the faults of the logical empiricist account of metascientific discourse are examined through a study of the modifications Rudolph Carnap made to his version of the program over three decades. We have already seen that the logical empiricist tradition was strongly prescriptive or normative with regard to the language of science. The language of science on the physicalist's model was considered unitary, and "purely theoretical." In Carnap's technical vocabulary, "theoretical" uses of language indicated relation to analytic and empirical functions; secondarily and by implication, they indicated insulation from metaphysical, including valuative, judgments.¹

The epistemic meta-standards appealed to were understood to rest squarely on logic as an ahistorical foundation for epistemology. In this hierarchical model of criteria, scientific meta-standards were originally thought to provide the analytic framework necessary for the

¹ Indeed conferring cognitive status to meta-ethics and denying it to normative ethics followed upon strict limits to what could be maintained as "purely theoretical."
resolution of disagreements at lower levels. 2 Demarcation criteria for distinguishing "genuine" science from "pseudo-science," or at least non-science, was one of the most important such provision. We have examined some aspects of how the demarcation of science was seen as derived from explication of the meaning of terms in the language of science. As a matter of the form of that 'unitary' language, demarcation criteria were conceived to be derived independently of the content of scientific beliefs, and to remain undisturbed by the vicissitudes of historical changes in belief.

Carnap's view of philosophy as "the logical syntax of the language of science" 3 is a reflection of the logicist outlook on meta-level discourse found implicit in his work. In Carnap's early metaphilosophy, the ability of a linguistic expression to be translatable into what was called the "formal" mode of speech, was made the touchstone of all philosophically respectable claims. In this way metaphysics was to be eliminated, and pseudo-problems in philosophy recognized and avoided. 4 Carnap's somewhat more mature distinction between internal and external questions (Carnap, 1950 p. 28) has the effect of translating or replacing allegedly ontological questions of the existence of theoretical entities, with the question of whether the use of a certain linguistic scheme is expedient or fruitful for certain purposes.

These considerations indicate a functional mutation that the concept of the "analytic" undergoes in the work of the logical empiricists. Always of secondary importance in the work of previous empiricists, the logicist's conception of the analytical (as integral to the

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2 The hierarchical model of meta-methodology is defended explicitly by B. Norton in his defense of Carnapian metaphilosophy (Linguistic Frameworks and Ontology, 1977). See also his "On the metatheoretical Nature of Carnap's Philosophy", Philosophy of Science, XLIV (March, 1977). This model has been criticized and contrasted with the benefits of a "retriculated" account by Larry Laudan in Science and Values (1984).

3 "Philosophy is the logic of science i.e. logical analysis of the concepts...Philosophy is the logical syntax of the language of science." -Carnap, LSL, intro.

4 See for instance Carnap (1932) "The Elimination of Metaphysics Through Logical Analysis of Language."
logical analysis of language) marks a project which on longer-standing conceptions of the analytic/synthetic distinction would have seemed quite radical. The rationalistic strains of logical empiricism are clearly evidenced in this treatment. One interesting aspect of the logical empiricist’s revision of the analytic/synthetic distinction was the work that they expected the concepts of analysis and analyticity to play in the characterization of metascientific discourse. J. Proust (1990) puts the logicist project in historical perspective by noting that “analyticity is ascribed a task that in Kantian thought should have remained outside its scope, namely, the systematic organization of knowledge, the constitution of experience -- the general ability to account for the application of forms to content.”

The "construction of a meta-language for science" was one way in which Carnap often characterized his lifetime’s work. Carnap like other members of the Vienna Circle was cognizant of a range of metascientific problems which were legitimate areas of inquiry for philosophers of science. The existence of ‘strange statements’ is conceded by some of the Vienna Circle associates, yet there was unanimity on a solid division of labor between scientist and philosopher of science. Such a division was considered little more problematic than the distinction between the empirical and the analytic. The division of cognitive labor Carnap envisioned was a reflection of the sharp distinction between the empirical and the analytic. Carnap’s logicist or analytic model, it is important to see, was consistently employed to govern all "cognitive" tasks of language. Virtually by definition, any "cognitive" issue of metascience is formulable as a proposal about the form of that language.

5 J. Proust, Questions of Form: Logic and the Analytic Proposition from Kant to Carnap, p. xv. Proust sees the logical empiricists as initiating the third of three broad historical treatments of analyticity, and develops arguments why this treatment ends in failure.

6 The existence of meaningful statements that were neither analytic nor synthetic yet important to science was disputed within empiricist schools. Strict empiricists typically disallow ‘strange sentences.’
Indeed as late as the 1950s, logical empiricists still expected to treat logical analysis of the
language of science as adequate to resolve all cognitive disagreements. Metascience seen in
this way must contain only a very restricted range of issues; otherwise the notion of formal
criteria of resolution becomes stretched beyond credulity.

I have followed Burian (1987) and McMullin (1982) in referring to this general
formalistic or analytic model of metascience as "logicism." Burian (1987) also calls it
"autonomism." This is to utilize the term to characterize the logical empiricist account of
metascience, rather than merely their view of the foundations of meta-mathematics, as the
term generally referred to as used by Frege, Russell, Carnap and others. Part of the
logicist's hierarchical model was the Wissenschaftsideal, or Ideal of Science. This ideal led
Carnap to see the basic duality of formal-analytic and empirical-synthetic as mirrored in the
meta-language, or as Radnitzky put it, "in the duality of meta-mathematics as the self-
reflection of mathematics, and meta-science as the self-reflection of science" (1970, p. 58).
Logicism in McMullin's sense is the idea of closure within a system, and the scientific system
was considered the prototype of a closed system with cognitive aims founded in reason and

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7 Burian characterized the received view in terms of two basic theses:
(i) There are universally valid methodological and epistemological standards by means of which both science in
general and the special sciences may be philosophically evaluated.
(ii) The sole inputs for evaluating a theory (explanation, law claim, etc.) as of a given time are (a) knowledge of
the formal (logical, syntactic, semantic) structures of theories (explanations, law claims, etc.) and (b) properly
posed statements of the total relevant evidence available at that time, together with a properly posed statement of
the theory (explanation, law claim, etc.) -Burian 1977, p. 5, from Kegley 1989, p. 238.

8 Carnap said that his syntactical account of the language of science "fulfills simultaneously the demands of
both formalism and logicism." These twin "demands" are given prominence in the forward to the 1934 Logische
Syntax der Sprache.

9 "The ideal of science with respect to empirical science will be 'universal physics,' preferably as applied
mathematics, i.e. idealized physics. This reveals a utopian trait, which makes us expect that the metascience LE
wants to produce will be largely prescriptive." -G. Radnitzky (1970) Contemporary Schools of Metascience.
the intellect. Questions within science are governed by what Feigl called a "confirmability criterion"; closure was thought to be demonstrated in principle for questions which were conducive to a confirmability or decidability criteria.

Carnap's explicit commitment to "construction theory" has much to do with the logicist conception of metascience and metascientific discourse. In Carnap's introduction to his first major work, *Der logische Aufbau der Welt* (1928), Carnap presents construction theory as a logically acceptable replacement for metaphysics. The *Aufbau* is Carnap's attempt "to formulate the logical requirements which must be fulfilled by a constructional system of concepts," and to demonstrate this potential "by actually producing such a system." Carnap is thinking specifically to replace the part of metaphysics that has infected traditional epistemology (Richardson, 1990), through "an attempt to apply the theory of relations to the task of analyzing reality. Comments Alan Richardson, "construction theory provides more than just the 'neutral foundation' of epistemology, it provides the neutral foundation within which any question of the legitimacy of any concept can be couched" (p. 11).

*It is in the account of the goals and aims of construction theory that we are confronted most apparently with the foundationalist character of their view of metascience. It is here, in part, that the justification of normative metascience receives a treatment quite different from that of normative ethics. Meta-ethics was not continuous with normative ethics, nor jurisprudence with legal ethics, but metascience was treated as continuous with a methodology and heuristic.*

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96
account of the language of science. The correlativity of the meta and normative levels in the two languages thus languishes, and the only justification for this is the one they gave: the difference in the foundations of science and other forms of inquiry.

There was in Carnap's thought a close connection between construction theory and the "thesis of meta-logic," the claim that all genuine philosophical statements are meta-linguistic and are concerned solely with the logical analysis of language (Oberdan 1990). Linguistic analysis under this thesis of meta-logic is presented by Carnap, Feigl, Reichenbach and others, as a tight application of descriptive and analytic methods to what were seen as corresponding empirical and logical questions (or frameworks for statements) within the meta-language of science.

Since the "cognitive" content of science was said to be exhaustively covered by the analytic and descriptive tasks of metascience, there was not, at least in Carnap's early "syntactical" account of the language of science, any sense to philosophical problems not translatable into the formal expression of a syntactical problem. To ask how the genuine philosophical problem was to be resolved was merely to ask whether it was a question posed in an empirical or a logical framework, so that the proper form of analysis could be provided. The questions answered by these techniques were taken as the only "genuine" philosophical

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11 The Thesis of meta-logic "asserts that all propositions of philosophy which are not nonsense are syntactical propositions, and therefore deal with linguistic forms." (1932, p. 435) Oberdan writes, "The immediate consequences of this application was that the problem was no longer to be addressed as it was in, say, the Aufbau, as a question of the basic elements from which all knowledge is constructed. (Aufbau, sec. 54) Rather, the question should be regarded as dealing with 'protocol sentences,' statements expressing the results of scientific observations. ... The key to Carnap's argument is the idea that, if two sentences are inferentially related, they share a common content, no matter how different their vocabularies appear. This notion in turn rests on the distinction between the 'material' and the 'formal' modes of speech."

12 The syntactical account implies that the meanings of expressions in terms of their syntactic features, is wholly sufficient for philosophical purposes. (Carnap 1932; Oberdan 1991). Oberdan says, "Any remaining facets of language use belong to the empirical realms of psychology, sociology, or some related discipline." -p. 12.
problems infringing upon science. But were there other problems which went unrecognized on this account? This question we can pursue in the next section by examining some of the modifications Carnap made to his version of the logicist program over several decades.

3.2 After syntax: the logicist legacy

It was syntactical methods that Carnap at this early stage identified as those that would put logic to work in a constructive way on the language of science. But Carnap's syntactic stage was to be short-lived. Historically the reasons for this include his interactions with Schlick and Tarski around 1935, under whose influence he came to believe that semantic methods and issues could be incorporated into metascience without involving the philosopher in metaphysics. But there were philosophical reasons for his revision of the program as well, though Carnap was loath to recognize them.13

By a middle stage in his career, Carnap conceded that semantics is necessary so long as semantics is extensional, allowing the reduction of all meaning to sensa. The shift is from the syntactic account to the development to a two-level account incorporating semantics, (characterized as "the theory of meaning and truth") and to a tripartite division between syntax, semantics and pragmatics. Looking back in his autobiography, Carnap discusses the reasons for his shift; he recounts,

A few years after the publication of the book (Logical Syntax, 1934), I recognized that one of its main theses was formulated too narrowly. I had said that the problems of philosophy or of the philosophy of science are merely syntactical problems: I should have

13 As Ayer would later write, Carnap smuggled semantics into syntax in any number of ways, including the notion of "experience expressions" (Ayer, Logical Positivism, p. 26). On these problems, see Introduction to Semantics, sec. 39, and Thomas Oberdan (1991) "The Concept of Truth in Carnap's Logical Syntax of Language." Oberdan argues that Carnap's definition of analyticity in Logical Syntax undermines his syntacticism.
said in a more general way that these problems are \textit{meta-theoretical problems}. The narrower formulation is historically explained by the fact that the syntactical aspect of language had been the first to be investigated by exact means by Frege, Hilbert, the Polish logicians and in my book. Later, we saw that the \textit{meta-theory must include semantics and pragmatics}; therefore the realm of philosophy must likewise be conceived as comprising these fields.\textsuperscript{14}

The last sentence here reflects upon the issues which I want to focus on as most important metaphilosophically. The widening of the account of the language of science beyond syntax to include semantics was not thought to impair the characterization of the theoretical. Semantics like syntax could be divided without remainder into "pure" and "descriptive" sides or tasks. As late as the Schilpp volume on his life work, Carnap reiterates, "...in syntax and semantics I deliberately leave aside all prescriptive factors."

"...Logic, and likewise syntactical and semantic analyses of language are purely theoretical."\textsuperscript{15} Hence Carnap liberalized the role of the "metatheoretical" aspects of science while retaining his original restrictions on the characterization of the theoretical itself.

Now very little of Carnap's syntax-era critique of philosophical pseudo-theses survived the shift to semantics. For that earlier analysis had rested on the differences between quasi-syntactical, real-object, and syntactical sentences (Oberdan, 1990). Carnap's stepwise liberalization of his conception of the language of science is not insignificant for understanding the current dissensus about meta-level discourse. It has been widely recognized that the \textit{thesis of meta-logic} survived Carnap's syntactical stage in the sense that he never rescinded it, and continued to treat it as important in his later works. But should it have survived this transition intact? I see this as a matter of grave inconsistency for Carnap.

\textsuperscript{14} "Intellectual Autobiography," in Schilpp, ed. p. 56. Compare Norton, 13, 14, 29 and 40. The three-way distinction is usually attributed to C. Morris.

\textsuperscript{15} Schilpp, (ed.), 1963, p. 923.
While the preconditions which this thesis lays down for objectivity in metascience demand the
type of discourse characteristic of cognitive reason, Carnap's later work in fact tends to base
"internal" scientific discourse more and more directly on "external" questions. The discourse
relevant to these questions is still cashed out as non-theoretical, and ultimately non-cognitive
and arbitrary with respect to belief and reason. Insulation from the "non-cognitive" in the
form of metaphysically-dependent background beliefs and valuations could no longer be
guaranteed by saying that the meta-language of science involved no "extra"-theoretic issues
except those resolvable analytically.

It is important that we clarify at the outset two distinct yet inter-related senses in which
Carnap and other logical empiricists used the technical term "pragmatic." I will annotate
these senses as pragmatic and pragmatic in our further discussions in order to keep these
senses as distinct as Carnap's metaphilosophy allows. What I term considers
are Carnap's uses of the term that indicate non-syntactic and non-semantic aspects of
linguistic analysis. Only in pragmatics is there reference to a user or speaker of a language
(Intro. to Semantics, p. 9). Information and issues which do so refer are of a pragmatic character. The analytic and descriptive tasks of syntax (and later, of semantics also) were
considered independent of such considerations. This then is the usage which corresponds to
the one part of the tripartite division of meta-linguistics into "syntax," "semantics," and
"pragmatics." This usage is clear enough that I will not bother to annotate the plural
"pragmatics," since Carnap saw this term as refering to a linguistic field compared with
"syntax" and "semantics."

The "pragmatics" of language was to be seen as a realm only peripherally relevant to
science. Carnap described syntax and semantics as "theoretical" fields, but apparently denied
this of pragmatics because it is something of a place-holder, standing for whatever is
necessary to the language of science besides syntax and semantics. Syntax and semantics each divide without remainder into their pure and descriptive tasks, but there is no analogous division in pragmatics.

The second sense of "pragmatic" [hereafter pragmatic], and the one that remains our central focus, is the sense associated with the division between theoretical and practical decisions and their distinct desiderata. External questions are non-empirical and non-analytic. This is "...a practical decision concerning the structure of our language." Decisions are truly pragmatic or practical in Carnap's explicitly stated sense, only if science has no theoretical or justificatory dependence on them (1951).

Of course, there is no easy connection between the introduction of "pragmatics," in the tripartite division of metascience into syntax, semantics, and pragmatics, and the distinct sense in which "pragmatic" and "practical" were used to categorize the "conventional" reasons for choice of a language form (my pragmatic). I am not suggesting that pragmatics in the first sense is meant to be wholly or primarily the study of what is pragmatic in the latter sense. But the dual usage and the notion of "pragmatic utility" that Carnap in his later years used to explicate reasons for the choice of linguistic forms, is intriguing. It indicates to me that the reasons the LE moved to liberalize their conception of 'logical analysis of the language of science,' are intimately inter-connected with the problems afflicting their defense of scientific objectivity by insulating scientific activity from "non-cognitive" questions and influences. Thus, as logical empiricists acquiesced on the sharp distinction between theory and
observation\textsuperscript{16}, Carnap attempted to retain and reinstate an equally suspect polarity between
the theoretic and the pragmatic\textsuperscript{2}.

In looking into the relationship between Carnap's dual usage of the term "pragmatic,"
two points should be emphasized. First, the end of the syntactic stage came about in
considerable part because Carnap realized that the syntactic account formulated problems
affecting the philosophy of science \textit{too narrowly}.\textsuperscript{17} Second, the \textit{LE} conception of both
syntax and semantics fit well with the objectivist ideal of genuine scientific knowledge as
‘knowledge without a knowing subject’ (Popper’s phrase); the expansion of the field of
pragmatics is an expected effect of the failure of philosophers of science to explain their own
activity in terms of this ideal.

Yet the concession that there are relevant pragmatic\textsuperscript{2} decisions and questions, even if
only at the periphery of science, left Carnap with new problems. The challenge to the

\textsuperscript{16} While Reichenbach distanced himself somewhat from verificationism, Carnap’s position on the
theory\textbackslash observation distinction is not as strong as some of the positivists. These are ways in which the philosophy
of logical empiricism was sometimes distinguished from "positivism." Carnap’s position is closer to Neurath’s as
represented in the famous boat analogy. In “Testability and Meaning”, Carnap wrote, "There is no sharp line
between observable and non-observable predicates... [but for] the sake of simplicity we will here draw a sharp
distinction.... By thus drawing an arbitrary line between observable and non-observable predicates in a field of
continuous degrees of observability...the general philosophical, i.e., methodological question about the nature of
meaning and testability will, as we shall see, not be distorted." -p. 455. But the reason why he holds this
‘arbitrary line’ won’t be distortive appears to be the assumption Rorty calls "methodological nominalism", the
view that all cognitive sentences about \textit{intensional entities} can be reformulated into either questions about
observable entities, or else questions of syntax. Carnap himself says that the view that "every sentence of
psychology may be formulated in physical language ... [is] a sub-thesis of the general thesis of physicalism."
Restated in (1962), p. 905, and on 951 in relation to ‘the thesis of extensionality’. See Rorty's classic introduction
to Rorty, ed. (1967) \textit{The Linguistic Turn}. See also H. I. Brown on Carnap’s views on theory and observation:
"Carnap’s proposal constitutes a major weakening of this program." -\textit{Perception, Theory, and Commitment}, p. 46.

\textsuperscript{17} Hence Brian Norton (on whom more below) conceives that Carnap’s expansion of his account beyond
syntax demanded that the "meta-theoretical nature of his basic principles demands that their status be broadened as
well." -p. 37 Hence it seems to me that instead of intending to strengthen his program by broadening the
tolerance principle to a semantic issue, this is actually forced upon him as a consequence of his inability to account
for such meta-theoretical principles purely syntactically.
adequacy of logical analysis is not alleviated, but the burden is merely shifted from the notions of syntactic and semantic issues onto a newer umbrella notion of “meta-theoretical problems.” But as I will argue in last two sections, logical empiricism at mid-century displays even more clearly the basic faults in its understanding of meta-theoretical or meta-level discourse.

3.3 A dogma of contexts: using and choosing a standard

Carnap’s classic 1951 article “Empiricism, Semantics and Ontology” (ESO), and Feigl’s “Validation and Vindication,” published in 1952, indicate deep revisions in the empiricist program of both men. Feigl’s distinction between validation and vindication as two contexts of justification, and Carnap’s distinction between the internal and external as two types of questions attending scientific language, are both similar in important respects. Both distinctions work to maintain a logicist conception of theoretical science while acknowledging a broadened scope of extra-theoretic decisions and questions peripheral to science.

In Feigl’s context of validation, a hypothetical imperative is presented by the implicit or explicit appeal to standards or norms standing as justificans in relation to a justificandum. The validation context retains an instrumentalist rigor within the situation of some goal-oriented and standard-using decisions.\(^\text{18}\) This is a context covered by the logicist’s

\(^{18}\) McMullin tests the viability of such criteria by arguing that it leans on a version of the fact/value dichotomy that was at this point being strongly reconsidered. McMullin, “Values in Science,” p. 6. “Value-judgement in the sense of evaluation could thus fall on the side of the factual, and the old dichotomy between fact and value could still be maintained. Value-judgement (so the argument went) is necessarily subjective; it involves a decision which is not rule-guided, and therefore has an element of the arbitrary. It intrudes individual human norms into what should ideally (if it were to be properly scientific) be an impersonal mapping of propositions onto the world.”
"confirmability criterion." In Feigl's central characterization of the second context, "vindication" is presented as a matter of proposals that an author, given his wishes and interests, favors. The account leans upon the non-cognitivist understanding of valuative prescriptions and stipulations themselves. In a particularly candid statement of this view, Feigl writes,

...there are limits beyond which rational (i.e. logical and/or factual) argument cannot be extended. Intelligent reflection concerning means and ends, conditions and consequences operates within the frame of basic evaluations. Beyond those limits there could be only conversion by persuasion, propaganda, suggestions, promises, threats, re-education, psycho-therapy, etc.

This passage indicates that the direction in which empiricists moved as the arguments against verificationism and the theory/observation distinction began to mount, was towards greater dependence upon the differences between Using and Choosing a standard. If the foundations for knowledge previously postulated in observational primitives could not be relied upon, the line could be redrawn holding the observational and theoretic languages together in contrast to non-cognitive discourse.

But this effort relied at least as heavily on the analytic/synthetic distinction as did the logicists's earlier views of metascience (Proust 1989). Feigl's distinction between "ordinary"

19 Feigl (1952), p. 677-9. "(Vindication)...must then consist in showing that its adoption will produce the sort of clarity that we seek....In other words, if we do not wish to open the floodgates to countless questions which by their very construction are in principle unanswerable, then the adoption of the confirmability criterion is indispensable."

20Feigl (1952), p. 669.

21 There seems to me a parallel between this kind of shifting in epistemology, and what we previously saw were alternative ways meta-ethicists dealt with the problem of logical gaps between beliefs, attitudes, and dispositions to action. In closing the gap between theory and observation, Carnap if effect widens the gap between the theoretical and the pragmatic.

104
and "basic" value judgments reflects Urmson's noted distinction between grading and valuing (or evaluation and valuation). As I have tried to point out however, this move ignores criticism coming on the other flank of the logicist conception of "theoretic" language. As I will argue in the last section, such a shift encourages misunderstanding both of the cognitive aspects of choosing standards, as well as of the social aspects of using them.22

Like the earlier formal/material mode of speech distinction, the contrast between internal and external questions was intended to capture what Alberto Coffa (1976) has called 'second-level facts of the matter.' This, however, is merely to parallel the status assigned to the material\formal mode of speech, now abandoned or deeply revised.23 Apparently, the assessment of linguistic categories is more a function of the pre-conditions set by the Thesis of meta-logic, than of logical analysis proceeding analytically. In describing the difference between internal and external questions in ESO, Carnap wrote,

An alleged statement of the reality of the system of entities is a pseudo-statement without cognitive content. To be sure, we have to face at this point an important question; but it is a practical, not a theoretical question; it is the question of whether or not to accept the new linguistic forms. The acceptance cannot be judged as being either true or false because it is not an assertion. It can only be judged as being more or less expedient, fruitful, conducive to the aim for which the language is intended. Judgments of this kind supply the motivation for the decision of accepting or rejecting the kind of entities.24

22 McMullin (1982), p. 6: "What I want to argue here is that value-judgement, in just the sense that Weber deplored, does play a central role in science. Both evaluation and valuing are involved. The attempt to construe all forms of scientific reasoning as forms of deductive or inductive inference fails. The sense of my claim that science is value-laden is that there are certain characteristic epistemic values which are integral to the entire process of assessment in science."

23 "If a sentence of the material mode of speech is given, or, more generally, a sentence which is not a genuine object-sentence, then the translation into the formal mode of speech need not always be taken, but it must always be possible." -LSL, p. 277.

24 ESO (1951), p. 214

105
Carnap’s statement is explicitly addressing only one extra-semantic, or metatheoretical problem: the relation of semantics to ontology. The overt nominalist, Carnap thought, created an unnecessary barrier to theoretical science by barring the use of non-observational terms and implying that their instantiation involved the user in ‘Platonic metaphysics.’ We can have sympathy for the development of semantical methods for clarifying, interpreting and analyzing languages of communication. But while Carnap had much to say in defense of semantics in relation to ontology, he did not appear to see the force of residual problems arising with respect to the relation of his semanticist project to the valuative or pragmatic aspects of metascience. ‘Well-formed’ questions may in common-language ask why we have the purposes we do, what virtues of theories should be valued, and how they should be weighted. Even if scientific beliefs were as separable from metaphysics as logicism expects them to be, the inseparability of metascience from valuative reasoning would have been anathema to a confirmed non-cognitivist about value judgments such as Carnap. To the end Carnap maintained his logicist framework, admitting nothing into the language of science that would amount to an incursion of the non-cognitive on the epistemology of the sciences.

The issue to Carnap then is not merely whether the sharp distinction between internal and external questions allows the philosopher of science to deal with theoretical posits semantically; it is the adequacy of that sharp distinction itself to the understanding of meta-level discourse en toto.\textsuperscript{25} Logicism starts to become undone when its own implicit relativism becomes apparent. As McMullin has argued,

\textsuperscript{25} Meta-criteria derivation can be "external" only at the cost of making the issues of theory evaluation and selection, and scientific axiology, foreign to science itself. As McMullin puts it, "Once Carnap allowed it to be posed, however "externally", it would not be long until theory-evaluation would be clearly recognized as the most "internal" of all scientific issues, defining as it does scientific rationality and scientific progress. After we have discarded his term 'external', we still retain his insight that the structure of decision in regard to the acceptability of a theoretical language is not one of logical rule but of value-judgment" (p. 14).
...Carnap conceded much more than he may have realized in this manoeuvre. By equating the general semantical problem of abstract entities with the problem of theoretical entities in science, he implied that the pragmatic "external" criteria are the appropriate ones for deciding on the acceptability of the linguistic frameworks of science, that is, of scientific theories. (McMullin, p. 13)

Seen in this way, Carnap's effort in *ESO*, however persuasive it was for many empiricists, tends to undermine the last support for the analytic/synthetic distinction and other central tenets of logical empiricism. It was on the basis of the analytic/synthetic distinction that Carnap defined the realms of the cognitive and the theoretic. It was on this basis too that he justified excluding prescriptive elements from syntax and semantics. Internal questions were seen as decisively resolvable by attending to the pure and descriptive chores of syntax and semantics. Carnap says that answers to internal questions may be answered "either by purely logical methods or by empirical methods, depending upon whether the framework is a logical or a factual one" (*ESO* p. 30). "Internal questions are here, in general, empirical questions to be answered by empirical investigations." (*ESO* p. 36.) Given this technical definition of the theoretic realm, the pragmatic must be considered a place-holder for all that is witnessed or required in normative metascientific practices, yet which fails to meet the marks of the theoretic. This dichotomous conception of meta-theoretical discourse results,

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26 "The questions dealt with in any theoretical field—and similarly the corresponding sentences and assertions—can be roughly divided into object-questions and logical questions....The logical questions...do not refer directly to the objects, but to sentences, terms, and so on, which themselves refer to the objects." — *Philosophy and Logical Syntax*, p. 277.

27 McMullin comments, p. 13-14: "The term, 'external', was obviously an unhappy choice, as things would turn out. The questions Carnap dubs 'external' would be external to science only if theory-decision is external to science. They were external to his logicist conception of how science ought to be carried on, of course. Only if science can be regarded as a ‘given’ formal system can the enterprise for the logician get under way."
as I argue in the final section, in a deep misunderstanding of ampliative discourse and of the rationality involved in cognitive evaluation.

3.4 Pragmatic decisions and rationality

In Linguistic Frameworks and Ontology, B. Norton (1984) describes himself as critical of logical empiricism, yet defends the metaphilosophy of ESO because he thinks the internal/external distinction and Carnap’s principle of tolerance could be defended independently of verificationism and other central empiricist tenets. He argues that Carnap’s account allows for a strong notion of "external rationality." Carnap, Norton argues, implicitly understood well-formed external questions to be decidable according to the criteria of "pragmatic utility."

Because the advocates of the standard interpretation fail to see that Carnap distinguishes between properly and improperly formulated external questions, they take his comments which relegate improperly formulated external questions to the realm of non-verifiable, undecidable pseudo-questions to apply to all external questions. In fact, external questions are decidable, but according to very different criteria than the criteria according to which internal questions are decided.29

28 Otherwise known by Carnap as "the principle of the conventionality of language forms." The thesis of metalogic is not mentioned in Logical Syntax and thereafter, as it has been absorbed into the Principle of Tolerance. Coffa writes, "...The linguistic world of knowledge was, for Carnap, an Upstairs-Downstairs world. Upstairs, in the object language, live the masters, the scientists, those whose task it is to find out the facts about the world. Downstairs, in the metalanguage, live the servants, the philosophers, whose task it is to keep the house of knowledge clean ...By and large Upstairs people can build their houses any way they want...The principle of tolerance refers to the Upstairs building and says that most anything goes in that domain; but what Carnap used to call "the thesis of meta-logic" imposes law and order Downstairs; and it does so by demanding the utmost sobriety of subject matter: metalanguages, and thus philosophers, are to talk only about their object languages: proper Downstairs talk is always Upstairs gossip.... What inspired Carnap's Downstairs demands was his distrust of the philosopher's ability to say anything interesting qua philosopher about things other than language." -Coffa (1976), p. 207.

29 Norton, 1977, p. 36.
I believe that Norton reads too much into Carnap's statements about external questions, and the possible development in Carnap's account of the notion of external rationality. He confuses the ability to develop a non-empiricist metaphilosophy with Carnap's actual position in ESO. Indeed one of Norton's central themes, that "the basic principles underlying 'Empiricism, Semantics and Ontology' are essentially independent of verificationism in particular and empiricism in general" (Norton, p. 5), appears quite absurd if taken as an explication of Carnap's own position. Carnap did not view them this way, and certainly did not make the break from empiricism which Norton suggests; while Carnap hoped to de-emphasize the question of verificationism in 1950, it continues to underlie criteria for distinguishing the internal from the external. In fact, Norton's attempt to defend Carnap's later metaphilosophy as laid out in 1950 involves him in far more of Carnap's empiricism than he is prepared to admit.

A neat division of work between the philosopher and the working scientist is constructed by Norton, and related to Carnap's indication of two ways of asking "reality" questions. "Philosophy, on this view, concerns the structure or form imposed upon the data in the presentation of a fact. Science, on the other hand, concerns the raw data, and presupposes philosophical issues in the presentation of facts" (Norton, p. 30.) Norton sides with Carnap against Quine\textsuperscript{30} (1951) and others who wanted to retain the semantical methods yet let go of

\textsuperscript{30} Quine was critical of Carnap's account of the relation of semantics to ontology; he was critical also of the dependence he saw in Carnap's distinction between internal and external questions on the synthetic/analytic distinction. See "On Carnap's Views on Ontology", Philosophical Studies, vol. 2 (1951), and Word And Object, p. 19-25, 271. Ways of Paradox, p. 134. From a Logical Point of View, p. 79. But Quine himself remained a logicist in meta-methodology, in the important respect that he sees no place for normative commitments in the beliefs that he proposed to test holistically. This criticism was well stated by Morton White in "Normative Ethics, Normative Epistemology, and Quine's Holism." Note that White (1950) had already attacked the "unteachable distinction" between the analytic and the synthetic prior to the publication of Quine's famous "Two Dogmas of Empiricism." "One of my main purposes in this paper is to persuade Quine to abandon a dualism between the methods of testing normative and descriptive statements which is as untenable as that between analytic and
the sharp distinction between logical sentences and factual truth. Like the empiricist, Norton assumes that knowledge is strictly to be identified with the cognition or representation of fact, and that philosophy "concerns not the datum contained in the fact, but rather the structure or form in which the fact presents itself." This form/content distinction, and the association of philosophy with formal (read analytic) inquiry, was certainly a central contention of the empiricists.

To Norton's credit, he indicates that "To say that external issues depend upon questions of pragmatic utility should not be the end, but the beginning of an account" (p. 146-7). But Carnap, as Norton recognizes, "failed to provide any useful analysis of the concept of pragmatic utility." This shows Norton's claim of the adequacy of Carnap's metaphilosophy to rest upon the potential Carnap's systematic philosophy has for accommodating the development of this concept. On my view, however, this confidence is unwarranted; to develop this notion of pragmatic utility would really mean leaving behind the central distinctions upon which the metaphilosophy of ESO depends. This of course includes the clear separability of pragmatic from theoretic questions and the associated insulation of theoretic reason from contact with the non-cognitive.

Norton's motivation appears to be the failure of efforts in the later 1950s and 1960s to retain logicist conceptions of rule-governed activity together with their introduction of conventionalism about meta-methodological issues. The methodological conventionalism of Popper and others only accentuates the implicit relativism of the tradition on issues involving

synthetic statements... "Quine's response to White's criticism by contrast is that of an empiricist disdainful of normative theory but trying to claim logical foundation for whatever normative claims his theory may commit him to. "When in a passage quoted by White I referred to 'the ultimate duty of language, science, and philosophy' I was using the word somewhat as when we speak of a heavy-duty cable of a tractor. It was what language, science, and philosophy are for, as eyes are for seeing." "For me normative epistemology is a branch of engineering..." -"Response to White" in Schilpp (ed.) 1986.

110
non-rule governed choice (Laudan 1989a). For both Norton and the Popperians, methodological rules hinge upon answers to conventional questions; what Norton wants to show is that one can deny any kind of underdetermination by instrumental reason to such questions.

Norton's conception of external rationality is developed through the few passages that Carnap wrote on the matter of external questions involved in the determination of scientific meta-standards. These passages indicate Carnap's attempt to accommodate rule-making "internally," by recourse to instrumentalism, or means-rationality. Carnap's response to Kaplan in the Schilpp volume is just such an effort to head off questions of value-judgment in metascience by reducing them to questions of means-rationality. As elsewhere in Carnap's thought, accommodating these issues as internal means showing that they divide without remainder into chores performed by pure description and logical analysis.

This emphasis on the rationality of external questions is related directly to what Norton like the LE calls the "decidability" of external questions. But this indicates another of his own empiricist leanings: disputes not capable of definite closure are those that are subjective and non-rational. Indeed logicism through its idea of closure within a system seems to require such an assumption. Norton wants to push Carnap to declare no part of science, including the metascientific, as closed to the decisionist solution. But there is much tension here between Norton's intention and Carnap's own. For part of Carnap's intent appears to have been precisely to save rule-determination in the "theoretic" sphere by giving it up in the rapidly expanding "pragmatic" sphere; the lack of constrained consensus or rule-bound decisions, was one of the central characteristics Carnap used to distinguish the external from the internal. If this is correct, then conventionalism plays a more expanded role for Carnap after mid-century than prior to this point. Carnap insists on a rule-decisionist outlook only on
questions decided on the basis of "truth or falsity," not on those of he characterizes as "a matter of degree" or of "more or less."

To summarize thus far, Carnap's emphasis on the internal/external distinction comes about precisely in order to save rule-determination within science by showing that all valuative or non-rule-determined decisions lie outside of science proper. Carnap is clearly of two minds when he speaks of "pragmatic utility" as governed by a kind of instrumental logic. His inconsistency is quite apparent in his vacillation between areas in which theoretical reason determines decision (the internal issues), and where he is adamant to concede it only influences decision but cannot determine it (the external issues; normative ethics also).

Carnap remains a non-cognitivist about value judgment throughout his career. As late as the Schilpp volume, he defends a version of this theory. Specifically, he distinguishes T1 (empirical) and T2 (analytic) statements, as the two kinds of statement exhausting "cognitive" discourse. The non-cognitivist thesis is identified as the thesis that any statement or utterance neither T1 nor T2 is necessarily T3, non-cognitive. Terms such as "better," are still said to be used in one of two ways, either instrumentally or prescriptively ("Autobiography," Schilpp, p. 1010). But if meant instrumentally, they are empirical, falling under T1(b): "Statements on means-end relationships, e.g., 'the action a is a means to achieve the aim b'," or T1(c): "Statements on the utility of a possible event (e.g., receiving a certain amount of money or of certain goods) for a person."

The analytic side of the meta-language of science was to include values clarification and explication. T2(c): "Statements giving an explication of relevant concepts connected with values or valuations, or consequences of such explications." But the characterization of expediency, fruitfulness, etc. as "theoretical" considerations, assumes that Carnap could accommodate them as either statements of type T1 or type T2. For my purposes, this
provides a useful criterion for determining whether cognitive evaluation can be included within Carnap’s technical definition of the cognitive. One of the principal ways in which Carnap distinguishes internal and external questions is in terms of need for "theoretical justification"; internal questions have it, and external questions, including valuative ones, don’t. The further move he wants to make now is to re-introduce the relevancy of instrumental reason. This shifts Carnap’s later account to a somewhat more empirical stance in contrast to the emphasis on analytic reason in meta-level discourse emphasized earlier.

The decision of accepting the thing language, although itself not of a cognitive nature, will nevertheless usually be influenced by theoretical knowledge, just like any other deliberate decision concerning the acceptance of linguistic or other rules. The purposes for which the language is intended to be used, for instance, the purpose of communicating factual knowledge, will determine which factors are relevant for the decision.

The move Carnap makes here reflects the general movement towards sophistication in non-cognitivist accounts of value (reviewed in Chapter One), from early emotivism, which largely separated normative ethics from empirical reasoning, to more qualified forms of non-cognitivism such as Stevenson’s, which re-introduced the relevance of considerations of the efficacy of means in situations of practical decision. But while Carnap typically contrasts judgments of expediency and fruitfulness as pragmatic judgments with theoretic judgments, he at times says just the opposite. At one point in ESO for instance, he says that "This acceptance (of a linguistic framework) is not in need of a theoretical justification (except with

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31 Reichenbach (1951) at just this same period says imperatives have instrumental meaning in contrast to cognitive meaning. Since he limits the kind of cognitive meaning value judgments have to "implicational meaning" and "token reflexive meaning," this implies that instrumental meaning must be adequately categorized under implicational meaning in order for instrumental judgments to be "cognitive," or "theoretical."

32 ESO, p. 31.
respect to expediency and fruitfulness), because it does not imply a belief or assertion" (ESO, p. 146-147). Even more strongly at another point he writes, "The efficiency, fruitfulness and simplicity of the use of the thing language may be among the decisive factors. And the questions concerning these qualities are indeed of a theoretical nature" (ESO p. 31).

I may appear here to be extrapolating too far by concluding from what Carnap says about desiderata of linguistic framework selection, that he prevaricates on issues of cognitive evaluation. But for those who agree with Popper in his criticism of the "Myth of the Framework" in positivist tradition (Chapter 4), these issues are not as far separated as Carnap’s discussion makes them appear. The inconsistency over whether such judgments of simplicity, fruitfulness, etc. are or are not "theoretical" —are determined or merely influenced by theoretical reason— is indicative on my view of Carnap’s perplexity with regard to cognitive evaluation. The choice of terms such as "relevance" or "influence" gives away Carnap’s intention to contrast issues where logic and evidence is determinative, and where it is not. This in turn is part and parcel of the persuasive definition of science and the contrast of disagreements in belief with those in attitude.

A closely analogous point was established in Jane English’s 1977 article, "Partial Interpretation and Meaning," in which English critically examined Carnap’s attempt to hold together a contextualism about theoretical language, and an anti-contextualism about observational language. This relates directly to the problems Carnap encounters due to his contrast of theoretical and observational languages, and his account of theoretical language as "partially interpretable." The failure of Carnap to make good on this attempt, English argues,

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33 "Only within a framework can meaningful question be asked and ‘meaningful’ (‘cognitive’, ‘theoretical’) ‘beliefs’ or ‘assertions’ be expressed." Cited from Wedberg, p. 162. Compare Logical Syntax, p. 20. Popper’s article in criticism of this view is discussed in Chapter Four below.
marks the historical juncture at which the problems about commensuration at the level of the normative standards of theory-appraisal (i.e. the metascientific level), first came to be regarded as serious by empiricist-minded philosophers. Objections to Carnap’s revisions of the program are numerous in the 1950s. These problems are most often expressed today in the way that Kuhn made popular a decade after Carnap’s influential article _ESO_, in speaking of "paradigm incommensurability." Kuhn’s challenge was made the more substantial through his expansion of the "unit of selection" from the theory to the ‘paradigm’ (discussed further in Ch. Four). But it seems still to be a little understood point that the grounds for Kuhn’s challenge to the received view, and for this expansion of the unit of selection issue, is prepared at the mid-century stage by the failure of Carnap that English points out.

But if Carnap as early as 1950 is confronting a version of the incommensurability problem in his characterization of external questions, he does not see how deeply the problem affects his account of metascientific discourse. To the extent that scientific observations and meta-standards are theory-dependent, they appear to be indirectly value-impregnated (H. I. Brown, 1987). The term "external question" turned out to be an unhappy choice, for it made normative criteria for theory-preference appear external to science; What we can see in retrospect is that valuations ungrounded in logic play an essential role in meta-level discourse. They are external not to science, but only to the logicist conception of science (McMullin p. 31). The characterization of answers to external questions as "pragmatic decisions" was equally unfortunate, since it perpetuates the segregation of belief and attitude. Verificationism is still entailed, even if only implicitly. Even the assumed division between talk about physical structures and talk about linguistic expressions is shaped by the verification principle. Carnap’s position in _ESO_ depends essentially on the difference between questions answerable "yes" or "no" (or true/false), and answerable only as "a matter of degree," and
verificationism is still the manner in which this difference is alleged to be determined. Along with the distinction between the cognitive and the non-cognitive, these are still the terms in which Carnap identified the differences between theoretic and pragmatic discourse. Yet the qualifications of the logicist program over three decades had already severely compromised these contrasts.

Even a minimal concession to the dependence of value judgments on natural facts and vice versa raises serious doubts about the usefulness of Carnap's sharp distinction between matters answerable yes/no or true/false, and matters of "more or less" or of "degree." Here is provided no sound criteria for demarcating what is a 'belief,' and what an attitude or matter of 'decision.' One can, I believe, be highly sympathetic to the logical empiricists' attempt to distinguish metascientific standards from moral values, and scientific judgment from moral judgment (Hempel 1960, 1983). Certainly also there is some validity to a distinction between more and less fact-sensitive desiderata of theory-choice. But this is not tantamount, as has been assumed by many contemporary philosophers influenced by the Carnapian metaphilosophy, such as Bas van Fraassen, to a sharp distinction between "epistemic" and "pragmatic" values.

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34 Hempel draws a parallel between value-assumptions and empirical postulates, rather than contrasting them. On both sides, we begin by choosing axioms which aren't proven, but are just statements not presently questioned. But Hempel still distinguishes sharply between conditioned and unconditioned judgments. The former relies only on "relative ultimates", not on "an absolute, or categorical, judgment of value" (1960, p. 96.) "The grounds on which scientific hypotheses are accepted or rejected are provided "by empirical evidence, and may include observational findings as well as previously established laws and theories, but surely no value judgments [categorical]."

35 See The Scientific Image (1980), and P. Churchland's pragmatist response in Churchland and C. Hooker, eds. Images of Science (1985). "Criticism will be directed primarily at van Fraassen's selective skepticism in favor of observable ontologies and against his view that the 'superempirical' theoretical virtues (simplicity, coherence, explanatory power) are merely pragmatic virtues, irrelevant to the estimate of a theory's truth [or 'empirical adequacy']."

Far more carefully than van Fraassen's division, Rescher has divided intellectual from non-intellectual values in the following way: The intellectual ones are those that are part of what he calls the "parameters of
Now as we saw in Chapter Two, there are crucial differences between a willingness to let naturalistic reasoning emerge in these cases and the reductionism of rigorously instrumentalist accounts of rationality. The mere form of the hypothetical imperative does not make methodological decisions purely empirical, nor does the content of axiological judgments render decisions of this kind non-cognitive. The deductivist assumptions permeating logicist metascience advise us to conceive deductive and ampliative processes as discrete and exclusive, much like the faculty psychology of Hume. What is needed as a remedy to this logicist legacy is a constant reminder that methodological and axiological aspects of science arise out of concrete problems in which these aspects are originally inter-fused.

Aside from the constraining notion of efficiency of means, we shall often need to weigh, for instance, predictive accuracy, against internal coherence, external consistency, unifying power, fertility, or simplicity. Such judgments of the relative weight to be accorded different virtues of theory do not appear to be formally renderable. It is clear at this point why objectivist philosophers of science including the logical empiricists have been so reluctant to acknowledge aspects of ampliative reasoning in metascience. Yet the problem stems more from the objectivist’s own persuasive contrast of science and values, than from genuinely relativistic consequences of the value-laden character of science. There may yet be a strong element of discovery or learning of epistemic significance to go with its elements of

\[\text{systematicity}\] (simplicity, regularity, uniformity, comprehensiveness, cohesiveness, unity, harmony, economy, etc.). These values have three marked features he argues: “(1) The issue is one of the cognitive norms or values that implement the requirements of intelligibility and understandability.” “(2) The values at issue are object rather than subject oriented. They relate to the objects or materials of our theorizing inquiry and not to the workers that carry it on. In this respect they differ altogether from such values as perseverance, honesty, probity, cooperativeness, etc., that represent desirable traits of scientists rather than of the science they produce.” “(3) The values at issue are tendential. [Preference of this type] is not absolute or peremptory.” -Rescher Cognitive Systematization, p. 16.

\[^{36}^{36}\text{ Will, 1988, pgs. 39-40 and 135-9.}\]
construction. The inviting antithesis is again between absolutism and relativism about goals and values. This polarity is one which the logicist's overstatement of metascientific objectivity and of normative-ethical subjectivity has only served to exacerbate.

To conclude, I should like to comment on Carnap's own understanding of the revisions in his metaphilosophy in 1950 and thereafter as a "pragmatization" of the logical empiricist program (Carnap from his "Autobiography", p. 1013). It is true that the revisions of logical empiricist programs over the course of 30-40 years, much of it triggered by in-house criticisms, led the founders of the movement to greater affinity with the pragmatist school encountered in America. But Carnap's characterization still involves a significant element of self-deception. The position of the later Carnap, in contrast to the earlier, has been to respond to criticism by recourse to a new distinction between internal and external questions. This strategy depends logically upon his ability to place even more justificatory burden upon the dichotomy between theoretic and pragmatic reason.

But pragmatists never viewed the pragmatic as non-cognitive nor, for that matter, the cognitive as non-normative! They never placed pragmatic reason outside of the normal justificatory procedures of cognitive inquiry. Pragmatism has always advocated opening epistemology onto a plane of psychological, social and historical conditioning that cut across these neat divides. Hence the deep differences between logical empiricists and pragmatists over the construal of metaphysics and valuative judgment are indeed sharpened rather than appeased by Carnap's efforts in ESO.

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In my estimation, the logicist school in metascience has encouraged misunderstanding of justification and discovery, reason and cause, etc., by assimilating these divisions with the difference between deductive and non-deductive reason. This is a significant part of the way in which the problem of ampliative inference is obscured by the deductivist character of LE epistemology. The bifurcated conception of reason this account entails is the basis for my claim that logicist tradition was given to that curious set of assumptions I earlier characterized as methodological nomotheticism.

Yet there is perhaps a deeper affinity than is generally acknowledged between these assumptions of the logicist school, and those of the historicist school which came to prominence after its demise. We must turn in Chapters Four and Five to examine the two main branches of the historicist school, focussing on how methodological nomotheticism came to exert influence over the discussions of objectivism and relativism initiated by those who identify with a "historical turn" in philosophy.
CHAPTER FOUR

THE BRANCHES OF THE HISTORICIST SCHOOL

Introduction

The criticism of logical empiricist conceptions of norms and norm governance in the first three chapters will now to be followed by a criticism of the two main branches within the "historicist school," broadly conceived. I will not be further concerned with reasons for the historical turn itself in the late 1950s and early 1960s, or with the historicist critique of logical empiricism, since I have already offered my own criticisms of logicist metatheory. The primary concern will be to examine the main schools of historicism from the perspective of pancritical pragmatism as introduced in the previous chapters.

The historical turn initiated far-reaching changes in views about the epistemological importance of background commitments to confirmation practices in the physical as well as the social sciences. All historicists agree at least that the logicist picture of metascience (1) underestimated the importance of conceptual factors in decision-making; (2) contradicted the variety and changing character of methods that empirical study reveals; and (3) conceived confirmation practices and the unit-of-selection issue in science in an overly narrow fashion.¹

¹ Rachel Laudan lists these as defining characteristics of the "historical school," along with 4) a view of science as value-laden, 5) a de-emphasis of the demarcation issue, and 6) a rejection of the cumulative view of change. These can be seen as shared assumptions of historicists who may nonetheless exhibit very different views of norm governance in science. My only problem with the list is that Lakatos, who is clearly a historicist, still attaches enormous importance to the demarcation issue, as I show below.
Despite these commonalities in criticism of the received view, the historicist "movement" was never homogenous, and became divided quite early in its infancy over fundamental issues associated with the objectivist/relativist debate.

For the purposes of this study, it will be instructive to divide the historicist "movement" into two broad post-logicist schools or traditions in meta-methodology. A distinction between thinkers I term "objective historicists" and those I term "contingent historicists" will be used to underline this split, particularly as it concerns issues of normative discourse and its explanation. Although of course too simple to capture all differences of substance, this distinction will be useful in identifying and clarifying a number of shared assumptions of positivist origin, which I claim permeate the responses of both these schools to issues involving understanding/explanation of the beliefs and decisions of historical agents.

I make the division between objective and contingent historicism using figures in epistemology identified respectively with objectivistic and relativistic views about knowledge. In Chapters Four and Five, my focus is directed to philosophy of science, although a broader focus might show that there have been analogous influences of historicist thought over ethics, jurisprudence, and the humanities. I. Lakatos, L. Laudan and J. R. Brown have all identified themselves as historicist-oriented defenders of the objectivity of scientific meta-methodology, while the "strong programme" (D. Bloor and B. Barnes) and

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2This division must be made without compromising recognition of the complexity of central issues in this debate. The issue of unity of methods in the social and physical sciences is perhaps the most apparent area of significant differences; but such issues of unity are commonly complicated by individual thinker's response to such issues as the "underdetermination" of theory choice, "holism" in unit-testing in science, or "incommensurability" of meta-methodologies ("linguistic frameworks", "paradigms", "conceptual schemes", "culture", "historical period," "research programmes," "research traditions," etc.). In this chapter it will be enough to again use a more generic sense of "meta-methodology", leaving the separability of the methods of the social sciences as an issue for the last part of the dissertation.
radical constructionist (Pickering) positions in the sociology of science are taken to exemplify thinkers who have rejected the objective historicist position through arguments emerging from contingent historicist perspectives. We may summarize what the branches of historicism share in common as the thesis that methodological standards of theory appraisal cannot be treated as a matter of logic and evidence alone, and that choices among traditions, paradigms, etc. depend upon "conceptual" issues and "background" commitments, variously interpreted.

This leaves us to conceive the difference between the two branches in term of their alternative interpretations of the epistemological implications of the ampliative and valuative aspects of philosophical reflection. The thesis of objective historicism I will take to be the following: theory selection in at least some part of science is "objective," and the justification of a programme of theory evaluation is evidentially dependent on normatively reconstructed history of science. Note that the "objectivity" of theory choice as here intended is compatible with choice being primarily a comparative and contextualized judgment among extant (actually existing) rival theories; most objectivists today see theory choice as a) comparative as between extant rivals, and b) contextualized (not by "discovery" and "justification" but) by historically-informed situations of "pursuit" and "acceptance." Similarly on the meta-methodological level, research "programmes," "traditions," etc. are typically seen as revisable; the sense of "justification" implied need not be taken as absolute, since the objectivist thesis as defined necessarily implies only that history of science is capable of providing objective support for the preference of one programme or tradition over another, a claim considerably weaker than the logicists originally supported. The sense attached to "objectivity" in the definition corresponds to an interpretation of the status of appraisal-norms in science, and of the sort of reason embodied in "normative" reconstructions of case history; this indicates an account of
explanation that attends the objective historicist’s account of the historiographical reconstruction of ‘internal history of science.’

"Contingent" appears to be the best characterization of the ‘left-leaning’ branch of the historicist movement, since perceived philosophical implications of the historical conditioning of conceptual changes, including change of standards, values and goals, are what lead to the rebuttal of objectivism and the affirmation of one or another form of cognitive relativism. This term "contingency" is therefore meant to refer to a class of interpretations of the historically conditioned character of norms, and not to the very notion of historical conditioning; for as we noted above, the objective historicists have also abandoned any pretense to treating norms as grounded ahistorically. The historical character of knowledge and the changing character of meta-level standards are read as undermining all notions of "immanent laws" of reason or history. The thesis of contingent historicism then is the assertion that there are no "objective" evaluations of rules, methods or decisions in any part of science (physical or social), and that because of the contingent character of historical knowledge itself, there is no inductive inference from history to normative philosophy of

3 Consider the close connection Andrew Pickering makes between relativism and contingency: "Even at the level of goal-formation and the dialectic of resistance and accommodation, where one can discern patterns and regular structure to practice, one can see that future knowledge production in intimately and irrevocably tied to the culture in which it is produced and which it is produced from. This, it seems to me, is an inescapably relativistic perspective on science...I pointed to the contingency—the sheer chanciness—of scientific practice. There is an element one cannot explain in goal-formation; the resistances that emerge in pursuit of goals cannot be foreseen or understood in advance; the possibilities for and contours of successful accommodations to resistances are likewise contingently discovered. This means that in telling the history of science, one has in the end to say—more and more often as one looks in increasingly fine detail—‘it just happened’" -Pickering (1991 Forthcoming), p. 28).

4 Laudan contrasts his view of methodological rules as "hypothetical imperatives" with the "categorical force" attached to them by logical empiricists; this is a clear indication of the a posteriori orientation of his conception of meta-science.
science that qualifies as "objective." Note that history of science and historiographical inquiry may be, and typically are, taken to be extremely important by contingent historicists. They, like the objective historicists, want to see theory brought back into harmony with actual practice. But descriptive and interpretive historiography, while serving well to undermine rationalistically and aprioristically inclined philosophies, is denied as being inductive ground for inference to "objective" normative reconstructions. Here again I attempt to make the definitions reflect what I take to be the fundamental issue of the dispute, the interpretation or explanation of normative discourse generally.

These definitions allow for recognition of what the two views essentially share as branches of "historicism." But the definitions just offered also hinge on persuasive definitions (glossings) that might be given by the theorists to "objective," "relative," etc. This is as it should be: whether offered self-consciously or not, argument by persuasive definition of key terms, particularly theoretical and normative terms, is a primary way in which normative meta-methodologists and their critics have played out their antithetical positions. When not offered self-consciously, such persuasive discourse often degenerates into loaded terminology and inflammatory rhetoric. Attention must be paid to objectivist and relativist arguments which lean upon question-begging rhetorical strategies. Making one's valuative assumptions

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5 While objective historicists may be naturalists, in the sense of advocating natural scientific methods for the social sciences, or anti-naturalists, advocating the objectivity of meta-methodology only for "physical," "mature," or some other subset of science, many contingent historicists typically reject the objectivity of criteria for adjudicating inter-paradigmatic disagreements in principle. While they may advocate or reject methodological differences between the physical and social sciences, the main thrust of their challenge is to rationalist and objectivist views about the justification and explanation of epistemological norms.

6 Not all glossings of theoretical terms need be considered a case of persuasive definition, since empirical and analytic definition are standard options. But glossings of normative-theoretical terms, I claim, typically do function as kinds of persuasive definition. This is the case whether the meaning of such 'mixed mode' terms are, to use the prescriptivist's dichotomy, 'primarily descriptive' or 'primarily prescriptive.'
explicit is one viable response to the shared tenet of all historicist schools that, to use Rudner's theme, "the scientist qua scientist makes value judgments." Reasoned discussion of persuasive definitions depends upon a recognition of the fallible and postulational character of normative posits, and the real shortcoming of the debate as I see it results from its not having been carried out on an explicit basis.

To some extent, Lakatos saw this and attempted to legitimize the postulational character of meta-standards by working them centrally into his "methodology of scientific research programs" (MSRP). However, as I argue in this chapter, the epistemological assumptions Lakatos makes in his development of the competition of research programmes obfuscates what was correct about his initial insight into legitimizing these aspects of meta-methodology. A rhetoric of the internal and the external abounds in Lakatos' thought on conceptual change and continuity, despite lack of substantive support. In logicist thought, as we saw, this contrast worked primarily to impose a framework for evaluation or explanation of beliefs/decisions, and also to encourage disparate attitudes towards the two contrasted frameworks. This is particularly evident in the quite antithetical positions we find espoused in recent historicist literature over issues of "evidential (probative, etc.) reason" and "social cause," in the explanation of belief and decision. Accordingly I will focus on the centrality in historicist literature of internal/external framework distinctions, and of methodological nomothetic assumptions structuring theoretical views about how the content of beliefs/actions is to be explained.

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4.1  Diverse reactions to the received view

The logicists located metascience at an enormous distance from actual scientific practice and its history; meta-methodology in the late 1950s and early 1960s would enact a number of reactions to this untenable a priorism, reactions that purported to lead to a reunion of philosophy with history of science and actual practice. Rudner, Hanson, Toulmin, Kuhn and Feyerabend, are names often cited as early representatives of this historicist turn. But the picture of metascience embodied in the "received view," to use F. Suppe's term, represented certain deep-seated pretensions of modern philosophy which could not easily be set aside. It is therefore not surprising that what is termed the "historical turn" in the philosophy of science took place under intellectual conditions still influenced by deductivism—influenced, that it, by what Bernstein calls the *Cartesian Anxiety*. The demand, so difficult to resist, to choose between 'truth *tout court*’ and 'truth relative to a framework' has been among the strongest influences on philosophy in our century.

This demand helped shape some of the radical implications writers like Kuhn and Feyerabend drew from their historicist-influenced criticisms of the ‘received views’ in the history and philosophy of science. It also, of course, helped shape the overt reactions of their early critics such as Shapere, Scheffler and Stove, who held to models of metascience much more in line with the received view. This shared *Cartesian Anxiety* may be said to be one of the contemporary sources for the current form of debate over cognitive relativism and objectivism.

I will avoid any direct arguments aimed at showing Kuhn to be a ‘relativist’, ‘consensualist’, ‘objectivist’ etc. Consistency was not Kuhn’s strong suit, and as he would later concede, his own inconsistency and ambiguity in the 1962 *The Structure of Scientific
Revolutions (SSR) was one primary cause of his being misunderstood. But the fault certainly was not all Kuhn’s. Since his work has been influential on later development in many schools and disciplines, it makes sense to speak of consensualistic (Rorty; Edinburgh school) relativistic (Shapere, Scheffler, Lakatos, Laudan) and even objectivistic (Holcomb) readings of Kuhn. One could hardly imagine three interpretations of Kuhn as far removed from one another as Laudan’s (1984 and 1985), Barnes (1982), and Holcomb’s (1989).8

Where the ‘real’ Kuhn’s affinities lie in the objectivism\relativism debate is not important for us; indeed that has been the focus of much of the inflammatory rhetoric in treatment of these issues in the last quarter century. My focus is on those assumptions which both branches of historicism still lean upon, which are shared with the logical empiricist school that went before. Two things are important here. The first I will only mention here, but not fully develop. This is that different understandings of what Kuhn "meant" are often reflections of the intellectual orientations of his readers. Joseph Rouse’s chapter entitled “Science as Practice: Two Readings of Thomas Kuhn” greatly clarifies what is significant about the wide diversity of responses to Kuhn’s work among both philosophers and social scientists.

What are we to make of these two ways of reading Kuhn? The first reading, Kuhn, treats science as the construction and appraisal of theories that aim to represent the world. It is replete with words like "believe," "accept," "see" or "observe," "theory," "counterinstance." Kuhn, challenges earlier philosophical accounts of what is involved in justifying and accepting a theory or observing the outcome of experiments, but does not deny that these are the important issues in the philosophy of science...Kuhn, does not present a revised account of science as the construction and appraisal of theoretical representations. Kuhn, challenges this more general framework for dealing with science that Kuhn, his logical empiricist predecessors, and many of his post-empiricist critics share. Science is not primarily a way of representing and observing the world, but rather

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a way (or ways) of manipulating and intervening in it...The shift from Kuhn, to Kuhn, is a transformation of the metaphors and models we use to understand science... Kuhn, replaces representing and observing with constructing, tinkering, and noticing as exemplars of scientific practice.9

Rouse’s perceptive distinction reveals an interesting aspect of the objectivist/relativist debate, one that had to do with Kuhn’s interpreters more than with Kuhn himself. Unlike Rouse, however, whose preference for Kuhn, is so clear in the title of his book, I have no philosophical axe to grind in this regard, or at least not one that is merely single-edged. It is enough here that we recognize these readings and the philosophical orientations that give rise to them, since these orientations help define the course of contemporary debate between, for instance, objectivist philosophy of science and radical constructivist sociology of science.

Secondly, and more importantly for us here, are the extravagances as well as the sound lessons of the historical turn. The Popper/Kuhn dispute in the early 1960’s is representative of both; by focusing on what was important and what was misguided on each side of this dispute, the middle ground I seek may be recovered. Some of the ‘extravagances’ I referred to are those ideas often identified with historicist relativism, which in general is characterizable as “framework” relativism.10 These are discussed in Section 4.2 below.

Were we to pursue the theoretical development of sociology, A. Gouldner’s noted article on "The Myth of Value-Free Sociology," and the works of C. Wright Mills represent some

9 J. Rouse (1987), Knowledge and Power: Toward a political philosophy of science, p.36-8.

10 Not all cognitive relativism is what Krausz (1989) lucidly calls “framework relativism”; there are non-framework relativisms on the market today as well. "Besides characteristic ‘framework’ relativisms, we should note ‘nonframework’ ones, such as those which hold that of a given domain the range of ideally admissible interpretations is multiple. That is, more than one uniquely correct interpretation applies to it, on account of a multiplicity of its standards —which in turn may well be incongruent." -Krausz, introduction to Krausz, ed. (1989) Relativism: Interpretation and Confrontation (Indiana: University of Notre Dame Press). My pragmatic "bootstrap" response to non-framework relativism, and discussion of the importance of a philosophical distinction between pluralism and relativism, are elaborated in Chapter Six.
important junctures of thought. The influence of contingent historicism upon Mills is particularly exemplary of the conceptual schism that occurred during the 1950's and 1960's in that field. For Mills, "there is, I believe, no [law] stated by any social scientist that is transhistorical, that must not be understood as having to do with the specific structure of some period."\(^{11}\) For my purposes, Mill's notion of a "period" may be placed with such other framework terms as "culture," "conceptual scheme," "language," "taxonomy," etc.

Were we to pursue this schism in the philosophy of history, we would find at this historical juncture heated disagreements between the positivist-influenced thinkers and the interpretive or *verstehen* traditions. This latter Durkheimean tradition, as Durkheim himself elaborated greatly in his lectures, found much affinity with American pragmatism. While I do not focus on the interaction of the analytic tradition with Continental thought, I want briefly to summarize some of the broader connections between my pancritical pragmatism and the Continental traditions. This I do by recalling the significant divergence I argued for in the last chapter between pragmatist treatment of means and ends, and logical empiricist versions of instrumentalism. The "technical-cognitive interest" in prediction and control; the "practical-cognitive interest" in communicative interaction and intersubjective understanding; the "emancipatory interest" in critique of influences of authority and power—these, to use Jurgen Habermas' terms, are the defining foci of *instrumentalism, critical theory, and hermeneutics*, respectively. The critical theory of the Frankfurt school and more recently of Habermas, as well as the interpretive or hermeneutic school of Foucault and others, represent alternative Continental traditions. Each gained considerable ground during this period through their respective central foci on the "hermeneutic problem" and on the "critique of scientism."

\(^{11}\) Mills, *The Sociological Imagination* (1959), p. 150. Mills at times took the neo-Marxist position that this historical character of knowledge warranted the interpretive or anachronical methods of historical materialism.
As I intended my argument in the last chapter to indicate, the pragmatic philosophy has affinity with all three of these traditions, and not merely the LE account that picked up upon Dewey's term "instrumentalism." Dewey's contextualism is general, like the hermeneuticists, not selective, like the logical empiricists; the meanings of observational as well as valuative utterances are both contextual, so that one fails to find in his thought the contrast between facts with intrinsic meaning and values with only instrumental worth. Dewey's pragmatism ties cognitive as well as ethical standards to human interests, like the critical theorists, and focuses on the generation, criticism and improvement of the ends of action in any normative system. Indeed for Dewey, instrumentalism was itself a tool for technology criticism. Larry Hickman has recently argued this important point, although he concedes that how such a project functions is not an area which Dewey adequately developed.\textsuperscript{12}

Changing views about history as a discipline are more central to my focus than are changing views about sociology and social theory. For this is where the issues of causal and intentional explanation are most directly dealt with, and it is these issues that most clearly inform us of polarized conceptions about the relationship of history and philosophy. During the 1950s and 1960s, the rift between philosophy and history was great; there was widespread mistrust of causal judgement in history among practicing historians. It was analytic philosophers, not historians who argued most strongly for the possibility of objective history, and the adequacy of what came to be known as analytic philosophy of history. In terms of alternatives to analytic philosophy, R. G. Collingwood's thought was as great an influence over philosophy of history as was Durkheim's over sociology, and led many historians to refuse accepting analytical philosophy of history. Around midcentury, and through at least the 1970's, such figures as W. Dray, and M. Scriven were central in these debates about

\textsuperscript{12} Hickman (1989) \textit{Dewey's Pragmatic Technology}.  

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historical explanation, especially in their published exchanges with Hempel, Popper, Nagel, M. Mandelbaum, and others defending some version of the deductive-nomological model as adequate for historical explanation.

We need not delve very deeply into the controversy over differences between the physical sciences, the social sciences, and the humanities, in order to see what was wrong with the form of naturalism tied to the thesis of the unity of science—that form which asserted the possibility of 'scientific' history and a hierarchical model of the sciences. What was wrong with it was the viability of probabilistic and deductive-nomothetic modes of explanation methods as uniquely or exclusively appropriate to historical and social scientific fields of study.

It was William Dray who, in his *Laws and Explanation in History* (1957) dubbed the deductive-nomological (D-N) thesis a 'covering-law' model of explanation, and who attacked its applicability to historical study. This dispute raises to focus the self-conception of the social sciences and humanities, and the complex questions of the relationship between "causal" and "epistemologically-couched" (or simply 'good reasons') explanation of actions and decisions of historical agents. Implicitly or explicitly, both objective and contingent historicists have based their conceptions of historiography and of the warranting role of historiographical knowledge, on answers to these questions. This raises serious issues about explanation, history, and historiographical method as they come to be treated by the two main branches of historicism. To these highly controversial issues we can now turn.
4.2 Science as social practice: the historicist’s challenge

In 1959, Sir Karl Popper found it significant to assert that "the logic of knowledge is concerned only with logical relations" (1959, p. 30). This claim, which surely appears tautological at first glance, gains importance when understood in the background of the Carnapian/Reichenbachian dogma of mutually-exclusive internal and external questions. Popper’s point is to qualify, --albeit in an arguably insufficient manner-- Reichenbach’s well-known assertion that "epistemology is interested in internal relations only" (1934, p. 30). The intended position is not identical to Reichenbach’s, because Popper also held that scientific knowledge is not produced by, or limited to, only a "logic of knowledge." Logical analysis is not enough, for "if we characterize empirical science merely by the formal or logical structure of its statements, we shall not be able to exclude from it that prevalent form of metaphysics which results from elevating an obsolete scientific theory into an incontrovertible truth."13 The demarcation of science from metaphysics (or at least from non-science) is still for Popper as for the positivists a primary function of philosophy of science. But Popper here concedes the inadequacy of the logicist’s attempt to provide a demarcation criteria on purely formal grounds.

What Popper called his "methodological supplement" to the logic of science is not very extensive or satisfactory, and how this supplement was to be grounded remains an aspect of inconsistency in his work. The "methodological supplement" Popper affirmed lacked the kind of justification he himself demanded of "disembodied" or "objective" character of knowledge in science. This can be seen by reflecting on the "conventional" character he attributes to

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13 Popper (1959), p. 50. Paraphrased from discussion by M. Pera (1989), p. 169. Popper, as mentioned above, is especially insistent on removing from historiography the dialectical materialist bias of Mills and other neo-Marxists.
methodological rules, and the contrast he makes between objective knowledge and social
convention. Popper seems to assent to the view that the philosopher of science must either be
a dogmatist or a conventionalist about methods. Logicism, as the last quote indicates, was
viewed as dogmatic. Of course, in adopting a conventionalistic view of the normative
standards of appraisal in science, he was only making explicit what we previously saw was
already implicit in logical empiricist thinking about standards in science. The Carnap/Popper
dispute largely concerned inductive probabilism as opposed to falsificationism as the logic of
hypothesis testing, and did not seriously bring into question the status of rules, or the
understanding of the normative dimensions of metascience.

This is one reason why Popper could make little more sense of the status of
methodological rules and their possible criticism than the logicists. Popper’s falsificationism
introduced a critical canon by which hypothesis-testing was to lead to the improvement of
theory; but this critical canon is either missing or inconsistently applied to the meta-
methodological issues. The inconsistency in Popper’s thought can be seen in his attempt to
claim both that the goals of science can change or grow (in a way parallel to the growth of
scientific knowledge), and also that methods and methodological rules are conventional and
thereby untestable, and highly limited in the extent to which rational criticism bears upon
them. Hence whatever “methodological supplement” is needed to make demarcation criteria
plausible is apparently itself beyond the scope of criticism. On the side of rational criticism,
Popper raises little more than a consistency requirement for methods and rules, and his
conventionalism doesn’t allow for the use of history of science as a test.

Popper had thought his thoroughgoing conventionalism at the methodological level
liberalized the Carnapian semantic conception of theory and Carnap’s inductionist
confirmation procedures.\textsuperscript{14} He was correct about this, but it appears to make virtual
nonsense of the semantic conception, if not of scientific reasoning itself. Popper at times
sought to 're-habilitate' the notion of convention in order to avoid its relativistic connotations
in the logical empiricist vernacular. He said at several points that methodological judgments
are "pragmatic" and judgable by consequences and implications. But he supplied no theory of
this and his account makes little progress over that of Carnap to 'pragmatize' logicism.
Indeed Popper's attempt contains a deep inconsistency to the extent that this effort to re-
describe the methodological practice of scientific knowledge-production works to undermine
his own characterization of the content of objective scientific knowledge. Popper's
conventionalism then, like the logicist's, fits ill with the hypothetico-deductive account given
of objective scientific knowledge. If such terms as habit, convention, intuition, etc. are
going to be rehabilitated, as I hope they will be, it must be on the basis of an account in
which the conception of the content of scientific knowledge coheres with the conception
acceptable to the scientific community of science as a practice.

As the great critic of historicism and psychologism, Popper was hardly disposed or
prepared to go very far with the attribution of "empirical law" or strictly universal claim to
historical generalizations. But he still maintained that the deductive-nomological model
provided demarcation criteria of science, and that the universal laws this model demanded
were shown scientific by being empirically falsifiable. Popper divided the sciences into the
"historical" and the "generalizing," and insisted that the empirical laws used in history were
not 'discovered' ala Marx, but imported from the theoretical sciences. Another way to put
this is that although he qualified the notion of a logic of knowledge, his own commitment to a
deductive-nomological view of history still left him with no viable way to exclude from

\textsuperscript{14}See also Laudan (1984).
science 'that prevalent form of metaphysics' that he found exemplified in the historical idealism of Hegel and historical materialism of Marx. We may shift focus to Hempel's treatment of explanation, since it was he who most consistently argued for a deductive-nomological construal of history. Hempel's philosophy of explanation is a more central focus here, since he directly entered into debate with Dray.¹⁵

Hempel argued that "Historical explanation, too, aims at showing that the event in question was not a 'matter of chance,' but was to be expected in view of certain antecedent or simultaneous conditions. The expectation referred to is not prophecy or divination, but rational scientific anticipation which rests on the subsumption of general laws."¹⁶ A certain degree of rationality is attributed by Hempel to scientific actors as a "higher order disposition." In the crude form in which Hempel puts it, this meant that in "intentional" explanation, that is, explanation by motivating reasons such as goals, desires, etc., the concept of a rational agent functions "as an idealized explanatory model comparable to the explanatory concept of an ideal gas."¹⁷ Presumably Hempel has in mind the "partial covering-law" (D-N) model mentioned earlier. Even in Hempel's most current statement, he continues to hold that, "Nothing I have said is meant to imply that elaborate deductively organized theoretical arguments do not play a highly important role in science, or that the precision and rigor of the deduction of specific quantitative implications from mathematically formulated theories is somehow illusory: the contrary is usually the case" (1988, p. 14). But


Hempel does make far deeper concessions to his Kuhnian critics than he previously had. He continues,

The limitations of deductivist procedures present themselves, rather, in the context of applying the theoretical apparatus to empirical subject matter. Kuhn has repeatedly insisted that the attaching of symbolic labels to nature often is not subject to, or learned by means of, precise rules, but is acquired by the scientist in the course of his professional training and career, somewhat in the manner in which we learn to master large parts of ordinary language without the benefit of rules, by observation and imitation of the way others use the language. 18

These recognized limits to the deductive-nomological model reflect concessions concerning what was wrong with nominalistic reductionism and the thesis of the methodological unity of the sciences. Returning to Popper's philosophy, Popper represented the objectivity of scientific knowledge as a discovered mind-independent "third-world" (Popper 1963; 1972). 19 This notion is intimately connected with Popper's realism and with rationalistic views about knowledge; it has, of course, been most severely criticized as unviable for social scientific knowledge and history, although few now see Popper's characterization as adequate for the physical scientific knowledge either. What Popper needed in order to maintain the mind-independent character of scientific knowledge were meta-standards of criticism that were indisputably of "third-world" character themselves; and at times he attempted to portray cognitive values in science as theory-neutral, content-neutral,


19 See Popper (1972) Objective Knowledge. Lakatos writes, "The first world is the material world, the second the world of consciousness, the third is the world of propositions, truths, standards: the world of objective knowledge (1978, p. 92.)" Part of my criticism is that World Three here is a hodge-podge that conflates issues that should be kept distinct. In particular, standards, which are human normative posits, are ill-conceived when thrown together with discovered aspects of a world of objective truth. While standards may perhaps be phrased as hypothetical imperatives, to view them as truths reflecting autonomous or mind-independent knowledge denatures the unique normative character of their meaning.
and applicable to all science at all times. But this would be to deny that cognitive values are part of the "methodological supplement" he portrays as conventional, and to retreat to the logicist account of them which he hoped to avoid. Neo-Popperians such as W. W. Bartley rightly labeled Popper's rationalism unncritical and even "fideistic" where it leans upon 'third-world' assumptions in characterizing the rules and values of scientific appraisal.

Recent objective historicists including Laudan point out some of these inconsistencies in Popper's critical philosophy; criticism of methodological rules, it would seem, ought to be allowable on a naturalized view of epistemology, since—if methods correlate with aims— it would seem to follow from Popper's own thesis about the possible growth of aims! Although Popper describes his epistemology as a "critical" rationalism, he doesn't allow this sense of a reciprocally critical relationship between methodology and either theory or axiology. Popper remains skeptical of inductive methods, and there is little more than a simplistic empiricism allowed into the assessment of relationships between ends and means. Some of these inconsistencies are addressed well by contemporary Popperians, many of whom identify with a "critical" or non-justificationist epistemology that represents a far broader revolution in epistemology than Popper himself originally advocated.

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20 The naturalism/non-naturalism issue, I have previously said, brings in questions of the relation of normative judgments to descriptive evidence. Popper says that norms cannot be derived from nor reduced to facts, since facts are always evaluated by norms, and norms always pertain to facts. While I support this anti-reductionism, we must see that this dialectic is more complex than originally recognized: for facts pertain to norms and norms are also used to set side constraints on descriptive claims.

21 Bartley's move also shows us that Popper is guilty of his own version of the "myth of the framework" despite himself. The abandonment of justificationism should mean that we focus on the competition between alternatives. The "pan-critical" approach towards which Bartley and other Neo-Popperians urge him denies that any part of theoretical science is certain, but insists also that no integral part of it is necessarily 'arbitrary,' purely conventional, a matter of intuitive psychology, or otherwise closed to criticism. In one of his last works before his recent death, W. W. Bartley credited himself for a suggestion that Popper later adopted: "I proposed a contrast between justificationist and non-justificationist theories of criticism as a generalization of his (Popper's) distinction between verification and falsification." —See Popper (1963), p. ix. Bartley's contrast here is the justificationism of Modern philosophy against the 'preferentialism' of critical theory: from how can we 'prove' theories, to how can
No doubt Kuhn’s influence is great over post-logicist reconceptions of scientific practices, and especially over methods of historiography. While we need not discuss the different readings of Kuhn again, Kuhn’s own views and criticisms of analytic philosophy of history and the covering law model are summarized in his article “The Relationship Between the History and the Philosophy of Science,” contained in *The Essential Tension*. Kuhn’s challenge to Hempel and Popper is there again represented by Kuhn’s call for “a rational reconstruction of science different from that now current,” one more attuned to science as a social practice, and to philosophy of science as historically informed.

4.3 The myth of the framework: Popper’s challenge

While Popper’s account of meta-methodology, as we have seen, remains mired in an unhappy mix of apriorism and conventionalism, he was keenly aware of the fallacies of radical historicism in the 1960s and 1970s. Popper saw at an early stage the positivist

we ‘improve’ them. The focus of this critical view is not restricted to hypotheses, but allows the inclusion of methodological and axiological issues under a “pancritical” view of reason.

22 There Kuhn writes that “No one in recent years has done so much to clarify and deepen my consideration of philosophical problems as my Princeton colleague C. G. Hempel. But my discourse with him and my acquaintance with his work does nothing for me at all when I work on, say, the history of thermodynamics or of the quantum theory. I commend his courses to my history students, but I do not especially urge that they enroll” - p. 12. For contemporary treatments of some of these issues, see N. Nersessian (ed.) (1987), *The Process of Science*, and T. Lavine and V. Tejera, (eds.) (1989) *History and Anti-History in Philosophy.*

23 Kuhn says that his concern with the ‘covering law model’ “is as an articulated version of a widely diffused image of history, one that makes the discipline seem uninteresting to those who seek lawlike generalizations, philosophers, scientists, and the social scientists in particular”. Kuhn (1977), p. 15. The naturalist’s attempt to carry this model over from the natural sciences to history results in “an almost total misfit” of method to the content of history as a field of study. This may be something of an overstatement, or at least contains a position in the question of the relationship between history and the sciences that we need not become involved in here. Kuhn is against the objective historicist ‘marriage’ of philosophy and history, which he says differs from making philosophy historically informed.

24 See also Popper’s *The Poverty of Historicism* (1957).
influence on Kuhn's distinction between "normal" and "revolutionary" science, and particularly on the attitudinal disparity with which Kuhn treated these two posited contexts. This is amply illustrated in the metaphor, which Kuhn has never given up as apt, characterizing adherents of different paradigms as 'inhabiting different worlds.' It is also illustrated in the terminology Kuhn uses, which leans markedly on contrasting standard-using reason in normal science, with terms such as "persuasion" and "conversion experience," "gestalt switch," etc. used to characterize periods of revolutionary science and inter-paradigmatic discourse.

The inferential strategies employed in normal science, where discourse is, according to norms established by one predominant paradigm, continually contrasted with "persuasion" as the vehicle of converting advocates of one paradigm to abandon it in favor of another at the beginning or end of a revolution. Kuhn's "postscript" to the second edition includes an attempt to re-address the issues of consensus formation by taking on the Quinean notion of language communities and radical translation. But Kuhn's contrast between "proof" and "persuasion" is as marked as ever, albeit qualified. While saying that "Nothing about that relatively [very!] familiar thesis implies either that there are no good reasons for being persuaded or that those reasons are not ultimately decisive for the group," Kuhn also insists on the aptness of the proof/persuasion distinction to capture the differences he finds important in his own distinction between normal and revolutionary science. In revolutionary times, "debate is about premises, and its recourse is to persuasion as a prelude to the possibility of proof" ("Postscript" p. 199).

*Kuhn's distinction between normal and revolutionary science, in short, deeply reflected the dichotomy between standard using and standard choosing, and hence the Carnapian philosophy of radically contrasted "internal" and "external" questions. What upset many of*
Kuhn’s critics was the dependence of normal science on periods of revolution, and hence on the psychology and idiosyncratic influences which Kuhn alleged characterize the emergence of a new consensus. But I have shown that a similar dependence was present at least implicitly in the thought of Carnap and Reichenbach in the 1950s. These two did not need Kuhn to deconstruct their cumulative notion of scientific progress; for in the course of unsuccessful attempts to resurrect the logicist programme, that account had self-destructed!

This relates directly again to the problems Carnap encounters due to his contrast of theoretical and observational languages, and his account of theoretical language as "partially interpretable," as discussed in relation to Jane English’s article in the last chapter. Herein at mid-century lie the seeds of the "incommensurability" problem, although that problem in its current form only arose a decade later after Kuhn chose the questionable term "incommensurable" to characterize the "partial communication" that partakes in inter--as contrasted with intra--paradigmatic discourse. The problems of partial interpretation and of partial communication issues from the same source. But this important aspect of the debate about "incommensurability" is rarely recognized by advocates of contingent historicist research traditions, perhaps because the issues are presented so differently by the logicist Carnap and the historicist Kuhn. This is one clear reason why some objective historicist critics such as Laudan (and Pinnick) find it important to focus attention on the original 1962 publication of SSR, rather than on Kuhn’s later publications.

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25Under criticism, Kuhn swiftly moved to liberalize his conception of persuasion, and thereby of revolutionary science and post-revolution consensus-formation as well. These qualifications, discussed in the "Postscript," do not I think impede the correctness of his criticism of the "cumulativist" theory of progress, since that theory can, and in my opinion should, be criticized independently of this distinction. But clearly, Kuhn’s attack on the logical empiricist’s notion of progress as cumulative, gained all the more persuasive force by being based dialectically on a distinction that already had deep roots in that tradition.
Popper also discussed the Carnap\Kuhn connection in "The Myth of the Framework" (1965; revised, 1972). This article criticizes what in my terms are contingent historicism and the "framework" relativism to which it is given. The myth, he said, is one that inclines us to believe the impossibility of rationally discussing the validity of our meta-epistemological principles, for in attempting to do so we must be appealing already to principles or axioms. The two apparent responses to the supposed regress are framework absolutism or framework relativism.

But all of this is mistaken; for behind it there is the tacit assumption that a rational discussion must have the character of a justification, or of a proof or demonstration, or of a logical derivation from admitted premises. But the kind of discussion which is going on in the natural sciences might have taught our philosophers that there is another kind of rational discussion.\textsuperscript{26}

Does what was wrong with the received view warrant the strong anti-normativism of some of Kuhn’s contemporaries? Popper’s account leaves plenty of reason to doubt critical rationalism\textsuperscript{27} and to develop the conception of science as a social practice. But given the role that "convention" plays in the received view prior to 1960, it becomes all too easy for the radical historicist to put a consensualist or socio-psychological glossing on precisely those aspects of scientific practice that the received view had dubbed "conventional." This is clearly evidenced in the radical historicism of Feyerabend, and appears to be an important part of Kuhn’s position as well.

\textsuperscript{26} Popper, (1965), p. 45. "The proponents of the view of the myth of the framework distinguish sharply between rational periods of science conducted within a framework (which can be described as periods of closed or authoritative science) and periods of crisis and revolution, which can be described as the almost irrational leap (comparable to a religious conversion) from one framework to another" -p. 43.

\textsuperscript{27} One example of this recent non-justificationist philosophy is D. Collingridge (1987) \textit{Criticism – Its Philosophical Structure}. 141
Kuhn’s use of the term "persuasive" in SSR is one particularly strong example of what I mean. There Kuhn glosses persuasion in terms of "subjective" in contrast to "objective" factors, and "idiosyncratic" in contrast to "rule-governed" decision. How much of this is really a challenge to scientific objectivity, and how much a conflation (in reaction to the logicist’s earlier insulationist partitioning) of individual decision and normative theory?

In his "Postscript" to the second edition, as well as in The Essential Tension (1977) and elsewhere, Kuhn says that "persuasive" discourse should not be interpreted as implying "extra-rational," etc. In saying this, Kuhn is rightly pointing out a need to revamp our understanding of the rhetorical dimension of normative discourse, including that of the sciences. Yet this does not justify, and indeed, I believe is undermined, by Kuhn’s 1962 extensive use of the contrast of proof and persuasion.

What that contrast indicates instead is Kuhn’s own leaning towards a version of the myth of the framework. Kuhn’s emphasis on ‘idiosyncratic judgment’ and rhetorical ‘persuasion’ may be seen instructively as giving a psycho-social twist to precisely the ‘external,’ ‘pragmatic,’ "extra-theoretical," "conventional," etc. issues that the logicists implicitly conceded ‘internal’ issues depended upon. It was in significant part, I conclude, because "convention" played so strong a role in the received view, that a view of paradigm-choice as ultimately socio-political and its justification as merely consensual, has been considered tempting by so many post-logicist thinkers.

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28 See Kuhn’s (1977) essay "Objectivity, Value Judgment, and Theory Choice."

29 Again, please avoid reading me as claiming that Kuhn is pleased with such readings of himself. The point is that we no longer need the kind of persuasive contrast Kuhn utilized in order to characterize the importance of history and social studies for our understanding either of the knowledge-producing practices of the sciences, or of the nature of the knowledge produced. Kuhn has made his own case for this much more clearly in The Essential Tension. As with terms like ‘relativist’, etc., I am not here claiming that Kuhn was a consistent consensualist. Consistency was not Kuhn’s strong suit, but it certainly makes sense to speak of the consensualist reading of Kuhn, since his work has been influential on later development in the Sociology of Scientific Knowledge.
What we are seeing, then, is how deeply the tension between consentualism and normative objectivity affects recent treatments of the natural and the normative. We have already seen how consensualism is part of the received view of valuative commitments in normative practices other than science. Norms of action, as we saw in the last chapter, were typically treated as issues in which decision is ultimately king. To review, Ayer's 'vicious circularity' charge against ethical systems, Hare's retreat to "principles of decision" at the root of ethical systems, and Harman's claim that the most plausible versions of naturalism involve moral relativism, all share a consentualist orientation about standard-choosing with respect to norms of action. Harman’s development of a moral relativist position, in particular, was expressly phrased in terms of a commitment to consensualism (and a strictly instrumental or minimalistic conception of rationality) tied to some de facto set of social norms or communal consensus. Such a consensualism, according to Harman, should be acceptable to naturalists because it

denies that there are universal basic moral demands and says different people are subject to different basic moral demands depending on the social customs, practices, conventions, values and principles that they accept.³⁰

Now, despite the fact that Harman, like the logical empiricists, employed this consensualism selectively, while Kuhnians (and many in "science studies" departments today) do not, there is a striking parallel between Harman's and Kuhn's consentualist leanings. Yet there is also an enormous lack of parallel between Harman's use of a consentualist approach to argue explicitly for moral relativism, and Kuhn's attempt to convince his objectivist critics that his

³⁰ From Krausz, ed. (1990), p. 10.
own consensualist account of consensus formation does not, even implicitly, harbor cognitive relativism.

It is not uncommon, then, to find critics of Kuhn arguing that he has an account only of disensus or scientific revolution, but no viable conception of consensus formation (Laudan 1984); or again that he understands the criteria of paradigm-choice in a way that deprives it of any genuine normative force, because he accords paradigms, to use G. Canguilhem’s exemplary statement, "only an empirical mode of existence as cultural facts." The gestalt-switch, and the notion that consensus formation occurs when the progenitors of the old paradigm “die off” and are replaced by a younger group, hardly suffice, as Laudan and others have made abundantly clear. Canguilhem’s charge is substantiated, I would hold, to the extent that Kuhn still holds a version of the framework myth rightly ascribed to his original position by Popper.

Kuhn has repeated even recently that in SSR he insisted that new paradigms do not "triumph ultimately through some mystical aesthetic"; yet he certainly failed to make sense of issues of continuity and consensus formation, either in his 1962 treatise, or I think in his later essays on the subject in The Essential Tension. His accounts of text-book learning and of immature and mature science were supposed to fill some of these roles, and have been much disputed. In this regard Kuhn’s influence over to the reconceptualization of science as social practice can hardly be overstated. But I have insisted also that this reconceptualization should not take on the insuperable problems of linguistic philosophy and framework relativism.

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32 The discussion of the continuity of research traditions must avoid two extremes, as Sarkar points out:
1) That it’s the result of a sequence of socio-psychological linkages,
2) That it’s the result of rationally following out premises and rules.
In answering his objectivist critics about theory-choice, Kuhn has recently rephrased one of the central arguments of SSR in the following way:

What I am denying then is neither the existence of good reasons nor that these reasons are of the sort usually described. I am, however, insisting that such reasons constitute values to be used in making choices rather than rules of choice. Scientists who share them may nevertheless make different choices in the same concrete situation. Two factors are deeply involved. First, in many concrete situations, different values, although all constitutive of good reasons, dictate different conclusions, different choices. In such cases of value-conflict (e.g. one theory is simpler but the other is more accurate) the relative weight placed on different values by different individuals can play a decisive role in individual choice. More important, though scientists share these values and must continue to do so if science is to survive, they do not all apply them in the same way. Simplicity, scope, fruitfulness, and even accuracy can be judged quite differently (which is not to say they may be judged arbitrarily) by different people. Again, they may differ in their conclusions without violating any accepted rule.\(^{33}\)

Kuhn is speaking of "theory-choice," and we cannot easily assimilate what he says here to choice of paradigm or "disciplinary matrix." In this sense the above passage will hardly satisfy his objective historicist critics who, like Popper, see Kuhn's incommensurability problem as falling out of his emphasis on community consensus and individual idiosyncracies (subjective factors) at the meta-level of paradigm choice. These are problems that I think Kuhn cannot adequately address with the kind of language he adopts, which includes the contrast of "objective" and "subjective" factors in theory and paradigm choice. But it should be noted that Kuhn here is no longer leaning on a strong version of incommensurability, and in The Essential Tension his strong holism remains an issue primarily in the field of historiography. Progenitors of different paradigms, Kuhn always conservatively held, "share the same list of values." The issue he still finds important, then, is not so much the partial communication across paradigms, as the potential for idiosyncratic elements to enter when

focussing on individual choice. This, however, is hardly an issue that many self-described objectivists would take as epistemically central. *It cannot be emphasized too strongly how damaging the conflation of epistemic issues of belief warranting with cognitive psychological issues of doxastic decision processes has been to communication about what is at stake in these debates.* It would certainly be to make a straw figure of mainstream epistemologists to present them as assimilating the normative force of prescribed standards of belief-warranting with the causality of doxastic decisions. But many historicists have fallen into this assimilation far too easily themselves, either by their own fault or by the failure of epistemologists to clarify the distinction. It is only in the context of doxastic decision processes that the question of determinism or indeterminism arises. As a soft indeterminist on that controversy, I hold that both the insulation from, and the direct determination by, psycho-social factors, are mistaken views. If this is a plausible position, the question for both sides then becomes, how far is the separation of normative epistemology from cognitive psychology a matter of clarifying issues that should remain distinct, and how far is it a reflection of a dubious objectivist construal of scientific rationality (i.e. normative decision-making) as *insulation* from psycho-social influence?

On my view, no relativism is *necessarily* entailed by the consideration that idiosyncratic elements *may* or (as an empirical claim) *often* do have an important effect on normative decision-making; nor by the fact that progenitors of one paradigm may place different weight on values than progenitors of another. The issue of relative weighting of values must be raised to central focus, both in respect to individual psychology and to what Kuhn calls inter-paradigmatic dispute. Kuhn's problem, as we will see more fully in Chapter 6, leads to the importance of human interests and axiological decisions, since it is in part the varying
cognitive interests in explanation that scientists may hold, that lead them to weigh values differently and to choose diverging sub and surrogate aims for inquiry.

My conclusion is that the radical consensualist strains of Kuhn's (1962) thought go together with his "conservative" contrast of proof and persuasion, and that this contrast is deeply influenced by background logical empiricist views. Kuhn simply fails to see how much his adoption of this contrast involves him in the problems of the logical empiricist metaphilosophy. This is parallel to the parasitic nature of Popper's conventionalism on the logical empiricist tradition. Indeed, what we have here is one problem, not two: consensualism after all is itself only a variety of conventionalism, one which insists that those conventions supported by the highest degree of communal consent are the last arbiter of normative authority or objectivity. Kuhn's inability to make rational sense of "persuasive" arguments for alternative paradigms, and Popper's inability to make rational sense of the criticism of "conventional" posits, are two skids off the same row. Positivism (as the philosophical status quo) had delighted in instilling a deep sense of the poverty of reason in the district of standard-choosing. Popper's radical conventionalism and Kuhn's thesis of meaning incommensurability are two children of this ghetto row. Perhaps that is why both theses have often seemed so heroic to critics of the status quo in the philosophy of science!

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34It should also be said that the arbiter of normative authority should not be equated with a statement of necessary and sufficient conditions for a rational cognitive decision. Kuhn started with a sharp contrast of persuasion and disagreement in which closure is possible, but years later said that he intended persuasion to include reasoned argument. The latter usage of course moves away from the way the term was treated in non-cognitivist literature, to something closer to a general notion of rhetoric. Kuhn's shift shows precisely the confusion present in his thought concerning discourse in standard-choosing contexts.
A related issue in the Popper/Kuhn debate which plays quite prominently in Popper's article is that of methodological holism.\textsuperscript{35} Popper's critique of the myth of the framework includes a refutation of the strong holism of Kuhn and Quine, the thesis that response to criticism of a theoretical system may come in the form of changes made anywhere in that system, and that rational selection rules or testing methods concerning the units smaller than these theoretical whole, can be counted out in principle.\textsuperscript{36} Strong holism is closely connected with the incommensurability problem in the thought of both Kuhn and Feyerabend. Kuhn also pays deference to Quine and others in saying that progenitors of different paradigms must see themselves as members of different language communities, and then attempt to "translate." Popper rightly objects that linguistic philosophy and its heritage pervades this analogy. The unit of selection issue is ill-represented when "evaluation" or testing is held to be subject to strong holism.\textsuperscript{37} Popper is quite clear in seeing the relativistic

\textsuperscript{35}Susan James has usefully distinguished between two controversies in which the term "holism" centrally arises. What I call methodological holism can be identified with holism of form, a holistic position in the issue about how terms in a theory are inter-related. This can be contrasted with holism of content, the holist position in the issue about whether social scientists should take individuals or collectivities as the primary/exclusive focus of study. On the latter issue, my position is not far different than James' "concessive holism." See James (1984) \textit{The Content of Social Explanation}.

\textsuperscript{36}Popper (1965) p. 97. This contrasts with weak holism, which only denies the possibility of a disproof of any separate component of a theoretical system, and does not presume reason is helpless to narrow this range of ways in which a system may be revised without becoming degenerative, ad hoc, or unscientific. Laudan has written extensively on the theoretical extravagances committed by contingent historicists who identify with strong holism. Laudan denies there are "infinitely many possibilities of how to replace" damaged premisses by making adjustments in some distant part, and restoring consistency; he is similarly critical of the idea that we can find plausible corroborating evidence "for even the most 'absurd' programme' if the search has sufficient drive." Laudan is right here, though Lakatos' legacy clearly shows that we cannot lean on individual rationality theory to show where modifications must be made or what length of time one may hold onto a faltering programme or tradition.

\textsuperscript{37}Kuhn, Feyerabend and Quine are noted strong holists; Duhem himself was more of a weak holist. Part of Quine's dogmatic holism was the idea that the total field of theory is so underdetermined by experience that there is virtually unlimited latitude in choice as to what part of the system to modify in the face of criticism or recalcitrant instances. Judgments of equilibrium within a system are possible, but criticism of specific aspects of it

148
import of these understandings of the unit-of-selection issue in science as due to relativistic currents already implicit in logical empiricism; he cites the influence on the views of Duhem, Quine, and Kuhn, by Whorf's notion that the structural rules of our language form a prison in which we live. This influence of philosophy of language is clearly seen in the common notion that the unit of selection is a linguistic framework, and that such frameworks and the 'epistemic conditions for commensuration' between them are analogous to 'natural languages' and 'conditions of translatability.'

The strong holist, incommensurability and underdetermination theses provided much of the work for Kuhn in SSR, and are still the basis for much radical historicist thought today. While I find the dependence on these dubious theses quite objectionable, I will pass over any lengthy discussion of them, because the influence of positivist thought on the understanding of these issues has already been discussed adequately by Laudan. Without going into further

are problematized. Popper and Laudan are foremost among those who reject the thesis. Popper's article included an attack on the "holistic dogma of the 'global' character of all tests" ("Myth", p. 98).


Linguistic philosophy has affected discussions of "incommensurability"; we may presume that the incommensurability thesis is most interesting where it claims a disagreement stronger than simple contradiction, and not so strong as incomparability. Kuhn's use of the term "partial" understanding underlines this. But the identification of the theoretical framework with linguistic schemes has its roots in logical empiricism. The empiricists defended the idea of a neutral observation language into which all scientific theoretical languages could be translated. Even conceding there is no one-to-one translation of terms from one theory to another, the neutral observation language was taken as a middle ground through which translatability without remainder could be accomplished. Quine's discussions of the holism and the indeterminacy of translation are wholly reflective of requirements drawn from linguistic philosophy. It comes to be challenged in Davidson's articulation of the nonsensicality of thinking of conceptual schemes in this linguistic medium. Arguably this influence continues in the assumptions common among self-avowed cognitive relativists that even partial commensuration depends on formal conditions of translatability between languages. Laudan responds by showing commensurability can be very partial and still allow a normative meta-methodological practice to get off the ground.

See also Laudan, "De-Mystifying Underdetermination."
detail, Laudan’s recent (1989) "‘The Sins of the Fathers...’: Positivist Origins of Post-Positivist Relativisms" can be cited as a prime example of the forceful critique of positivist assumptions in contingent historicism by an historically-oriented objectivist. I have previously (Chapter Three) discussed Laudan’s gradualist model of change as a favorable response to strong methodological holism, and won’t pursue the connection between holism and linguistic philosophy further here.

Kuhn’s “radical historicism” and his "conservativism," I have attempted to show, are closely associated. The 1962 publication of SSR, before such qualifications were made, was highly conservative in significant ways with regard to normal science. Indeed it was this conservatism in Kuhn’s thought concerning decision-making in normal science that concerned both Popper and Lakatos almost as much as did his radicalism about scientific revolutions. Kuhn’s portrayal of normal science, both men say, is too closely akin to the logical empiricist picture, which presents science as a kind of "closed society" with little incentive towards change of aims and methods, so long as anomalies have not triggered an era of paradigm conflict. Kuhn was correct to draw attention to an important difference between contexts in which it is respectively appropriate and inappropriate for practicing scientists to engage in

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41The positivist-influenced theses of "Post-positivist relativisms" which Laudan focuses on are discussed under five sections: 1) Translation and the Linguistic Turn; 2) The Thesis of Meta-Epistemological Subjectivity; 3) The Mystique of the Algorithm; 4) The Underdetermination Thesis; and 5) The Cumulativity/Progress Thesis. My critique of contingent historicism leveled above agrees with and expands on this. Laudan writes, "Kuhn’s relativism about methodological matters emerges directly out of his acceptance of a sharply drawn fact/value distinction, coupled with a conviction that value differences are not adjudicable by facts. But this combination is perfectly standard positivist fare. If we find Kuhn’s relativism about rival methods of inquiry objectionable...it is important to see that putting it right will require the repudiation of much of the meta-philosophical apparatus which has been entrenched in positivism, and in analytic philosophy generally, since the 1920's" (p. 21). Compare Holcomb’s radically different account of Kuhn as an defending the objectivity of values. The Laudan/Holcomb readings of Kuhn on this matter appear equally suspect, since I view Kuhn as confused about value judgments and influenced by received non-cognitivist views, but also as searching for and giving some indications of an alternative cognitivist construal of them.
discussion of "fundamental" issues and "philosophical" problems. But he goes too far in suggesting that these discussions take place just in "immature sciences," the implication being that when sciences mature to a certain point, discussion of conceptual and meta-methodological issues is largely left behind. A more important aspect of Kuhn's conservatism still is his disparate presentation of the criteria for decision-making in the two contexts he calls "normal" and "revolutionary" science --i.e., that presentation implicit in his contrastive terminology.\footnote{\footnote{Popper and Lakatos argue that competition and revision are not only a healthy state but a normal state for science generally. However, this might be interpreted as a wilful misunderstanding of Kuhn. For Lakatos history of science finds very few of the cases of communal consensus and agreement on fundamentals that characterize Kuhn's normal science. "The history of science has been and should be a history of competing research programmes (or, if you wish, 'paradigms'), but it has not been and must not become a succession of periods of normal science: the sooner competition starts, the better for progress." "One must never allow a research tradition to become a Weltanschauung, or a sort of scientific rigor, setting itself up.... 'Normal science' is nothing but a research program that has achieved a monopoly." He disagrees with Kuhn by holding that history provides very few and short lived examples of the stability of consensus as in Kuhnian normal science. Lakatos, pgs. 68-9: "'Theoretical pluralism' is better than 'theoretical monism': on this point Popper and Feyerabend are right and Kuhn is wrong."}}

Before moving on, one example should be cited to show how the contrast of intra and inter-paradigmatic discourse, and hence of internal and external contexts of decision making, remains a core issue in contemporary versions of cognitive relativism and anti-normativist constructivism. This is particularly clear for instance in the radical constructivist position Andrew Pickering takes in criticism of Paul Roth's essay in \textit{Deconstructing Quarks: Rethinking Sociological Constructions of Science}, edited by Paul Roth and R. B. Barrett (1991). Pickering's views draw heavily on the underdetermination and incommensurability theses that Steve Fuller and Roth both agree have been asked to bear too much philosophical weight by the radical constructivists and others. Pickering writes,
Drawing upon considerations of the underdetermination of theory by evidence, the theory-ladenness of observation and incommensurability, [certain] authors all contrived to suggest that, in one way or another, the search for a logic that could satisfactorily explicate important instances of theory-choice was futile. I entirely agree with this conclusion, but the best known way of putting the argument is deceptive. Kuhn and Feyerabend, especially, argued for a kind of ‘frame relativism’. That is, they located the problem of ‘logic’ in the incommensurability of big, unitary entities — Kuhn spoke of paradigms, Feyerabend simply of theories. And thus they effectively split the problematic of the philosophy of science-as-knowledge into two. One problem became that of trying to find a logic that could bridge the ‘rationality gap’ that Kuhn and Feyerabend had opened up, and thus to salvage the objectivity of science tout court.

...The other, lesser, problematic circled around that of articulating an image of objectivity-within-a-frame. Bayesianism and cognitivism addressed themselves, I think, to this lesser problematic.... The overall objectivity of science has been put on the shelf in the possibly temporary concession that scientific knowledge is relative to some frame, but the hope is still to rescue objectivity-within-a-frame. The point I want to make in this connection is a simple one. There is no reason in the world to think that the line can be held at objectivity-within-a-frame.

Pickering’s statement reflects many of the same concerns I have addressed; yet it is interesting how strongly Pickering and others continue to lean on the underdetermination thesis, and how this undergirds the cognitive relativism and anti-normativism Pickering explicitly urges be taken as the philosophical consequences of these failures of objectivity tout court and objectivity-within-a-frame. This, I would urge, is quite a different set of conclusions from that which one reaches when one challenges more directly the viability of framework models of discourse themselves, including of course framework terms like "historical period" and "culture." Both the correctness of some of the sociologist's criticisms of objectivism, as well as the mistakeness of some of their proposed alternatives (explanational symmetry; social constructivist; finitist account of meaning) will concern us further when we consider the exchange between Laudan and the Strong Programme in Chapter Five. Our attention now, however, must shift to the development of objective historicism and its own inheritance from the logical empiricist's dualistic framework of explanations.
4.4 Lakatos' objectivism and Popper's 'third-world'

The development of an objectivist account of norm governance along historicist lines was hampered by the fact that history, and historical inquiry, did not originally appear to most logical empiricists as accommodating "covering laws" of even a partial kind. While the idea of a "rational reconstruction" of science goes back at least as far as Reichenbach's 1938 *Experience and Prediction*, these and other normative interpretations were not typically seen as a proper task of the historian of science. Indeed as Radnitzky has pointed out, the kind of *historical* study of science that was commonly deemed complementary to the formal analysis of its logical structure, was one that concerned itself with social psychology and other merely descriptive (in contrast to explanatory or subsumptive) tasks. For in order to be explanatory, universal empirical laws were said to be needed, and most logical empiricists thought these could not be supplied for history. Thus, although Hempel and a few others had been interested in more fully integrating history as an explanatory or deductive-nomological science, it took some time for objectivist philosophers of science, shaken by the self-destruction of logicism, to adapt the idea of a history of science capable of serving as test of alternative research programmes.

Lakatos was among those who first responded to the structural inability of the received view to take account of history of science. The metascience of logical empiricism, he protested, made confirmation processes rest on logic and empirical testing, and did not attempt to garner support inductively from past success of methods in actual practice. The historical turn for Lakatos implied that meta-methodology must now be a "three-cornered" project, not a two-cornered one; a history of successful problem-solving provides inductive

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support for a "research programme." The unique connection Lakatos forges between normativity and objectivity recalls Hempel's dispute with Dray of a decade earlier, and Hempel's position that an attribution of *rational disposition* to individual historical agents is necessary in order to get any "critical normative" model of rationality off the ground. For Lakatos too, "It is the use of such a premise in explanatory schemes describing scientific change that I called 'rational reconstruction of the history of science'." For Lakatos, 'History is a test of its rational reconstructions.' His objectivism about the *MSRP* is seen in the claim "that methodologies may be criticized without any direct reference to any epistemology (or even logical) theory...."44 The objective historicist thesis underlying Lakatos' later philosophy may be stated as the claim that historiographical reconstructions of "internal history of science" provide an objective data-base and method for choosing between rival research programmes, and determining what constitutes theoretical progress.

I say this of Lakatos' "later" philosophy, because it is not clear whether his early work was meant to be contradictory or merely *complementary* to the logicist school of meta-methodology. Even in later years, after harnessing historical evidence to support normative meta-methodology, Lakatos' conception of the *content* of scientific knowledge was in important ways quite close to that of the logicists, although his objectivism about scientific norms was supported along very different lines. Science and objective knowledge are still identified by Lakatos just as in positivism. Furthermore, the "scientific value" of a theory "depends only on the objective support these conjectures have in fact."45 This notion of objective confirmation extended to rules of appraisal and to judgments of progressive

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45 Lakatos (1978), p. 1; See Musgrave and Radnitzky articles for support. Note that Lakatos really is an inductivist more than a falsificationist, and hence that the discussion of "confirmation" processes rather than falsification processes is the correct idiom.
problem-shifts, the objectivity of which "is not dependent in the slightest on the scientist's beliefs, personalities or authority" (1978, p. 118). This indicates that the objectivity present in theory selection is mirrored at the level of a broadened unit of selection that all historicists insisted be given prominence. For Lakatos this unit is the "scientific research programme."

He supported the idea that hypothesis confirmation is an issue of logic and evidence alone, but extended the conception of evidencing methods for research programmes by marrying history to philosophy, or perhaps better, harnessing history of science to the project Reichenbach described as "rational reconstruction" of science.

Like Popper and contra Kuhn, Lakatos sought both to insulate appraisal methods from psychological considerations, and to delimit severely the degree to which individual or idiosyncratic judgment affected meta-methodology. All of this of course, except for the new empirical foundations sought for meta-methodology, is very much in line with positivism. Lakatos' thought however underwent constant liberalization through the course of his career, as he came to qualify this epistemology/psychology divide, and to recognize the limitations of individual rationality theory as the arbiter of normative authority. Lakatos' later philosophy would even seem to contradict his early philosophy in serious ways: his prohibition against "ad hoc" reasoning and his demands for "severe tests" and "content-increasing" revisions of theory, would be unwarranted were the personal biography of real scientists not epistemically relevant.

The same appears to be an equally viable objection against Popper. Even in its mature stages, Lakatos' meta-methodology, not unlike Popper's, remains in precarious balance. His empiricist leanings remain ambiguously related to his own apriorism which, as I argue below, is evidenced particularly in 1) his account of the autonomy of 'internal history of science',
and 2) the explanational dualism or methodological nomotheticism that attends his conception of historiographical inquiry into ‘internal’ and ‘external’ history.

Now the notion of the ‘autonomy of history’ has meant many things to many people, although both Dray and Kuhn (1977) adhere to it, and Lakatos adheres to a selective version of it for "internal" history of science. There is little room for a constructive aspect in Lakatos’ account of meta-methodology, since judgments of progressive problem-shifts and research programmes are explicitly characterized as a discovery of the Popperian "third-world." Lakatos shows his deep debts to his mentor Popper in his emphasis on the demarcation problem, and in the assertion that the philosopher of science ‘calls on third-world premise(s)’ (p. 191) in any demarcation criterion! As long as historians think there is objective progress, Lakatos asserts, they must always invoke a definition of demarcational criteria in the heart ("hard core") of their philosophical systems (192). Rational reconstructive historiography of ‘internal science’ is identified with "objective appraisal" (189); demarcation at one point is identified with "a universal definition of progress" (p. 178).

Lakatos even describes a demarcation of internal and external history as a division between "autonomy" (objectivity/third world) and "individual minds" (110). But the inference from well-confirmed persuasive definitions of science (and a confidence in the public character of evidence), to the discovery of a Popperian ‘third-world’ or "knowledge without a knowing subject,” appears to me ungrounded.

One of the differences between Popper and Lakatos was that while Popper embraced conventionalism about rules of appraisal as well as advice, (or else only inconsistently maintained a difference here), Lakatos quite openly tried to avoid what he recognized as the relativism implicit in Popper’s view. His response was to attempt to disconnect rules of appraisal from heuristic advice, and to insist that the former are epistemological in some non-
cognitive sense, so that only the latter must be covered by a Popperian-style conventionalism. The insulation of epistemological concerns was maintained by Lakatos in the same way as the positivists, by appeal to the distinction between discovery and justification. This response I think was ill-conceived, because it leans heavily on positivist doctrine. Positivists had always treated these issues as subject to the justification/discovery division: while psychological issues do play a role in the methods by which hypotheses are originated, "epistemology" is interested only in their justification or appraisal. Lakatos' account also leans on this justification/discovery divide, which he attempts to make equivalent to epistemological and non-epistemological concerns. It was this that allowed him for many years to hold onto a qualified logicist position with regard to rules of appraisal. Epistemology declares itself to be concerned only with justification, so that whatever psychological aspects go into discovery techniques can all be counted on to come out in the wash! If the lessons of the historical turn and of resituating science in a broader class of normative practices are worth anything, this apriorist guarantee of the systematic filtering of social factors from the content of scientific knowledge cannot be maintained.

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46Lakatos' dismissal of psychological considerations of background knowledge from epistemology was a primary reason why he originally distinguished so sharply between theory appraisal and methodological advice. Appraisal is part of epistemology, while heuristics is not; psychological considerations play a part only in the form heuristic advice takes, not on the content of scientific knowledge. One of the advantages of blurring the distinction between discovery and justification is the incentive this provides for recognizing the coherence between epistemic concerns and heuristic advice. Methodology then is seen in constant interaction with both theory and axiology, and not set off as a 'conventional' concern completely separate from epistemology. Heuristic advice will be discussed in Chapter Six. As Lakatos gave up on his view that individual rationality theory could supply criteria for one unique choice of research programme, these issues of the importance of background knowledge and individual psychology were taken to be even more important than previously recognized.

47Though also an 'insulationist' of sorts, J. R. Brown criticizes this dependence on the discovery/justification distinction as providing an objective 'filtering' of social factors from the content of science, which goes back as far at least to Reichenbach's (1938) *Experience and Prediction*. Brown writes, "Now all of this was well and good in the days when theory choice was thought to be absolute. But things have changed...Theory choice is comparative, with the consequence that justification, even when it is working perfectly, is not the perfect filter. The reason is
Research programmes according to Lakatos serve as historiographical or meta-historical texts, and so can be criticized by criticizing their normative reconstructions of history of science. "History may be seen as a 'test' of its rational reconstructions" (123), because history of science is always an interpretive or normative enterprise (121). Since all such research programmes are normatively posited, they function by applying persuasive definitions to "internal" history. This they may do implicitly or explicitly; but when a conscious effort is made to render these normative commitments explicit, the problem of appraising rival logics or rational reconstructions of science acquires paramount importance (p. 121).

...Philosophy of science provides normative methodologies in terms of which the historian reconstructs 'internal history' and thereby provides a rational explanation of the growth of objective knowledge; (b) two competing methodologies can be evaluated with the help of (normatively interpreted) history; (c) any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) 'external history'.

In the following two sections I attempt to do a reconstruction of my own, a reconstruction of the influence which positivism had on Lakatos' dichotomy between internal and external history of science. Lakatos, as my discussion will attempt to show, systematically distorts the character and understanding of normative discourse through a misguided attempt to "separate sharply the descriptive problem of the psychologico-historical role of metaphysics from the normative problem of how to distinguish progressive from degenerating research programmes" (p. 96).

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very simple. *Any social or psychological factor which is systematically present in the comparison group will be systematically overlooked in the process of rank-ordering.* (1988), p. 156.


158
4.5 Lakatos' historiography and the demarcation problem

A central focus for my critique of the "methodology of scientific research programmes" (MSRP) is the excessive weight Lakatos placed on the "demarcation problem," and the justificatory role he expected demarcation criteria to play. It is here that we uncover the deep connection between objective historicism and the internal/external distinction so vital to Carnap and the logical empiricists. For Lakatos, the units of evaluation were the "problem-shifts" and the "research programmes"; evaluation of these was to be accomplished on the basis of objective judgments between "progressive" and "degenerating" shifts/programs!

Two premises of objective historicism with which I agree are 1) that values do enter our historiography, and 2) that (for at least some explanatory purposes) we ought to make them explicit. Lakatos is concerned with how we can make research programmes 'hard-edged', so that they can be more readily testable and compared as epistemic competitors. This concern to empiricize questions previously addressed only through aprioristic assumptions represents a most important advancement beyond logicism.

But the manner in which Lakatos brings historiography to bear on the MSRP is quite objectionable. Lakatos began a process of empiricizing the testing of normative meta-methodologies, yet ironically what he thought he had justified in this way was not merely the comparative advantages of one programme over another, but the possibility of "stating universal conditions under which a theory is scientific" (p. 168). He assumes indeed that a demarcation in the content or products of science must be the inner "hard core" of each meta-methodology. Each methodology of science determines a characteristic (and sharp)

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49 Lakatos' own notion of 'replacing in a positive way' would seem to narrow the range of appropriate ways to revise a theory. But Lakatos' holism is much stronger at the meta-methodological level, where he holds to the dogma of unassailable "hard cores" of research programmes. Interestingly, Lakatos held a quite dogmatic version of holism at this level, and unsuccessfully tried to present it as compatible with objectivism concerning the rules
The demarcation between (primary) internal history and (secondary) external history" (p. 138).

"The vital distinction between normative-internal and empirical-external is different for each methodology" (p. 102).

Lakatos says (p. 94) that his normative delineation of progressive and degenerating problem-shifts is based on and "almost identical with" Popper's demarcation criterion! This concedes that the problem of demarcation plays the central role in the evaluation of competing meta-methodologies. The demarcation problem is conceived as the system of appraisal itself, rather than as a problem the importance of which may itself be questioned by less positivistically-influenced research programmes (p. 176).

To review, Lakatos' objective historicism made the identification of the superiority of a research programme over its competitors dependent upon a kind of persuasive definition of the defining characteristics of internal history of science; these definitions, in turn, are conceived as intimately connected to the generalized "demarcation" problem. Now, the normative definitions required by Lakatos are sometimes identified as concerning "science" itself, while at other times they concern "progress." Most often for objective historicists like Lakatos, however, the concept of "rationality" bears the weight of the normative concept to which demarcation criteria applies. The possibility of progress and the presence of a "scientific" character are made dependent on the presence of a quality of rationality in decision procedures. Indeed the distinction between internal and external history of science,
so crucial to Lakatos' account of meta-methodology, functions through the demarcation of rational from non-rational episodes in the history of science. Thus, Lakatos asserts,

The generalized demarcation problem is closely linked with the problem of the rationality of science. Its solution ought to give us guidance as to when the acceptance of a scientific theory is rational or irrational.\textsuperscript{50}

By contrast, I argue, the justificatory role that the demarcation of "rational" decision-making is here taken to perform is closely linked with the extension of what I have called "the internal/external framework myth," or in my more technical sense, "methodological nomotheticism," from positivism into objective historicism. That the internal history of science is "autonomous" is an assumption underlying this sharp division between rational and non-rational historical episodes. Lakatos' statement clearly exemplifies how, even after the division of labor between logic and psychology came to be questioned and qualified or supplemented in various ways, the internal/external distinction remained crucially important for the self-image of science as an objective, knowledge-producing practice. Lakatos attempted to reinstate a version of the positivist's contrast between the logic and the psychology (or sociology) of science, by making this distinction co-extensive with what he presented as the difference between "internal" and "external" history of science. Lakatos' new way of separating normative and descriptive problems, then, was to focus on the difference between intellectual and social histories of science. But this in many ways was only to rephrase and not to reject the notion of objectivity as insulation from social influences;

\textsuperscript{50} 1978, p. 169.
it hinges upon Lakatos' attempt to characterize internal history as 'autonomous', and to make it the exclusive focus of rational reconstructive historiography.\textsuperscript{51}

Borrowing the form of Lakatos' statement, I can phrase my response as follows: The identification of the objective superiority of a research programme with the problem of demarcating rationality from non-rationality, is closely linked with methodological nomotheticism; the explosion of this connection (as represented in the importance attached to the distinction between internal and external history, normative reason and social cause, etc.), ought to give us guidance towards an altogether more adequate conception of explanation and understanding as it pertains to historiographical methods, and to a philosophy of norm governance.

According to Lakatos, "internal history is self-sufficient for the presentation of the history of 'disembodied science,' including degenerating problem-shifts. External history explains why some people have false beliefs about scientific progress, and how their scientific activity may be influenced by such beliefs" (117). Lecturing on the Copernican revolution, he asserted that "The whole development is narrowly internal... External history in this case is not only secondary; it is nearly redundant." But is this ever the case with social history of cognitive norms?

Lakatos characterized the peculiar connection he forges between normative meta-methodology and demarcation criteria by saying he has "relativized...the internal/external distinction to methodology."\textsuperscript{52} But this constitutes another version of methodological

\textsuperscript{51} "In view of the autonomy of internal (but not of external) history, external history is irrelevant for the understanding of science." -I. Lakatos (1978), p. 102.

\textsuperscript{52}Lakatos, p. 190. Part of the reason for this 'relativization' of the distinction is legitimate. Lakatos wants to disentangle questions of "appraisal" from questions about whether a theory is "believed". This is a progressive move over Carnap's version of the internal/external framework myth, to the extent that it shows that issues of appraisal and acceptance are normative and contextual issues distinct from issues of what doxastic states epistemic
nomotheticism, in the sense that it presupposes a dualism in the content of beliefs (or in the causes of belief), and an insistence that each of the two realms of content be explained in a methodologically distinct fashion. Lakatos sought covering laws, all right, but two distinct kinds for two distinct classes of content: psychological for external history, and rational for internal history. Here is the source of the schism between "good reasons" explanations and "social causes," which many recent sociologists have found so objectionable. This claim that external history is 'redundant' in the Copernican case is underlaid by a version of what I will call the "autonomy thesis": "In view of the autonomy of internal (but not of external) history, external history is irrelevant for the understanding of science" (p. 192). This is among the clearest examples of what I charge is Lakatos' own version of the internal\external framework myth, or, in my more technical sense, of his methodological nomotheticism.

4.6 Intuitionism and the ‘scientific elite’

We can further explore Lakatos' methodological nomotheticism through his understanding of "the autonomy of internal history of science." Lakatos says that ."...Progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational.... When a better rationality theory is produced, internal history may expand and reclaim ground from external history" (p. 131). Lakatos did recognize limits to this internalization: the ideal could not be to render all past incidents in the history of science narrowly "internal," since even our 'best' agents actually are in. But this distinction I accept between normative and doxastic issues is not identical to Lakatos' division between epistemology and psychology.
social scientific theories would surely inform us of some degree of irrationality to be expected in any social practice.

Lakatos' basic requirements for preference among research programmes are summarized by H. Sarkar (1985) as:

1) Squaring with the basic value judgments of the scientific elite.
2) Reclaiming more of external history of science as internal history.
3) Predicting scientists' basic value judgments.
4) Leading to revision of scientists' value judgments.

I pause now to briefly discuss four preliminary objections to the MSRP. This preliminary round of objections against Lakatos will put us in a position to discuss, in the next chapter, what I take to be the deepest generic problems with objective historicist approaches to meta-methodology.

Four problems often observed with Lakatos' meta-methodology are that (A) it depends upon judgments of progressive and degenerating problem-shifts by a scientific "elite" (Sarkar, 1985; McMullin, 1982; Laudan 1986); (B) it depends on shared intuitions among the elite, such as those about rational cases of change/decision in the history of science (Laudan 1986); (C) it disconnects heuristic advice from epistemology, leaving advice a mystical affair (McMullin, 1982; Laudan, 1986); and (D) it depends on a problematic version of holism, leaving the warrant of research programmes no less implicitly relativistic than the contingent historicism it claims to replace (Laudan 1986).

The first two objections move to censure the bias of Lakatos' objectivism and intuitionism. These objections are sometimes advanced by contemporary objective historicists as well as contingent historicists. Laudan and H. Sarkar, for instance, both criticize Lakatos'
elitism. Sarkar allows a limited role for intuitions in meta-methodology, while Laudan (since 1986) claims to reject intuitionism generally in favor of a more thoroughgoing naturalism. J. R. Brown (1988) rejects naturalism and is far closer to Lakatos' original position than either or the other two; his account relies heavily on an "elitist" version of intuitionism.

There is of course an important difference between 'native' and 'cultured' intuitions. Empirically or a posteriori-oriented meta-methodologies typically allow cultured intuitions as a crucially important part of a bootstrapping account of support. But cultured intuitions in this sense are just normative posits and have no special or foundational status. It is native intuitions that the so-called intuitionist schools have always leaned upon, precisely because they are candidates for the objective foundations for meta-methodology they sought. Popper's critical rationalism and Lakatos' objective historicism are certainly a part of the 'received view' and present no real alternative on this issue.

The latter two objections, having to do with Lakatos' conventionalism about methodologica, advice and his holism, might also be advanced from both branches of historicism, though they are viewed in quite different fashion by the proponents of the two branches. Contingent historicists typically affirm methodological holism, but focus on the inconsistency of this in Lakatos' objectivism, and insist that his strong holism should be seen as undermining objectivism. Recent objective historicists (Laudan 1986) generally recognize an implicit relativism in this doctrine when applied to theory or research tradition, and not restricted to the historian's focus, as Kuhn in The Essential Tension appears to restrict it (1977, p. 20). They thus argue that Lakatos' account faces many of the same problems that,

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53 It might be argued however that Laudan's requirement that "research traditions" 'save the canon' is still elitist, although it is clearly a much more qualified thesis.
Lakatos charged, beset Kuhn and Feyerabend (Laudan, 1986); they maintain the importance of rejecting both strong holism and strong conventionalism about either methods, while retaining the objective character of rational reconstructions of internal history of science.

I have already discussed holism above, agreeing with no more than a weak version of it. Hence I forego further discussion of these topics here and will offer comment only on how Lakatos' elitism connects with a longer and more deeply entrenched "intuitionist" tradition. Contingent historicists have effectively argued that Lakatos' meta-methodology is "elitist," and Sarkar and Laudan have supported this criticism themselves. Sarkar for instance argues that thesis (4) above, that a meta-methodology should spur improvement of basic value judgments by working scientists, is quite at odds with Lakatos' elitist requirement (1). While (4) is an indispensable requirement of a normative research programme, it is complicated if not rendered impossible in its pursuit by the demand for squaring with value-judgments presently held by the scientific 'elite' (whether made on an explicit basis or considered a "tacit" skill). The demand that a tradition preserve current value judgments is incompatible with the desire to have scientists change their value judgments.

Lakatos' elitism is amply demonstrated by his commitment to a high degree of "false consciousness" among scientists. This is a commitment that is influenced by Lakatos' neo-Marxism; in some circumstances it sets up what Q. Skinner has called "the mythology of coherence," by ignoring the possible incoherence or irrationality of historical agents. At its worst, this becomes the methodological basis for a thoroughly anachronistic reading of history of science, resulting in explaining away rather than explaining what is not well understood in

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54Laudan (1986) defines meta-methodological intuitionism as "the view that we or scientists usually make reliable and trustworthy judgments about methodological matters (such judgments make up our shared 'intuitions'), but that our explicit theories about such matters are not usually so reliable, presumably because we have yet to develop a methodological theory which does justice to our presumably sound intuitions."
the historical record. Historical evidence showing that agents ascribe reasons for their own action different than what is ascribed to them in a 'rational reconstruction,' is interpreted by Lakatos not as a possible fault of the reconstruction, but as case-evidence for a high degree of self-confusion or "false-consciousness" among historic scientific actors. The presumption is said to be merely that scientists are for the most part rational, and are good at making objective decisions. Yet the justificatory work these underlying ‘real reasons’ for decisions are supposed to serve in objectivist theory far outstep this presumption.

In a related fashion, Lakatos sometimes says his notion of an internal explanation has the force of, ‘in keeping with my best-confirmed meta-methodology’. Lakatos says that internal reason, because it represents our best-confirmed theories (including social scientific ones) defines the external problems which sociologists of science address. "The internal skeleton of rational history defines the external problems." But this becomes a quite elitist and self-serving notion for methodologists, since ‘the best’ social scientific theories which Lakatos (and Brown, 1989) want to allow do play some role in informing epistemology, are implicitly judged to be only those that accept the "external problems," and conform to the internal/external history division, that Lakatos claims is supplied by normative philosophy of science. In other words, Lakatos’ elitism introduces a deep circularity into MSRP, because the "most important" problems will be those for which present methods are successful, and the "best theories" will be those that are picked out by value-judgments presently held. Both results are highly conservative, and do not allow for a great deal of novelty.

Let me note that my criticism of elitism is not identical to Laudan’s. Lakatos seems to have intended some concession to what Polanyi called a "tacit dimension" to scientific

55The history of science is always richer than its rational reconstruction. But rational reconstruction or internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history." Lakatos (1978), p. 118. See also p. 191.
knowledge, and this I think we can view in a positive light. If we take the 'elite' to represent merely those who have received training and education in a professional field or practice, the skills and knowledge which are reflected in their judgments may not be fully articulated or articulable. Like Polanyi I do not think this lack of articulation renders any meta-methodology that recognizes a 'tacit dimension' to scientific methods necessarily 'obscurantist' or irrationalist.\textsuperscript{56}

It is important to focus on the justificatory role that Lakatos' intuitionism plays in supporting his version of objectivism, and in particular the role it plays in support of the distinction between internal and external history of science. \textit{What the intuitionistic character of Lakatos' meta-methodology shows is that the intuitions of the scientific 'elite' play the role of an ungrounded ground for the demarcation of internal history.} Clearly, the judgments of the scientific 'elite' upon which Lakatos' meta-methodology depend are a type of intuition upon which are conferred a privileged status. Laudan, in "Some Problems Facing Intuitionist Meta-methodologies"\textsuperscript{57} has acknowledged and repudiated his own previous dependence on shared intuitions about rational segments in the history of science, intuitions that underlaid his approach to the testing of research traditions in his (1977) \textit{Progress and its Problems}. Laudan notes that intuitionism was strongly a part of positivist meta-methodology, and was adopted by

\textsuperscript{56}Intuitions I hold are an essential part of meta-methodology, but what is important is that the intuitions we recognize as undeniably present be 'cultured' intuitions and that we treat them in a fallibilistic fashion. This still supports the general a posteriori character of meta-methodology, and contrasts with the native intuitions treated as unrevisable foundations of meta-methodology in early Laudan (1977) and in Brown (1989). It is this latter approach which relies on shared native intuitions as foundations of meta-methodology that is rightly labeled 'intuitionism' and rejected, as Laudan himself now does.

\textsuperscript{57}Laudan (1986) p. 115-129. In allowing and extending Garber's (1985) criticism of him, Laudan writes, "In short, Laudan (1977) proposed that we could -indeed that we should- test proposed methodologies of science by demanding minimally that they "capture" as rational certain specific choices in the history of science which (as Laudan then believed) anyone who studied them would see intuitively to be instances of rational theory choice. These archetypal instances of rationality, rather than the whole of the history of science, were to provide Laudan's "data base" for evaluating rival methodologies."
himself and others influenced by the historical turn, in order to avoid the conventionalism about rules of appraisal that we have seen flowing out from logical empiricism. He writes, "I can certainly report that my own reason for treating the intuitions as unrevisable was that they seemed to be the 'bedrock', the justificatory foundation on which everything else had to be built if we were to avoid rank conventionalism about methods" (122). He now rejects as wrongheaded the assumption that a meta-methodology must reconstruct the acknowledgedly best gambits as 'scientific', and indeed even the requirement "that a methodology or epistemology must exhibit past science as rational."58

Conclusions

Both Popper and Kuhn took decisive steps in the direction of a more pragmatic conception of metascience or meta-epistemological discourse. Popper's critical turn is such a step, as is Kuhn's understanding of methodological criteria (even in normal science) as "values at work" in contrast to rules 'algorithmically' applied. But it is equally important to see that both men were limited in the extent of their revisions of logical empiricism in the early 1960s --limited, indeed, by their own inheritances from this tradition.

If Kuhn remained in some ways strapped by the internal/external framework division coming down from Carnap and the late logical empiricist era, this same inheritance is witnessed in Lakatos' attempt to make 'internal history of science' autonomous from 'external history of science,' and to render the former, insulated in this way, the focus of rational reconstructive historiography. This project associated Lakatos with an untenable meta-methodological intuitionism, and with views about the "demarcation problem" that most latter

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58L. Laudan (1987), p. 20. I will later however have something to say about whether Laudan's recent "normative naturalist" approach to meta-methodology escapes the grip of intuitionism as he believes it does.
historicists, including most later objective historicists, rightly reject. We are only beginning at this point to investigate the influence of methodological nomotheticism on the historiographical methodology advocated by objective historicists; so my fuller conclusions concerning this influence must await the discussion of more recent versions of this general approach in the next chapter.
CHAPTER FIVE

RECENT OBJECTIVE HISTORICISM

Introduction

Our examination of 20th-century objectivism has thus far uncovered a conceptual shift in the manner in which factors internal and external to an intellectual system were demarcated. To summarize briefly, our study of this issue began with Carnap's classic division between a 'logic of science' and a 'science of science', a division that had been widely accepted throughout the positivist era. The traditional way to mark this division was to treat logic (or science qua body of knowledge) as autonomous and independent of psychology (or science qua body of action). LE epistemology insisted that the subject matter or content of the logic of science could be abstracted "from the persons asserting the statements and from the psychological and sociological conditions of such assertions" (Carnap, 1938). This distinction provided demarcation criteria and framed methodological issues regarding disparate modes of explanation to be applied to scientific and non-scientific discourse.

In the last chapter we examined how the Reichenbachian dogma that 'epistemology is interested in internal relations only' came to be imported into Lakatos' development of his historiography, with its focus on the "internal history of science," as well as into the relationship he conceived to obtain between philosophical and psycho-sociological inquiry.
Focusing on the explanatory account of "good reasons" or rationalizing explanations that accompanies Lakatos' preferred historiographical project, we found that it inherited much from the tradition of Carnap and Reichenbach. The division between internal and external history divides the content of scientific history into two parts, just as surely as had the cognitive\non-cognitive and internal\external question distinctions that preceded it. Lakatos' conception of internalist historiography and rational reconstruction implies a dualistic explanatory scheme for these two realms of content as surely had Carnap's treatment of internal and external questions. In brief, the attitudinal disparity that attended what I call the 'insulationist' conception of epistemology is just as important in Lakatos' objective historicism as it had been in the objectivist tradition of logical empiricism. This attitudinal disparity as it affects the conceptions of explanation and historiographical method, is co-extensive with the influence the group of assumptions I have called methodological nomotheticism.

These conclusions, however, should not blind us to certain advancements that Lakatos' historicism represents. We can see Lakatos as initiating an actual shift in paradigmatic views of the demarcation problem, insofar as he shifts emphasis from the difference between logic

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1 What I have called epistemic insulationism and related to methodological nomotheticism is closely related to what S. Fuller has recently focussed his criticism upon from the perspective of his "social epistemology." "...a major source of today's confusion is the illusory character of philosophy of science's object of study, namely, the internal history of science. ...I suspect that a symptom of our failure to come to grips with the Continental European philosophical tradition is the unsatisfactory treatments of facts and values, 'naturalism' and 'normativism' in scientific methodology" (1990, p. 2-3). My utilization of Fuller's criticisms to supplement my own will be of particular significance in this chapter. Fuller views the philosophers' focus of the internal history of science as an "imaginary locus of inquiry" that is the result of the abstractness of twentieth century epistemology from social practices. "Its main historiographical assumption is that there is a natural trajectory to the development of science when it is regarded as a knowledge producing activity" (ibid).

172
and psychology, to a difference between intellectual and social histories of science. But my examination has shown the important doctrinal commonalities that Lakatos' historicist version of objectivism shares with logicism; that is to say, it has worked to reveal methodological nomotheticism as a root, underlying assumption of twentieth-century objectivism, one which is rarely studied explicitly by historians or critics of this tradition. This in turn leads us to further consider what philosophical basis there is for this pervasive commitment. The present chapter will examine this issue, as it drives current debate, through an examination of the most contemporary versions of objective historicism.

Like Lakatos, J. R. Brown and Larry Laudan continue to base the superiority of research programmes (or more correctly for Laudan, "traditions") on reconstructive demarcations between internal and external factors in the development of knowledge. For these objective historicists, the concepts of "rationality" and "irrationality" are among the ones that demarcate factors internal and external to the intellectual system of science. For them as well as for the early Lakatos, this takes the form of tying the explanatory and prescriptive tasks of a meta-methodology to a tradition of historiographical inquiry wedded to a peculiar philosophical

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2 I attempt here to align my analysis with the topology of responses which S. Lelas (1985) admirably developed. He contrasts three main contexts for the demarcation of internal and external factors in the development of knowledge: 1) Logic of science versus science of science (Carnap and positivism); 2) Intellectual versus social histories of science, including those asserting versions of what he calls the 'asymmetry principle' (Lakatos/Laudan/Brown would be prime examples); and 3) Rationality versus relativism (Hollis and Lukes, ed., contributors on both sides of the objectivism/relativism issue). My final chapter might be taken as an attempt to outline a fourth response, 4) Functionality and disfunctionality, including the various functions that different types of functions served by human interests. One cannot "classify" the agent-interests in the way that the first two paradigms presume, and one cannot classify interests as rational or relative, as the second presumes. The sociological turn in thought establishes at least that knowledge is not developed under conditions that make feasible a sharp distinction between epistemic and pragmatic interests. But one can clarify the different kinds of functions (epistemic, economic, aesthetic, political, etc. that a set or overlapping sets of interests serve. In my final chapter I do maintain the viability of distinguishing between epistemic and societal functions the multiple interests of scientists serve; but I do not presume that one can ascribe particular interests to actual agents that come pre-defined as internal, cognitive, etc. Interests in this latter sense can hardly even be individuated, let alone ascribed to actual agents.
interpretation of "good reasons" explanations. The transition from Lakatos' to more contemporary versions of objective historicism, I will argue in this chapter, has not succeeded in casting aside the philosophical bias of methodological nomotheticism. This can be seen by examining a group of fundamentally related "asymmetry" assumptions about explanation that Brown and Laudan share with Lakatos.³ In the first section, I will briefly examine J. R. Brown's non-naturalist account. Laudan's attempts to substantiate these assumptions in the context of his normative naturalism will be treated separately in subsequent sections.

5.1 The rational, the social, and the normative

The version of objectivism J. R. Brown presents in The Rational and the Social (1989) is sophisticated in the sense that it has taken some lessons of the historical turn in epistemology to heart: Brown sees theory choice as comparative and contextual, and historical case studies as evidentially (if somewhat indirectly) relevant to the criticism and defense of proposed normative meta-methodologies. The debate contrasts the "historically-oriented

³ Lelas defines the asymmetry principle underlying second (objective historicist) approach in the following way: "If individuals or groups hold false or irrational beliefs, or reject true and rational ones, then an extra explanation referring to external, non-cognitive (i.e., psychological, economic or social) factors is needed to explain this. Such an extra explanation is not needed where rational beliefs are held or irrational ones rejected. The beliefs for which an extra explanation is required can still be described as rational if one can show that, given the psychological, economic or social conditions, the belief in question serves individual or group interests; but essentially it is a history of irrationality" (Lelas, 1985, p. 68). Both Laudan and Brown, as we will see, emphasize rationality-ascriptions but reject or de-emphasize truth-ascriptions in their versions of asymmetry. The "charity principles" that Laudan's historiographical project depend upon are not of the Davidsonian sort, that refer to the "truth" of the beliefs of agents; it is their capacity or competency for making "rational" decisions/inferences that is important for Laudan. Laudan also distinguishes a history of non-cognitively rational action from irrationality.
rationalistically-minded" philosophers of science (Lakatos, Laudan and himself) with the "strong programme" (hereafter SP) in the sociology of scientific knowledge (hereafter SSK), associated primarily with David Bloor and Barry Barnes.4

Brown's version of objectivism I think fails to convince an outsider to this debate, because he fails to attach any significant sense to the possibility that the rational and the social are overlapping spheres. Rational decisions are "caused by good reasons, not by social forces." This would be extremely contentious if it was not virtually a tautology for Brown, in view of the manner in which he carries the mutual-exclusivity of the rational and the social by definition. This contrast is odd at the level of common language, but odder still on the "rationalistically-minded" basis Brown constructs. For it is just this misguided dichotomy which obscures a constructive dialectic between human inductive propensities and social context. If this is correct, then Brown still fits Bloor's characterization of the misguided rationalist who insists upon "treat(ing) our inductive propensities as if they work against our social natures, rather than through them" (1988, p. 66).

The 'reasons are causes' thesis that Brown (and Laudan) hold(s) does not itself satisfactorily deflect the SP criticism of asymmetrical schemes of explanation; for much more

4 This programmatic statement of the SP appeared in D. Bloor (1976: 5):

1. **Causality.** A proper account of science would be causal, that is, concerned with the conditions that bring about belief or states of knowledge.

2. **Impartiality.** It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.

3. **Symmetry.** It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false, (rational and irrational, successful and unsuccessful) beliefs.

4. **Reflexivity.** It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry, this is a response to the need to seek for general explanations. It is an obvious requirement of principle, otherwise sociology would be a standing refutation of its own theories.
is involved in that thesis than merely substantiating the claim that rational and non-rational beliefs have different causal origins. Brown (and Laudan) says that the SP is attacking only a straw rationalist, one who thinks that rational beliefs are 'uncaused.' But this I think represents a misreading of the SP and misses the correct critical focus of the SP critique of objectivist epistemology.\(^5\) The objective historicists treat the force of Bloor's critique as being deflected if only we see that they (objectivists) take reasons to be causal, and hold a theory of rationality that is *explanatory* in the sense of reflecting real differences in the causal origin of belief.

On my view, Bloor is correct to throw charge of constructing a 'strawman argument' charge back at the objectivists (Bloor/Laudan exchange 1981), since this type of response misses what I take to be the thrust of the SP criticism, and saddles it with a quite different position. The SP criticism falls instead on the what the SP describe as the rationalist's "self-justifying" conception of reason, or of what I will call "epistemologically-couched accounts."

*Their criticism on my reading focuses not on the causal status of reasons, but on the understanding rationalists and objective historicists have of the normative reasons they invoke for the purposes of rational reconstructions of science: that is, it focuses on the understanding of the normative posits historiographers invoke, and the warranting functions the objective historicists expect internalist historiography to perform.*

\(^5\) Laudan, I hold, succumbs to a similar misreading insofar as he says that the strong programmer attacks only a "straw" rationalist who claims that rational beliefs are "uncaused." In the (1981) exchange I will discuss between Bloor and Laudan, Bloor points out this misreading and states that his explicit target is views of rationality as self-moving or "self-explanatory." In response Laudan, interestingly, concedes that "self-explanatory" accounts of the reason involved in rational reconstructions would be viciously circular, 'no response at all,' but that Bloor's criticism of this view is also directed against a 'strawman' position. In my discussion of Laudan I will follow up on this by arguing that Lakatos, Brown and Laudan share certain rationalist-influenced assumptions about types of belief (invoked in epistemologically-couched accounts) and their privileged mode of explanation that are rightly to be called into question as a form of 'self-justifying' conception of the reason.
This is perhaps the place to point out that my intention is not to engage in the debate so much as to comment on the way it has been carried on. This is why my focus is specifically on the critical side of the SP literature, and not on the alternative monistic historiography that the SP take as the correct implication of their critique. Indeed I think that the strong program methodology for interest imputation is ultimately as untenable as the internalist historiographer’s. As I argue in Chapter Six, their own conception of explanation of normative human reasoning and decision is as reductive and misguided as that of the rationalists they take as the target of criticism. Hence the reader must beware of reading support for the strong programme into my commentary on the debate.6

We have already seen a striking example of a "self-justifying" conception in early objective historicism, in the form of Lakatos’ claim of the "autonomy" of internal reason from external history. This surely is a vestige of the disinterested contemplation model of knowledge which classical rationalism held.7 It is clear by the example of Lakatos’ autonomy principle that the SP is not attacking mere "straw" rationalists, but understandings of normative discourse still alive and kicking in mainstream epistemology in the past 20 years.

Rationalism puts into service what Brown simply calls the "best" social scientific

6My criticism will focus specifically on the "causality" thesis, and on the interpretation placed upon the "symmetry" thesis that it represents as a principled claim to methodological exclusivity or primacy of symmetrical methods for those who engage in historiographical inquiry. But "Internal history" on the view Bloor objects to is "congruence between scientific behavior and some privileged set of rules." "External history" on the view he objects to consists "of everything that scientists do that deviates from the specified rules." This is, on my view, a criticism of methodological nomotheticism, and I agree with Bloor that this kind of criticism finds a target in Brown’s and Laudan’s conceptions of historiographical explanation.

7 The problem is not with the ideal of disinterested contemplation, but with the causal explanatory program which reduces agent’s "good reasons" to merely logical and evidential relations resulting from disinterested contemplation. One should not be confused by this connection between classical rationalism and logical empiricist objectivism because, as was explained in Chapter Two, the logical empiricist usage of the analytic side of the analytic/synthetic distinction put the concept of analyticity into the service of many views about knowledge that were shared by classical rationalism.
theories. Brown privileges Mertonian methods where the content of scientific belief is concerned. Sociology concerns only non-cognitive structures such as institutions, and not the content of scientific belief. At the root of Brown's account is his endorsement of a regressive version of the "arationality principle," that principle which formalizes Mannheim's assertion that "thought has an existential [i.e. social] basis insofar as it is not immanently [i.e. rationally] determined." Brown acknowledges that this principle "is probably the very antithesis of the sentiments embodied in the recent sociological turn"; yet his introduction and unargued adoption of this principle in the first few pages of the book indicates a serious petito in his argument.

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8 Theories of the Mertonian type posit a "scientific ethos" consisting solely or primarily of cognitive interests. Brown's rationalism is an "elitist" response as surely as was Lakatos. Ron Giere, whose naturalism Brown opposes, recently commented, "There is little evidence that he (Merton) consciously thought of his work as complementary to the program of logical empiricism. But his program was complementary nonetheless.... Merton left the analysis of 'methodology' and the 'substantive findings of science' to the logical empiricists. He was providing an analysis of the 'cultural structure' of science, which as it turned out complemented their analysis of its 'logical structure.' In separating the 'substance' and 'methodology' of science from its social structure, functionalist sociology cut itself off from any explanation of the distinctive cognitive products of science...." —Ron Giere, (1989) pgs.29-32. Giere's differences from Laudan on this score have an added importance because Giere, too, is sometimes considered as a normative naturalist (but would not be an objective historiast).

9 If, as Brown trenchantly maintains, "Sociology is only for deviants," sociology and other social sciences have little constitutive or critical role to play in the normative conception of philosophy of science. Epistemology itself remains insulated from the social sciences. I must agree with the critics of Brown and Lakatos that what internal history excludes is by no means necessarily irrational or non-rational. In fact, background metaphysical beliefs and values can help show the rationality of choices by individuals that might not otherwise be apparent. Background commitments are "epistemic", as the sociologist insists, but this doesn't mean they are closed to criticism. Brown and Lakatos both claimed to make something of a critical interplay between "internal" and "external" historiography, yet their conceptions of this interplay are quite barren.

10 Mannheim held that social determination "...may be regarded as a demonstrated fact in those realms of thought in which we can show...that the process of knowing does not actually develop historically in accordance with immanent laws, that it does not follow only from the 'nature of things' or from 'pure logical possibilities,' and that it is not driven by an 'internal dialectic'...." —Mannheim as paraphrased by Bloor, 1988, p. 61.

11 I will later argue that the arationality principle basically represents a professional territorial dispute rather than a genuinely philosophical issue. To insist on it is to retreat to the same basic divisions of labor advocated by
The arationality principle as a methodological thesis depends upon a systematic method for distinguishing rational from irrational beliefs; "The arationality principle assumes as already given what rationality is." Yet Brown's definitions of rational decision, when he (later) stops to give them, are remarkably empty and impotent to perform the justificatory tasks he asks of them. Rationality is presented only as decision that has appealed in its formation to "good reasons"; whatever the normative methodologist can't explain by invoking norms of evidence and sound reasoning, etc. become candidates for sociological explanation. But these definitions have virtually no content aside from Brown's claim that they support whatever our "best theories" are; they supply no independently grounded principles for distinguishing rational and irrational beliefs, and hence no residual confirmation that the preferred theories are really the best. The definitions only lean back upon an assumed foundation of shared native intuitions of rationality. Indeed Brown is a self-avowed intuitionist in this matter, and simply fails to take account of the problems which Laudan now concedes dog that basis for objectivism.

5.2 Laudan's normative naturalism

Laudan's 1977 Progress and its Problems will be immune to some of the criticisms previously addressed to Lakatos' and Brown's versions of objective historicism. In discussing positivists; it fails to clarify the way that sociology can both complement or compete with normative reconstructive philosophy of science.

12 For instance, in one of the only places Brown bothers to define what he means by rationality, he writes, "As a card-carrying rationalist, I certainly believe that scientists can, and typically do, make rational choices. That is, they give an objective, rational ordering of any collection of available rival theories, and they accept the best from among these. These cognitive decisions are caused by good reasons, not by social forces." But he explicitly makes the identification of good reasons and their difference from "social forces" depend upon shared native intuitions.
issues of rationality, causality, and explanation as they are reflected in the objective historicist’s dispute with contemporary sociology of science, we must remain aware how Laudan marks himself off from other defenders of the arationality principle such as Mannheim, Merton, Lakatos and Brown.

Laudan’s (1977) defense of the arationality principle is somewhat less restrictive and more empirical. He concedes that many philosophers and sociologists have treated it as an ‘article of faith,’ but defends a methodological version of it himself on empirical grounds. Because he holds that conceptual factors play an important role in the formation of theories, his version does not bar cognitive sociology a priori from explaining the content of science, but only from explaining that content for which we already have "adequate" internal explanations. He disagrees with the strongest proponents of insulationist epistemology such as Merton, Richter and Mannheim, by holding that science is not self-contained and cut off from philosophy, values and religion (p. 220). There appears to be an implicit contrast here between the possibility of doing social history, and the impossibility of giving causal sociological explanations of actions or decisions of rational scientific actors. Laudan clearly states that historiography is the vehicle for causal explanation, and that historiographical method depends on use of the 'best' available theory of rationality.13

The ironic point here, however, is that Laudan’s repudiation of the intuitionism he concedes centrally entered his construction of the issues of rationality and historiographical explanation in his Progress and its Problems (1977), is quite recent (See Laudan 1985). This also complicates my attempt to reconstruct the Laudan/Bloor debate of 1981-1982 as a time

13 Although his conception of rationality has moved dramatically away from its explicit dependence on intuitionism in the 1977 work, he clearly accepts the burden of explicating the notion of rationality on which his historiographical project depends; this notion he has consistently maintained to the present is a version of rationality as efficiency maximization (1977, p 124).
slice in Laudan’s thought. Accordingly, I will proceed by commenting first on the
intuitionism imbedded in Laudan’s original formulation of historiographical explanation,
before moving to argue that the position Laudan holds in the Laudan/Bloor exchange is still
tantamount to an intuitionistic one. Finally, we will consider whether there is any better
ground for the arationality principle on the thoroughgoing anti-intuitionistic basis Laudan now
claims for rationality theory and internalist historiography.14

Laudan’s 1977 account of history and sociology appears motivated by an Hempelian D-N
model of explanation. His account gives historical and sociological study a strongly
nomological construal; indeed like Hempel himself he discusses at length how the covering-
law model of explanation is to be made applicable to both history and sociology.

Any cognitive sociological explanation must, at the very least, assert a causal
relationship between some belief, \(x\), of a thinker, \(y\), and \(y\)’s social situation, \(z\). It will
(if the explanations of sociology are in any sense ‘scientific’) do so by invoking a general
law which asserts that all (or most) believers in situation type \(z\) adopt beliefs of type \(x\).15

14 This criticism of Laudan’s overemphasis on a formalistic basis for scientific meta-methodology, and his
parallel devaluing and disparaging of “intuition” in recent years, establishes a parallel to my earlier criticism of
Hare’s formalistic turn in meta-ethics. Hare’s early intuitionism was pointed out to him in the form of “decisions
of principle” he was committed to dealing with as ultimately arbitrary. We saw that in his more recent work, he
claimed this incipient relativism was annulled by his divorce from meta-ethical intuitionism and his taking a more
thoroughgoing naturalistic meta-ethics. Laudan’s rejection of his early metascientific project based on pre-analytic
intuitions, and his shift to a normative naturalism alleging no dependence for meta-methodology upon ‘non-natural’
assumptions, shared some very interesting parallels. The comparison could also be extended to formalists in
juridical philosophy, like Rawls. On my view, these formalists have been equally unable to show that formal
epistemology and empirical evidence can provide sufficient grounds for the metascientific and meta-ethical forms
of objectivism they respectively argue for.

15Laudan (1977), p. 217; see also p. 184-9 on covering law explanations and intellectual history. Note that to
his credit, Laudan (1981) criticizes Bloor by saying that ‘causal talk’ is neither a necessary nor a sufficient
condition for the scientific status of an explanation. But it is quite beyond me how this statement can be made
consistent with his claim indicating a deductive-nomological conception of sociology quoted. If Laudan’s 1981
statement truly reflected a deep revision in his views, he should not, I would argue, have continued to gloss the
rational/non-rational belief-distinction is such straightforwardly causal terms, or the theory of rationality as
necessarily “explanatory” as he there continues to do. More on this below.
This account of social science following in the D-N model of the physical sciences, and of causal, covering-law explanations as the exclusively legitimate methods of history and social science, reflects a far stronger version of methodological naturalism than most contemporary naturalists are prepared to defend. Its historical lineage is so apparent as not to require further comment, although its connections with methodological nomotheticism are indeed interesting. For this latter connection points directly to the tension between the monistic and dualistic tendencies in twentieth-century epistemology. In logical empiricism, as we saw, this takes the shape of a tension between the 'unity of science' theme and the disparate categorizing of fact and value, science and non-science, logic and psychology, etc. for purposes of explanation. In Laudan's objective historicism, the tension is greatly transformed, but still centrally present: here it takes shape in a tension between Laudan's espoused "naturalism" and his commitment to an asymmetrical scheme of historiographical explanation or inquiry.

In concert with this conception of explanation in the human sciences, Laudan's statement of the arationality principle in *Progress and its Problems* (1977) promoted a division of cognitive labor along disciplinary lines, a division supporting primacy claims to Mertonian structural functionalist methods of historiography: "The sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits.... Essentially, the arationality assumption establishes a division of labor between the historian of ideas and the sociologist of knowledge" (p. 202). *Progress and its Problems* posited a clear inferential relationship from the internalist or rational reconstructionist history of ideas to objective desiderata for preferring certain research traditions over others, and for preferring a certain theory about progress.
Now, Laudan conceded both that the historical lineage of ideas is important to their understanding, and that past versions of the arationality assumption have had the status of articles of faith in the thought of most rationalistically-minded philosophers. Yet the historical account I have given linking Lakatos' thesis of the "autonomy" of internal history to Reichenbach's *a priori* assumptions about epistemologically-couched explanations, will carry little weight with Laudan. Although Lakatos' influence on Laudan's understanding of historiography is surely great, Laudan claims that his version of the arationality principle is a rehabilitated, empirically-based thesis. Central to Laudan's attempt to defend it on a consistently empirical basis is the *methodological* status he assigns it. He is not defending Lakatos' 'autonomy' of internal history, but only insisting on the methodological *primacy* of epistemologically-couched accounts over sociological accounts where it is the history of science that is the focus of inquiry.

The arationality assumption, we must stress, is a methodological principle, not a metaphysical doctrine. It does not assert that 'whenever a belief can be explained by adequate reasons, then it could not have been socially caused'; it makes the weaker, programmatic proposal that 'whenever a belief can be explained by adequate reasons, there is no need for, and little promise in, seeking out an alternative explanation in terms of social causes.'

This methodological status falls short of the full-blown claim of the *redundancy* of social history to the history of ideas. This methodological principle enters to adjudicate dispute where sociological accounts are thought to compete with epistemically-couched ones, and does not have the absurd consequences that follow from the radical separation of the rational and the social. The idea here, what gives the principle its "empirical base," is that internal and

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16Laudan (1977), p. 202-3. Compare p. 166: "...if one can show that someone's action was reasonable (under the [historical] circumstances), then there is nothing left to explain — our explanatory task is finished".
external historiography can be seen as alternative competing research traditions; the former with an impressive track record, accounting for "more" of history of science than sociologically-couched historiography. This is to treat internalist and externalist explanations as competitors, and their competition as adjudicable on an inductive basis of "how many" cases each research tradition has been able to account for up to this point.¹⁷

Still, one might wonder who besides Laudan takes this question of explanatory success to be straightforwardly empirical; the notion of an "empirical track record" of a research tradition can easily become distortive if it conceals valuative aspects in the notion of successful explanation. It is entirely unclear what methodological force Laudan means to attach to the conclusion that one tradition has accounted for "more" of history of science -- more individual cases-- than another tradition. Counting cases hardly is grounds for a methodological preference, for instance, if all of the extant traditions are, as I would maintain. seriously deficient. This issue of counting successful explanations, even if it could be glossed as straightforwardly empirical, is hardly adequate grounds to support even a "methodological" version of the arationality principle; it imposes no necessity on the inquirer who holds that all the existing research traditions are seriously deficient. For Laudan is assimilating what appear to be two separable questions: the ability to make a non-arbitrary choice between two particular competing causal accounts of an historical case, and the ability to make a non-arbitrary choice between two competing historiographical traditions. Moreover, as we shall discuss in greater depth below, the criterion Laudan utilized in Progress and its Problems to identify the "best theory of rationality" is intimately tied up with

¹⁷ The latter tradition is younger, making it more difficult to compare its relative worth on the basis of a shorter record; but has this empirical record improved in the past twenty years, while numerous journals and college departments have been devoted to sociological study of the cognitive content of scientific beliefs and decisions? If they have, it is the result of ignoring Laudan's division of labor. But this point aside, Laudan answers in the negative.
the intuitionism he espoused. *The objectivity of this choice was said to be assured by what he called shared "pre-analytic intuitions" (PIs) about exemplary cases of rationality. The best theory of rationality is explicated as that "which does the greatest justice to our PI's concerning HOS!*" (p. 165). 18 Thus the demand Laudan makes that historians utilize the "best available set of norms" is unmotivated if they have contrasting intuitions about rationality, or if they think none of the established research traditions present the best available set of norms.

Then again, one may question whether Laudan himself consistently treats the principle in the restricted manner that his "methodological" formulation of it indicates. For there appears to be a *prima facie* tension between the two formulations of the principle we have already developed. Laudan's claim that it is a decision procedure for adjudicating conflict between actually existing *competing* accounts of case history, appears to be at odds with the claim that the principle represents a "division of labor" broken down by mutually exclusive spheres of content: the point is that the "methodological" status of the principle as a decision procedure does not motivate, and indeed appears even to undermine the grounds for conceiving the principle as establishing a "division of labor." A division of labor of the kind Laudan claims to have established would appear to render the inquirer blind to competition. One question

18 "It is the historian's intellectual -even moral- obligation not only to be self-conscious about the kinds of norms he is applying, but also to see that he is utilizing the best available set of norms. How can he make that choice? By accepting that model of rationality (or perhaps those models if we can find more than one satisfying the appropriate conditions) which does the greatest justice to our PIs about HOS," (1977, p. 165). Laudan's approach was to seek a set of "preferred pre-analytic intuitions about scientific rationality". "The degree of adequacy of any theory of scientific appraisal is proportional to how many of the PI's it can do justice to" (p. 161). Compare Laudan (1986, 1987, and 1989). Laudan viewed this as alright 1) because in 1977 he viewed rationality theory is parasitic on a theory of progress, which he views as explicitly normative (whereas today he apparently views the two issues as merely separate, without basing one on the other); and 2) because the circularity was not considered vicious so long as doing justice to our PI's was taken to confirm that the norms utilized by philosophers were "the best available set" of norms.
for us then becomes, 'is the principle treated consistently as a methodological maxim, or is it
sometimes presumed to have implications that we can see would not follow unless it really is
an article of objectivist faith?' Another is, 'are both versions consistently defensible on
empiricist grounds?'.

But perhaps we should work into these issues with a slightly easier question, addressing
why Laudan thinks a rule-governed decision procedure is needed at all. Why do philosophers
need to "lay down in advance some way of deciding on the boundaries of the potential
problems of cognitive sociologists" (p. 201) at all? Laudan's defense of the methodological
need of such an adjudicatory principle is remarkably sketchy. He says there is a practical
reason and a theoretical one. The practical objection is the need to delimit the problem-set
for cognitive sociology (p. 200). But since Laudan's discussion was prefaced by mention of
the problem-set as those cases in which there appears to be competition between sociologists
and rational reconstructionists, the domain already seems to be delimited. The practical need
to delimit it in terms of a division of labor along lines of demarcated content remains
unjustified by this pragmatic consideration alone. But if we look to the theoretical problem to
find further justification, we are not enlightened on this matter. The theoretical problem
Laudan addresses appeals to the suppositious claim that if all beliefs had the same causal

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19If there is genuine competition at all, as Laudan's methodological formulation appears to grant, why not a
merely a case-by-case decision procedure? Apparently because there might be no common ground for agreement
on what constitutes a good explanation, so that we have not competition at all, but only the relationship between
two 'incommensurable' explanations, perhaps both equally reductionistic. We can easily concede to Laudan that
there is good reason, for example, that the doctor has a decision procedure in examining a patient's reported
symptoms, and seeks the most plausible and obvious medical causes first. In its most innocuous glossing, the
arationality principle amounts to little more than the suggestion of such a common ground for philosophers and
sociologists, biologists, psychologists, etc., who have often been prone to deterministic and reductionistic
explanatory schemes. But reductionism must be avoided not merely by methodological fiat, but by reconstructing
the conception of epistemological interests on a more historically-informed, and thereby firmer, footing. The
doctor, too, uses his presumption of an ordering of potential causes of illness as a first step to further testing, and
not as putting an end to the empirical questions he faces.
origin—that is, all were "simply determined by the social decision of the believer" and none had a different causal origin in "rational deliberation"—then "the whole enterprise of cognitive sociology would be self-indicting" (p. 201).²⁰

This attempt to convict the symmetrical historiographer of self-referential inconsistency is, I think, unfair, considering the respect paid to the self-referential problem by cognitive sociologists. Bloor's problems with respect to it, I agree, are considerable, but this I view as due to the fact that in defending his program as a descriptive, causal 'science of science,' he shares with Laudan something of a causal and deductive-nomological construal of the interest explanations he (Bloor) sees historiography as supplying. Now self-reflexion is not per se otiose; it becomes so only when it undermines the practice it reflects upon. Moreover, the problem of circularity comes back around to the normativist with no less force than to the descriptivist. If the problem for the SP is the plausibility of a descriptive causal explanation for their own theorizing, the problem may have a parallel in the normativist's problem of choosing between research traditions on a basis of normative intuitions of judgments about rationality. While self-reflexivity is not a problem for the rationalism the SP objects to, intuitionism may be! Given that the SP critic is accusing the objectivist of making good reasons 'their own explanation,' Laudan's way of getting rid of the sociologists challenge before it starts is inappropriate.

Some further analysis of Laudan's claim (above) to restrict the principle to a methodological status is called for by way of a preliminary discussion. Laudan drops such

²⁰Laudan thinks "rational action can be readily assimilated to deliberative activity and irrational action to non-deliberative activity" (1981, p. 73). This points out two classes, deliberative and non-deliberative, for which a good case could be made for genuine causal differences. However, these are not readily assimilated to the rational and the irrational. If rationality is defined in only this formal way, it is far too broad to be of much epistemic interest, since deliberation may take place even where the criterion employed not only is inconsistent with means-efficacy, but actually contrary to it. A normative conceptions of rationality seems to be required in order to make any sense of notions such as comparative efficacy.
terms as 'adequate reasons,' 'need for' and 'promise in' etc., into this formulation. First, if the principle does not preclude the possibility that a belief, explainable by adequate reasons, may still have been 'socially caused,' doesn't the methodological proposal stand in the way of discovering this fact? As I read the passage, it appears to say merely that even if there were such an explanation, it is not one we should, or need to be, interested in. For then they can be judged to rest of theories less progressive than their rivals. But if something like this reflects the difference between the "metaphysical" and the "programmatic" Laudan leans upon here, it presumes much about what interests historiographers should have, and not merely what theories they should hold to be vest supported (more on this below).

Second, the Laudan of 1977 fully realizes that notions such as "adequate reasons" for a belief are normative notions, but he thinks that their use is appropriate in this context because philosophers and historians cannot avoid utilizing a theory of rationality, and should do so explicitly; the alternative would be a merely descriptive account. With this of course I agree, but I discuss a possible equivocation between valuative meaning and causal significance in section 5.4 below. The relationship between the natural (causal) and the normative (valuative) meaning of terms in Laudan's normative naturalism must be approached carefully, since Laudan intends for there to be this dual meaning in such terms as "adequate reasons." The question of whether or not the circularity in the notion of normative naturalism is "vicious" is a question that I approach but do not directly address here, since I only argue in a more restricted fashion that this is the case with specific uses of normative terms Laudan employs in explicating the arationality principle: those that I find to be wrapped up with the intuitionism of Laudan's 1977 conception of rationality, and those for which his current minimalist criterion of rationality are insufficient to ground.
Third, the mention of "adequate reasons" also points to an important impasse between my *satisficing* conception of rationality and Laudan's *maximizing* conception. The latter account is much more restrictive than the former, since it restricts rationality to the instrumentally *best* choice among means to any end; it is tied up with the assumption that there is always or generally an objective way to determine the uniquely best means on a purely empirical basis. On the satisficing account, by contrast, it remains possible that there are at times "adequate reasons" for several incompatible decisions; even if there are adequate reasons for a decision, one does not preclude legitimate (social scientific) interest in the question, 'why this good decision and not some other.'

Fourth, Laudan's explication of the arationality principle also made claims to the effect that there is "no need for" and "little promise in" explanations in terms of social causes when epistemologically-couched accounts are available. To my mind, this strikes at *restricted interests in explanation* which Laudan has, and which I think show the arationality principle (and asymmetrical historiographical methods) to ill-apply to historiographers others who do not share his interests in explanation. If "deliberation" and "rational causes" are only some among various *necessary* causes of a belief which historiographers select as important to their own interests in explanation, than other historiographical interests than those Laudan is interested in are hardly precluded. But if Laudan holds, as he continually seems to, that the historiographer is discovering *sufficient* cause of a belief/decision, this would place a burden of proof on him to demonstrate that the reasons cited truly are sufficient. This would seem to entail proving that no sociological account of the sufficient causation of that belief/decision *can* be found. It is *because* Laudan appears to be claiming that "good reasons" accounts identify sufficient and not merely necessary causal conditions, that *SP* advocates have often charged he places a burden of proof upon himself to provide a kind of 'negative
demonstration' that no conditions will come to light in the future which make a competing
sociological account (Bloor 1981; Manicas 1990). But to reject the reading of Laudan’s
historiography as seeking accounts of this kind would lead straight away to deeper suspicions
about the "explanatory" theory of rationality which Laudan (1981) uses as his main defense
for the view that confines sociological inquiry to the explanation of 'non-rational' actions and
episodes.

Since versions of historical materialism do appear to have motivated many of the
historiographical traditions sociologists have suggested, I want to be clear in assessing the
extent to which the arationality principle goes beyond a reasonable response to historical
materialism. Russell Keat has defined materialist social theories as holding both

(i) that the values accepted by social groups have little if any explanatory power,

and

(ii) that the acceptance of those values can itself be explained primarily by reference
to "material" conditions —"material" being taken to exclude, at the very least, any
processes of a rationally or reflective nature.21

I note here that (ii) aggravates rather than sublimates the contrast between sociality and
rationality, and hence should not to have a place in the SP if Barnes and Bloor genuinely aim
to surmount that contrast. Keat and others have claimed to find a deeply materialistic strain
in the SP, however. The arationality principle surely serves as one way of guarding against
the a priorism of (i). Comparing the empirical track records of rationalist and materialist
traditions, and basing acceptability of an explanation on the superiority of a track record of
research, serves this purpose. But the problem with the arationality principle is that it is
much stronger than what is needed to guard against the a priorism of materialist accounts.

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Focusing further on Laudan’s detailed controversy with David Bloor of the Edinburgh school will allow a fuller development of the assumptions about "good reasons" explanations (epistemologically-couched accounts) implicit in Laudan’s version of objective historicism. In the next sections I focus first on Laudan’s assumptions of pre-theoretically supplied categories of belief and decision. Following this I will discuss the manner in which Laudan moves from this dualistic classification of beliefs and decisions, to causal-explanatory conclusions, and finally to conclusions concerning the primacy of internal or "good reasons" explanations. These discussions can be aided by organizing them in relation to three subtheses into which I think the arationality principle can be broken down. I will call Laudan’s thesis that "rational beliefs...have a different causal grounding than beliefs we come by irrationally," (1) the causal dualist thesis. To warrant the division of professional labor the arationality principle asserts to be appropriate, more than the causal dualist thesis is needed. The path leads through two other sub-theses implicitly held by Laudan: (2) an explanatory asymmetry thesis, which ties the explanation of a belief or decision directly to its causal origins, and 3) a principle of the primacy of internal explanation, in which is entailed a particular philosophical understanding of the content of epistemologically-couched explanations.  

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All three I see as basic tenets of objective historicism. Parenthetically speaking, (1) the causal dualist thesis, asserts a difference in kind between beliefs or decisions (Laudan includes both belief and decision as capable of classification into rational and irrational), a difference based on a dualistic model of causal origins. The warranted assertability of the causal dualist thesis as an empirical claim is taken to impose the basic framework within which all historiography (history of ideas) and sociology of science is legitimately carried out. (2) The explanatory asymmetry thesis, asserts that the mode of explanation for any belief or decision is entailed by the belief’s causal origins. It asserts that we should explain rational and non-rational beliefs asymmetrically, by appealing to their respectively different causes. (3) The "primacy thesis," asserts something more about the status of the explanatory asymmetry. It insists upon a certain understanding of the reason invoked in rational reconstructions and epistemologically-couched accounts, an understanding construed as justifying an objectivist view of historiographical evidencing.
5.3 The causal dualist thesis

All versions of the arationality principle, including Laudan's, defer ultimately to a thesis of causal dualism in the formation of beliefs. In his article directed at the strong programme, "The Pseudo-Science of Science?," Laudan (1981) writes, "Where the symmetry principle insists that rational and irrational beliefs are to be explained indifferently, the suggestion here is that rational and irrational beliefs arise in different ways and thus are to be explained by different mechanisms" (188). While Laudan and Bloor appear to take it as a shared assumption that historiographers are, or should be necessarily causal in their ambition, I see the issues of rationality-ascription and causality as separable, and will attempt to discuss them accordingly.

The thesis that beliefs arise by different causal mechanisms seems to be sound common sense on one level, for we typically do think that some beliefs are arrived at by ratiocination and others are not, being instead the result of submission to force or to the unconscious authority of customs or habits. But how far can this difference be pushed? Not as far, I think, as Laudan wants to take it: to a view of ratiocination as the sufficient cause of a belief, and to a "causal" and "explanatory" theory of rationality. Laudan rebukes what I see as the progressive move of the later Lakatos of untying of the knot between normative reconstruction and causal significance (or prescribed normative model and causation); Laudan's position in response to Bloor instead reaffirms the more traditional (and early Lakatosian) causal-explanatory ambitions of individual rationality theory. "Unlike Lakatos' 

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"Unless one puts forward the absurd thesis that our reasoning processes never play a causal role in leading us to believe what we believe, then we must acknowledge the fact that rational beliefs (i.e., beliefs arrived at by a process of reflection and inference) have a different causal grounding than beliefs we come by irrationally" -(1977), p. 188. He holds that even if reasoning processes are a kind of black box, any conditions for one set of belief that differ from those for another set already establishes causal dualism.
model, most theories of rationality are explanatory in their ambitions" (1981, p. 189).24

Laudan maintains that his minimalist account of rationality is still

* a causal theory of rationality, in so far as it avers that reasons can, and often do, function as the causes of belief. It rests on a contrast between beliefs that result from a process of ratiocination and reflection and those that do not. It insists that there are specific mechanisms for the generation of rational beliefs (among others, various inferential mechanisms) which are not involved in the generation of unreasoned beliefs.25

On such a causal account, the difference between "kinds" of belief results from a difference of causal origins; beliefs resulting from "ratiocination and reflection," are to be classed separately from those caused "by the direct action of social and psychological forces unmediated by reasons." Clearly the classes of the rational and the irrational are not intended to concern static characteristics of beliefs for Laudan; what is important for him as for most traditional epistemologists is not only the content of the belief, but the process of how it was acquired. The epistemologist wants to tell a good reasons story about this. Hence, while the conditions of rationality as Laudan conceives them are formal ones, they concern conditions for holding beliefs, and not the character of the beliefs taken independently of this. The relationship between the minimalist theory of rationality and diachronical historiography is such that Laudan would affirm Newton's utilization of metaphysical and theological beliefs are part of the rational-making character of Newton's acceptance of the new mechanics; it was rational in the light of his broader background beliefs. I have no trouble with this diachronical historiography, but only want to point out that a diachronical view of conceptual

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24 As Newton-Smith put this position (also in 1981, and some five years before Laudan himself would repudiate it), "To explain an action as an action is to show that it is rational". Newton-Smith (1981) *The Rationality of Science*, p. 241.

beliefs and background commitments means that "rationality" can no longer be glossed as depending on a set of isolable appropriate intellectual norms and procedures. For this would be an entirely distinct way of glossing rationality than the minimalist account claims to warrant.

Yet Laudan's causal theory of rationality still commits him to a project of formally or pre-normatively demarcating the conditions for rational and non-rational decisions. The SP critique focuses on the objectivist making evaluations of belief a pre-condition of their explanation, and the objectivist responds that the circularity is avoided because the categorization of decisions proceeds pre-normatively. If the demarcation of classes of decision were the result of prior evaluation, the objectivist would be cut off from the strong causal role he attaches to explanation on the basis of this classification. Hence we hear Laudan saying that the arationality assumption is "crucial as a demarcation criterion between rational explanations of belief and extra-rational explanations thereof" (1977, p. 203). But the SP considers the objectivist to be placed in a problem of pre-normative demarcation no less serious (and perhaps logically related to) the problem the received view took so seriously, of finding a criterion for the "demarcation" of science.26

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26 This indicates that Laudan's assertion that a theory of rationality should be a causal theory remains mired in a set of problems similar to those that beset the positivists. But Laudan has written at length against the exaggerated importance positivists attributed to the "demarcation problem," and he does not use the rational/arational belief distinction as assimilable to the scientific/non-scientific belief distinction. How he does use it is as explicating the internal/external history distinction, or the division of beliefs into those to which sociological explanation is amenable and those to which it is not. This being the case, I think we can still ask whether Laudan's demarcation problem does not face the same issues, framed differently, that we found afflicted Lakatos' sharp division between internal and external history, and which forced Lakatos to address the received view's problem of the demarcation of science. It matters little here whether one sees Laudan's arationality assumption as an extension of the demarcation problem, or as he would insist, as a separate one; the point is that they are at least historically linked via Lakatos' influence on Laudan's views of historiography.
Laudan wants to say that "a belief is rational or reasonable provided the agent can give reasons for it and can show that those reasons were antecedent to the adoption of the belief" (187). If we are speaking of doxastic states as the result of cognitive decisions, it is strange indeed to think that there are decisions whose basis could not even in principle be citable; doxastic states for which no "reasons" in the broad descriptive sense are at all citable by cognitive agents are difficult to classify as the result of a process of cognitive decision-making at all, and seem to presuppose instead some kind of straightforward determinism. In a similar vein, Bloor questions what examples besides pure appeal to perceived authority or force, could be given of beliefs 'caused by the direct action of social and psychological forces unmediated by reasons' of any kind. Although much human action is seen to be habitual and non-deliberative, reasons for a belief or action are usually citable by agents. The question of the rational adequacy of these reasons and appropriateness of habits is a second, valuative, issue.

From where, now that Laudan has acknowledged the problems with his intuitionistic stance in "Some Problems Facing Intuitionist Meta-Methodologies" (1986), can he derive a pre-normative criterion of demarcation? Since Laudan does not concede that his repudiation of intuitionism undermines the grounds for the arationality principle, he must believe that he can meet the burden to supply criteria for demarcation of causally different classes in another way. To summarize, in order for the 'post-intuitionist' Laudan to justify the arationality principle, he must justify the categories of belief he uses as neither 1) the result of prior evaluation of beliefs, nor 2) intuitions of the foundational, pre-analytic type he now repudiates. And this is in fact the direction that Laudan's very substantial revisions have taken, as exemplified in his attempt to treat the causal dualist thesis as an empirical hypothesis, as well as in his movement away from the explicitly prescriptive construal of
rationality in 1977 and towards the formalism of the "minimalist" or means/end instrumental theory he holds today. The break that this required between a theory of rationality and a theory of progress is quite familiar to readers of Laudan’s "Progress or Rationality" (1987).27

His recent attempts, then, to construe the causal dualist thesis as an empirical claim, is the first thing I must respond to. In the Laudan/Bloor exchange, both men claim to deal with their respectively preferred theses of causal difference or sameness of beliefs, as empirical questions and open issues. These are worthy intentions, but I think anyone who reads this clash of polarized views must come away skeptical that either one genuinely treats the issue in this fashion. The claim by each that the other in practice deals with them the issue of kinds of belief by fiat is far closer to what I take to be the case.28

A determination of what constitutes sameness or difference of "kinds" of beliefs must, of course, be partly a semantic issue.29 That Laudan concedes he cannot "close" the question

27 Laudan’s restriction of normative concerns to a theory of progress means for Fuller that his normativism gives up all chance for a theory of the scientific reasoner, and all relevance of cognitive science to normative epistemology. "In his earlier work, Laudan (1977, ch. 5) wanted to capture the ‘preanalytic intuitions’ of rank-and-file scientists who came to realize that a particular research tradition is worth pursuing over its competitors. While several philosophers (including Fuller 1988 and Laudan 1986) have cast doubts on the wisdom of this approach, at least it paid lip service to the need for a theory of the scientific reasoner. In nowadays separating the claim that science has made progress from the claim that science has proceeded by individually or collectively rational means, Laudan has not so much eliminated the problematic intuitions as relocated them to the more secure ground of the inveterate kibitzer, the reflective historian. What has been eliminated, however, is the scientific reasoner, who Laudan concedes is philosophically unfathomable." -1989, p. 54.

28 "The symmetry thesis, for instance, seems to involve the a priori dogma that all beliefs are caused in the same manner. Equally dogmatic was Bloor’s insistence that social causes in science were more fundamental, more primary and more pervasive than any other sort" -Laudan (1981), p. 74. Bloor’s response to Laudan’s asymmetry thesis is remarkably similar, and, I argue below, is equally well-directed, with some qualification. This is not the same as saying Laudan commits the so-called ‘genetic fallacy.’

29 Some further prefacing remarks on the notion of "cause" and "sameness" or "difference" of cause are in order before proceeding with the discussion. Laudan charges that Bloor’s notion of the sameness of the causal
of causal sameness or difference is one reason that the arationality principle is seen as a
*methodological* principle. I certainly do not think Bloor treats the issue in any more
consistently an empirical fashion than does Laudan. But we can agree, I would presume, that
there is a clear difference between asserting there are different *kinds* of belief (or different
causes of belief), and normative questions of assessing the evidential support
epistemologically-couched reasons offered as a basis for *adoption* of those beliefs. At present
I intend only to raise this question. But the question is very much in line with the tenor of
the strong programme’s criticism of Laudan. The notion of beliefs “caused by reasons” is
one object of their scorn, since they take rationalists to task for reifying into natural classes
what they say are “evaluations” of belief. In light of the equivocation I warned of in the last
chapter between doxastic processes and epistemic warrants, this takes on special importance.
While it is difficult to say that the source of confusion lies on one side or the other, the
Laudan/Bloor dispute—like most exchanges between objectivists and historicists—surely
seems to be carried forward through conflations of this sort.

To summarize, Laudan’s emphasis in recent years has been on a historically-
contextualized instrumental (empiricist) version of rationality theory in contrast to his earlier
explicitly prescriptive (intuitionistic) version. Since all rationality is instrumental on his
present view—a matter of an agent’s choice of the best means to his/her goals—rationality is

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origins (not to mention their characterization as “social”) presumes wrongly that we have a topology of causes. I
agree with Laudan that Bloor’s notion of causal “sameness” is not merely an “inductive” truth as he attempts to
make out. Like the arationality principle, the symmetry principle should aspire to be no more than a
methodological principle serving a rather narrow range of explanatory interests. The manner in which it is
presented as a thesis of causality belies the methodological characterization Bloor gives it in one of his more
circumspective moments. Yet Laudan’s own assumption of causal dualism is at least equally open to the same
criticism he forwards of Bloor, that it hinges on our having a topology of causes. Indeed, when it comes to issues
of explanation, symmetrical methods would appear to have a prima facie advantage: they garner support merely by
denying grounds for causal asymmetry, and need not hinge on a positive assertion of causal monism; whereas
Laudan’s explanational model depends on the Laudan’s ability to defend the assertion of causal dualism on
empirical grounds.
considered as a feature of beliefs and decisions that can be discovered by empirical study.

One might say then that Laudan still expects his current minimalist conception of rationality to play the same fundamental role rationality theory had previously played for him, and to play it in spite of the fact that the formalistic conception of rationality theory he now holds is far weaker than the one he held previously. This is interesting, because there is very little in common, as far as I can see, between the content of Laudan’s concept of rationality now, and in 1977. Perhaps this flip-flop is itself reason to doubt the status of rationality judgments in Laudan’s account. What links these two accounts is just the grounding *role* rationality theory (or the explanatory role rationality theory) has been expected to play in Laudan’s historiography from 1977 to the present.

Clearly then, a fuller discussion of these issues would concern the empirical character of Laudan’s account of rationality, and whether the instrumental conditions for rationality it is considered to entail are able to provide a notion of rationality strong enough to do the work Laudan expects of it. While we must not become side-tracked with a lengthy discussion of recent work on individual rationality theory, it is important to see, firstly, that the type of rationality theory Laudan currently employs is one that usually in the literature goes under the name of a "minimalist" account. This is one that is instrumentalist, in the sense of judging the efficacy of agents’ actions and means or methods only in terms of their own ends in view, not in terms of the presentist or ethnocentric ends of the inquirers. Rationality then becomes equivalent to the technical-instrumental interest in prediction and control, a view that

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30This is a retreat from what Laudan called the rationalist’s view of meta-methodology as an ‘omnibus’ theory of rationality of a strongly explanatory character. The omnibus theory was strongly Whiggish or presentist; but the minimalist account by contrast can aspire to little more than a statement of necessary conditions for rationality. In contexts such as those of theory pursuit and method choice (if not also at times in theory appraisal) these relatively weak conditions may be met in a plurality of ways. Here then is the inferential ‘gap’ that the minimalist account of rationality faces when considered as an explanatory theory.
denies that the focuses on ends in critical theory and interpretation/hermeneutics concern the problem of rationality.

Secondly, this instrumental account is cashed out in terms of maximizing efficacy of choice. There are certainly alternative theories (i.e., satisfying theories) that are available competitors. Despite Laudan's 1977 claim that the "richest" account of rationality should be preferred, maximizing accounts are much stricter (less "rich") than alternative satisfying accounts, and result in capturing less of actual scientific history.\(^3\)

Thirdly, the minimalist treatment can be seen as an attempt to provide a notion of rationality that fits the Popper/Hempel covering law model of explanation; that is to say, 'good reasons' explanation can be made to fit the Hempelian model if the "universal" empirical laws that model requires can be supplied by an instrumentalist conception of rationality. But the legitimacy of the claim that this is the best theory of rationality available for social scientific inquiry, is no stronger than the claim that this model best fits the subject matter of the human sciences and the humanities. It is noteworthy here that the pragmatist tradition has always dealt with means-end instrumentalism as an abstraction from a much thicker and more complex causal network.

To conclude this section, we have noted that causal dualism is a background assumption of rationalist epistemology which has rarely been explicitly advanced as a thesis and argued for, and that recent attempts by Laudan to justify it as an empirical thesis has been less than compelling. My final section will discuss the Laudan/Bloor exchange in relation to what I called Laudan's subtheses (2) and (3), and I will argue that even if we accept some version of

\(^3\) What Bloor called the rationalist's 'retreat to the limiting cases' in discussions of rationality indicates a broad retreat by rationalists away from claims to be able to specify sufficient conditions for rationality, in the sense of one optimally correct choice in any given decision-situation, and towards retaining merely necessary conditions (intellectual honesty, etc.) that might, in specific instances, allow a multiplicity of "rational" choices.
the causal dualist or causal monist theses, neither Laudan’s and Bloor’s methodological directives follow as they expect. In this sections I continue to treat the question of difference or sameness of causal grounding of beliefs as an open question, but focus on conceptual biases that shape the antithesis between asymmetrical and symmetrical explanatory programs.

5.4 The explanatory asymmetry and primacy theses

The explanatory side of Laudan’s model of rationality can now be presented more fully:

If there are some beliefs arrived at by ratiocination and others which are not (and this is surely an uncontroversial claim), then it is evidently sound tactics to insist that our explanations of beliefs should reflect those differences...Unlike Lakatos’ model, most theories of rationality are explanatory in their ambitions. They maintain that rational beliefs are arrived at differently than irrational ones are, and that each calls for a different sort of causal story.³²

It certainly seems innocuous to say that if there are causally different kinds of belief, our explanation of why one comes to hold a certain belief or make a certain decision should reflect this fact. Laudan here rightly sees a prima facie stress between any recognition that there are different kinds of belief, and the methodological maxim embodied in the symmetry thesis, that asserts that all beliefs are to be explained in the same way. But on application, Laudan’s methodological directive is not as straightforward as he intimates. To begin with, why the focus on this breakdown of the content of beliefs and not some other? More

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specifically, why is the rational/arational distinction the only one Laudan seems to allow as having causal implications for the formation of beliefs?\textsuperscript{33}

Laudan's assertion that some beliefs "caused by reasons" should be "explained by reasons" must be considered together with Bloor's complaint that rationalism in both its traditional and contemporary manifestations "makes 'logic, rationality and truth' appear as their own explanation." Bloor's incisive critical point had been, not that nothing corresponds to this notion of internal dialectic, but that it represents continues to be mis-described in objectivist epistemology.\textsuperscript{34} In "More on Bloor," Laudan (1982) says that a view that a rational belief is "its own explanation" is just a version of the view that rational beliefs are "uncaused." Hence it would appear that he concedes that strongly rationalistic conceptions of knowledge-acquisition as a self-revealing and self-justifying process --what Mannheim called explanation by simple appeal to "the internal dialectic of knowledge"-- are viciously circular and provide no explanation at all.\textsuperscript{35} But, Laudan continues,

To give a rational explanation of a belief is to point outside and beyond the belief itself to the background beliefs and deliberative processes of the agent....Accordingly,

\begin{itemize}
\item \textsuperscript{33} Causal explanations, Dray argued, are also selective, in the sense that they only specify certain necessary conditions among others that the inquirer regards as particularly important or interesting. Beliefs may be explained in different ways by appeal to different aspects of their sufficient cause. If this is correct, then the explanatory asymmetry requirement is not straightforwardly indicated by a presumed difference in causal origin of belief.

\item \textsuperscript{34} "The counterpart of the 'immanent laws' that govern the growth of knowledge", Bloor comments, "is to be found in what Lakatos called the rational reconstruction of the 'internal history' of science." -David Bloor (1988) p. 61.

\item \textsuperscript{35} Bloor (1988) concedes that something does correspond to Mannheim's notion of an inner dialectic, or to the "moving force of rationality itself." But his point is that this something has been "misdcribed" by rationalists who maintain a dualism at the heart of their position. It is not difficult to see also that Bloor's own explanatory 'monism' may be an over-reaction to this state of affairs, and that even if not reductionistic of normative discourse, it aspires only to explanations of decisions in terms of social interests quite general and perhaps so uninformative as to be of little use or methodological interest to practitioners.
\end{itemize}

201
advocates of rational explanation (or explanation by reasons) are no more guilty of making rational beliefs their own causes or their own explanations, than they are of making them uncaused.\textsuperscript{36}

In one light, this response to Bloor is an articulation of Laudan's reticulated model of meta-methodology and is supported by pragmatic conceptions of belief warranting as a defeasible, bootstrapping process. In this sense Laudan's approach merely reflects the epistemic situation in which human agents find themselves. Critical self-reflection has to start somewhere, and where it starts is always with the background beliefs and natural reasoning capacities of the human agent. The historical nature of the background beliefs that represent our starting place in philosophical reflection implies neither objectivism nor relativism: it neither confers a necessarily privileged status on our own actual traditions and background theories, nor implies that all change is mere contingency. The necessity of social history to the understanding of change does not mean its sufficiency in explanation. The all-engulfing use of terms like "social" and "historical" is a semantic categorization, and does not entail that human beings are uniquely well-characterized as "social" and "historical" beings, or cannot be equally-well described in other terms, "biological," "psychological," etc. Historical conditioning is no more assimilable to historical determination than biological conditioning is to genetic determinism. There is, we may affirm, strong inductive ground for holding that our historically-conditioned starting points are not merely the result of historical "accident," but also of progress and of learning through the development of traditions with a high degree of internal continuity. This can be maintained even while recognizing that progress and learning are normative concepts implying retrospective evaluations of change.

\textsuperscript{36}Laudan (1982), p. 72. Laudan is right to criticize Bloor as having wrongly persuaded himself "that if he once grants that the causal mechanisms productive of rational belief differ from those that produce irrational belief, he will be forced to restrict sociology to the study of the irrational" (1981, p. 190).
However, despite the availability of cogent ways to argue against sociological reductionism, it seems equally clear that Laudan misses much of what is at stake in the exchange with Bloor. One example of the rationalism Bloor objects to is Hamlyn's view that beliefs can be pigeon-holed into natural classes, and that there is little more to explanation of a belief than to refer to the correct natural class to which it belongs. Hamlyn for instance says that when the "correct" natural class has been appealed to in explanation, the case "provides no room for scientific explanation, since none is called for" (Bloor 1981, p. 205).

Bloor regards Laudan's explanatory model of rationality and the rational/irrational classifications it is committed to as a similarly misguided, if somewhat more circumspect, version of this same essentialistic approach.

Bloor characterizes all class-judgments as "evaluations." There is nothing wrong with this suggestion, I think, so long as the notion of convention that attends it is not construed in a non-cognitivist light. Restated in a way that does not presuppose radical social constructionism, it is in Laudan's (1977) commitment to the evaluation of a belief as a precondition for its explanation, that Bloor sees a vicious circularity to Laudan's approach. It is here also, or more particularly in the pre-theoretical status assigned to Laudan's classifications, that Bloor sees Laudan as espousing a 'self-justifying' account of rationality. Bracketing Laudan’s claim that his classifications are pre-normative, it is at least clear that Laudan does have a commitment to basing the explanation of beliefs and actions on prior

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37 In their general opposition to methodological individualism, the SP distances itself from the idea that there are only individuals and their behaviors, and that social structures are just 'patterns of interaction' among individuals. But while social structures and institutions are real in this way, the emphasis is on their socially constructed nature. The SP are social constructionists of varying sorts, and typically treat all classes as largely if not wholly human constructs. The radical version of social constructivism and Barnes' related "finitist" theory of meaning is suspect, at least insofar as social constructionists appeal to the underdetermination problem in building their case. For our present purposes we can put aside the question of the opposition between natural class theory and social constructionism, in order to concentrate on class judgments themselves.
classification. It is amply illustrated in Laudan's statement of the arationality principle, which includes the phrases, "When a person does what it is rational to do...," and "whereas when he does what is in fact irrational..."

When a thinker does what is rational to do, we need enquire no further into the causes of his action, whereas when he does what is in fact irrational—even if he believes it to be rational—we require some further explanation.38

In order to see the real radicalness of the SP's criticism of the objectivists, one must understand the parallel Bloor draws between explanations based on a privileged pre-categorization of beliefs (into a dualistic framework) by self-described normative naturalistic philosophers, and the asymmetrical structure of classical "supernaturalist" traditions. Bloor alleges in "Rationalism, Supernaturalism, and the Sociology of Knowledge" (1988) that appeal to asymmetrical explanation on the basis of pre-evaluations of belief has no defense in naturalism, but instead represents a retreat to a kind of "supernaturalism".

The connection of supernaturalism with the rationalist history of science is easily proven. Both are dualist theories. Both divide the world into opposing principles with a characteristic asymmetry of evaluation and explanation. The opposition of spirit and flesh becomes the opposition of knowledge and society. The word of God expressed through Church doctrine is replaced by the inner dialectic which drives knowledge forward.... The internal history of science thus replaces the history of apostolic truth. The category of error replaces that of sin, and heresy in its modern form is hunted down under the name of 'irrationality'. We may even express supernaturalism by commandeering the very words used by our rationalist philosopher [Laudan, above]. Thus: 'When a Christian does what is orthodox, we need enquire no further into the causes of his belief, whereas when he believes what is in fact heretical—even if he believes it to be orthodox—we require some further explanation.'39

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38 L. Laudan (1977) p. 189.

S. Fuller and R. Giere are among those who see Bloor's challenge as devastating to the attempt to make "internal history of science" the focus of historiography. These figures are intent on moving beyond the internalist/externalist antithesis and towards a more constructive dialogue between philosophers, historians, sociologists, cognitive psychologists, etc. But there is another quite straightforward way to argue that Laudan fares poorly in the debate with Bloor. This is to examine what "naturalistic" basis there is for Laudan's asymmetrical treatment of the two issues he himself calls those of "epistemic symmetry" and "rational symmetry." For one of the most interesting aspects of the debate is the purported commitment of both men to a "naturalistic" account of knowledge.

Laudan's exposition of three separable epistemic, rational, and pragmatic questions is developed in his critical discussion of Bloor's "symmetry thesis." That latter thesis, the heart of the "strong programme," states that a proper account of science "would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false, (rational and irrational, successful and unsuccessful) beliefs." Laudan suggests that this be broken down into its constituent subtheses, which he states as

(i) epistemic symmetry: true and false beliefs are to be explained by the same types of cause.
(ii) rational symmetry: rational and irrational beliefs are to be explained by the same types of cause.
(iii) pragmatic symmetry: successful and unsuccessful beliefs are to be explained by the same types of cause.

As an anti-realist, Laudan insists that the truth status of beliefs "is largely if not entirely irrelevant to their explanation." Hence he affirms Bloor's thesis with respect to "epistemic

\[\text{Laudan (1981) p. 184.}\]
symmetry," holding that explanation can get along fine without any pre-categorization of beliefs as true or false.\textsuperscript{41} Laudan's acceptance of the subthesis of epistemic symmetry comes because he, like many other fallibilists, believes that we have no privileged access to the "truth" status of our own theories.

Knowledge of a theory's truth is radically transcendent. This transcendence entails the epistemic version of the symmetry thesis since we are never in a position to partition theories into the true and false and then proceed to explain beliefs in them differently on account of their truth status (p. 186).

Yet interestingly, Laudan must insist on a radically different stance with regard to the status and cognizability of the parameters of rationality and success judgments. \textit{That is to say, he takes a radically asymmetrical stance toward the understanding of the classificatory parameters of belief.}\textsuperscript{42} Laudan sees no special problem in this, since he holds some parameters may be relevant to explanations and others not. Indeed, taking the difference between truth assertions and rationality assertions as his main focus, he asserts "There is no logical connection between these two themes."

This assertion I think deserves further scrutiny. Is it the case, as Laudan alleges, that Bloor muddles the grounds for asymmetrical explanation by illicitly 'sliding' from one distinct problem to the other? What I want to suggest is that such a response to Bloor is inadequate because it fails to address the more general problems Bloor contends attenuate the view that

\textsuperscript{41}Laudan differs in this from many rationalistically-inclined thinkers who lean towards epistemic realism, as for instance Newton-Smith, who insists that "we cannot begin to decide what kind of explanation is appropriate until we know whether or not the belief is true." Newton-Smith's rationalism goes well beyond the more moderate view that we at least need some exemplars, paradigm cases of true and false belief.

\textsuperscript{42}Rationalists, as the contrast of Laudan's and Newton-Smith's positions above testifies, have often disagreed among themselves over which parameters are constructed, which are natural; and they have disagreed further over which of the natural ones are accessible, and which transcendent. They will accordingly differ amongst themselves in deciding which parameters are to be granted a privileged status or treated as pre-epistemically given.
there are *any* objective parameters to belief in the sense Laudan assumes --including those concerning a belief's rationality or irrationality.

Rationalist bias, Bloor insists, enters when one insists that any parameters of belief are free from social influence. This point taken alone can be deemed moderate and pragmatic on the issue of the relation of language to the world, and does not necessarily entail (although some SSK inquirers claim otherwise,) any strong version of social constructivism that makes categorization a matter of social negotiation without the character of the real as a central constraint. The objection to this position of Laudan, in other words, *can* be phrased independently of the SP's radical conventionalism about meaning, as represented in Barnes’ and Bloor’s "finitist" account of meaning.43

Laudan’s attempt to break the connection between responses to epistemic symmetry and to rational (and pragmatic) symmetry, rests entirely on a difference he alleges in the "accessibility" of what he calls the different "valuative parameters" of belief:

> There is a crucial difference in the two cases, having to do with the *accessibility of the relevant valuative parameter*. Because we know how to ascertain whether an agent is behaving rationally or irrationally, we can conceive of a programme of empirical research which would ascertain whether rational belief is produced in the same fashion as irrational belief. But there is no parallel in the epistemic case. Because we do not know how to tell which theories are true, we cannot neatly partition theoretical beliefs into the true and the false and then proceed to examine whether they have similar or dissimilar causes.44

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43 Note that the finitist conception of meaning is closely connected in Barnes’ thought with his own consentualist construal of all norms of "validation" of belief. The "collective contingent judgment" of the scientific community is described as "conventional through and through." "In science...there is no basis for validation superior to the collective contingent judgment of the paradigm-sharing community itself." -Barnes, 1982, p. 27 and 50. Note that this is one clear connection between SP and contingent historicism as influenced by Wittgensteinian conventionalism and Kuhnian consentualism.

That this is an insufficient and indeed highly problematic response to Bloor can be seen by reflecting on several of the involved issues. First, Laudan’s notion of a "valuative parameter" is quite ambiguous with regard to the intuitionism he began with and the thorough-going empirical naturalism towards which he moved. Laudan, like Brown, depends on "taking it for granted what rationality is" (Brown 1988, intro.). Even if Laudan makes no explicit reference to shared pre-epistemic intuitions, the notion of a "valuative parameter" sounds especially close to the intuitionistic posits the Laudan of 1977 allowed that judgments of rationality were. But if a judgment is "valuative," it would appear not to be pre-theoretical except if taken as a normative intuition. The notion of an empirical programme of research into rationality is left so vague as not to have any explicit content, but we can suppose Laudan means we can empirically tell whether the agent’s actions instrumentally serve his/her own goals better than other available courses of action. I have already pointed out (Ch. Two) however, that much valuation is involved in the notion of instrumental efficacy. Judgments of rationality might vary, depending for instance on whether we take means efficacy as reflecting the quickest, surest, more economical, or most potentially rewarding choice of action. These are not the same way of glossing "efficacy," and the same decision might be classed very differently depending on which evaluative orientation is utilized. Laudan’s assertion is that we have an empirical or experiential knowledge of the valuative parameters of rationality-assertions and to success-assertions, but not of truth assertions. But one might counter that the variability in construing efficacy and success are structured by valuative commitments just as much as any other parameter of belief.

This criticism suggests my deeper disagreement with the general need which Laudan sees for asserting ‘pre-epistemic’ foundations in meta-methodology, and in particular for the pre-epistemic status he insists on conferring upon judgments of rationality and success.
Borrowing the form of Laudan's argument concerning the epistemic question, we can advance an equally plausible argument against at least strong versions of rational and pragmatic questions: 'We are never in a position to partition with complete objectivity beliefs or decisions into cognitive and non-cognitive, rational and non-rational, successful and unsuccessful, and then to explain them without circularity by reference to their pertinent class.' The claim to do so lacks naturalistic grounding unless the partitions can be consistently maintained as empirical or formal, and I have shown adequate grounds for denying this claim.

Are the "specific mechanisms" alleged by Laudan to be involved exclusively in the generation of "rational" belief merely natural inductive propensities, or does Laudan present them as normatively posited rules? To confirm the latter would be to undermine the point of employing a specifically "causal" theory of rationality; Fuller would then be correct in concluding that the objective historicist slides between valuative meaning and casual significance, and is shown thereby to be confused about the directly evidential role internalist historiography is expected to play.45

Similarly, Bloor seems right to say that in the way Laudan explicitly presents rationality as a naturalistic concept, its primary reference is only to typical human reasoning propensities. Yet in all versions of his employment of rational or internal explanation as undergirding a process of objective selection between competing research traditions, Laudan seems to slide at leisure between this narrow conception of rationality and a much more full-

45 Fuller's stance in Philosophy of Science and Its Discontents substantially agrees with this criticism of Laudan: "Not only do internalists slide between value and causal significance, but they also presume a curious relation between normative and descriptive accounts of science" (1989, p. 6). Even the confirmed rationalist Newton-Smith (1981) concurs that Laudan as well as Lakatos confused the causal and evidential aspects of rationality theory, and both presented confused views about the causal and evidential roles which they assigned to rational reconstructions.
bodied notion of normative rationality or preferred patterns of inference. There is a sense in which Laudan wants to claim that, as a "normative naturalist," his normative positions are supposed to be entailed by his naturalistic ones. But my conclusions certainly support the implication that this has not been worked out in any consistent way through his thinking on the issues of historiographic methods which we have examined in this chapter. That is to say, he cannot claim to have sufficiently supported this position non-circularly on the basis of his present account of historiography. The latter is not sufficiently developed to support that weight.

If I am correct in contending that there is a valuative dimension to Laudan's selective use of the notion of choice-maximization, then his conception of rationality is stretched beyond credibility in instances where he is found speaking of irrational decisions as those that give evidence more weight than it "cognitively deserves," or in making choices "less adequate" and "less progressive" than what is maximally optimal. Bloor seems right here that there is no naturalistic basis for the manner in which he shifts from the merely formal structure of instrumental thinking to such normative posits. Laudan's approach, in short, can still be considered a version of the 'omnibus theory' of rationality, and of the 'self-justifying'

46 "Since my critic declares himself impatient with anything but a naturalistic approach, the 'asociological' aspect of his model can only refer to natural rationality. On the other hand, the reference to 'good' reasons points towards a concern with normative rationality. Alas, he cannot have both at once" (Bloor, 1981, p. 210). Laudan insists that "We know, contra-Lakatos, that scientific rationality is not static, but constantly evolving" (1981, p. 189). But Laudan himself denies that we can give a Habermasian 'transcendental' account or an evolutionary account of instrumental interests which can provide a sufficient grounds for choosing objectively between competing normative meta-methodologies. Laudan also says quite clearly that "'Rational' functions both as a normative and as a descriptive concept" (187). Hence I think Bloor's charge that Laudan's objectivism is carried through an equivocation between natural and normative rationality is credible. Despite this, Laudan is not wrong in seeing a "normative naturalism" as a viable position. But such a position must be laid out on the basis of a socialized epistemology far different from Laudan's position; for Laudan to encompass the criticisms correctly pushed at him along these lines by the sociology of scientific knowledge (SSK) would mean repudiating or compromising not only the dependence on individual rationality theory but some other aspects of his meta-methodological objectivism.
conception of reason, to the extent that he fails to provide a satisfactory response to his critics on this issue.

A number of clarifications of my preliminary conclusions on Laudan's debate with Bloor are in order. The inadequacy, as I see it, of Laudan's response to Bloor does not imply that either truth, rationality or success are useless concepts in science. Indeed I think they are all of some value, and would agree with Laudan that both their empirical support and their theoretical value may vary. Moreover, I still agree with the central thrust of Laudan's criticism of sociological reductionism. In Chapter Six I attempt to develop what Fullr calls aetioaxiology—the study of the values/causes nexus—as a focus for historiographers very different than the focusses on "internal" and "external" history that objective historicism has made central.

47 For further criticism of SP treatment of interests and explanation, see S. Woolgar (1981), S. Yearly (1982), S. Fuller (1989), R. Giere (1989), and D. Henderson (1990). Barnes and Bloor sometimes speak as if the notion of rules of inference and evidence-weighting are wholly explainable by reference to societal interests; in social constructivism the role of reasoning and evidence in science paradoxically appears to drop out as primary constraints on theory formation, criticism, and selection. On my view, the SP's symmetry thesis draws the wrong conclusion from the constructive aspect of the valuative parameters of 'believe', arguing that they are simply irrelevant to scientific explanation, or at least to explanation from a scientific qua sociological point of view. It is not the case, as the SP's positivistically-inclined notion of scientific explanation has it, that all scientific explanation must be causal, and certainly not the case that categories of "professional" and "class" interest etc. are any more objective valuative parameters than those epistemologists have traditionally maintained.

48 Deconstructing the notion of internal history as the locus of inquiry means challenging the steps objectivists take towards establishing the philosophical significance of the internal history of science. Fuller recasts and challenges these steps, each of which deal integrally with the notion of the autonomy or primacy of internal history. These steps he discusses I paraphrase as follows:

(1) Most of the best cases of scientific reasoning have exhibited an independence, or "autonomy," from other sorts of deliberations. This autonomy may be defined psychologically or institutionally, but in either case the general idea is that the deliberators do not let non-epistemic matters intervene in the epistemic assessment of a scientific claim.

(2) What makes these cases of scientific reasoning so good is their autonomy.

(3) The role of autonomy in increasing the likelihood that the most rational, objective, and/or valid scientific claims will be chosen has been sufficiently great to make autonomy worth recommending as a general methodological strategy.

211
The shortcoming I allege in Laudan’s response is a failure to justify the status he himself demands of rationality judgments, in order for them to support his prioritizing of one mode of historiographical explanation for each of two recognized classes of belief. This is the same as a failure to support what I called Laudan’s thesis of the primacy of epistemologically-couched accounts. There seems little point and much danger in attempting to render our inductive intuitions, if that is what such valuative parameters amount to, as ahistorical principles.

Conclusions

Claims of exclusionary methods and primacy in explanation follow on Mannheim’s division between beliefs determined socially and beliefs determined rationally. We have seen how Laudan’s version of the arationality principle, in its assigned “methodological” status, qualifies much of the implicit dualism taken only as an article of faith in the background rationalist and logical empiricist traditions. In its most qualified form, the principle does not imply that rational decision-making has no social dimensions; it stresses, rather, that in cases where agents have sound reasons for their beliefs, those reasons are the most appropriate items to invoke in an explanation of their beliefs. I have argued, however, that the notion of exclusively appropriate historiographical interests which Laudan employs are still quite suppositional in this ‘methodological’ form of the principle.

(4) There is prima facie reason for believing that suboptimal cases of scientific reasoning—when less rational, less objective, or less valid claims were chosen—are traceable to the intervention of non-epistemic matters. From this tenet follows the division of cognitive labor canonized by Laudan (1977, Ch. 7) as the arationality assumption: namely, that philosophers study scientific reasoning in its autonomous phases, whereas sociologists study it in its heteronomous ones. (1989, p. 13).
Moreover, the stronger implications sometimes drawn from the principle, as a division of labor between philosophers and cognitive sociologists, points out the extent to which methodological nomotheticism continues to color objective historicist treatments of explanation. The establishment of his division of labor, if it is principled at all, is based at least implicitly on a principle of primacy or exclusive methodological legitimacy. Laudan's division of cognitive labor entails a thesis to the effect that epistemologically-couched explanations are primary over one domain, and sociological explanations over another. The effect of the primacy thesis is to lend tacit support to a certain philosophical view about epistemologically-couched accounts, which in turn is taken to justify the objective historicist philosophy of science.

When causal issues are our primary interest, diachronical historiography and a minimalist theory of rationality are important. But when the concern is for the broader, normative interests of meta-methodology, the relevancy of individual rationality theory is not as Laudan conceives it; on my view the dependence of philosophers on rationality theory needs to be supplemented by consideration of sociological and economic interests, aesthetic dimensions of weighting, and by emphasis on the important role of the cognitive sciences in informing us of the potential, limitations, and propensities of human reasoners.

I conclude that Laudan should qualify his commitments to the arationality principle and to the privileging of asymmetrical historiographical methods that accompanies it. This conclusion would indicate a need for reconceptualizing the philosophical status of the "good reasons" explanations that historiographers might offer of episodes out of case history. My conclusion is underlined by the pragmatist view that the meanings of beliefs and decisions, like concepts and statements, are multilayered. The beliefs and decisions philosophers, historians and social scientists examine are colored by social context, and it is a contextualized
conception of reason rather than the thin abstracted instrumentalist conception that leads
historiographers to the most comprehensive understanding of history. As Sarkar has similarly
noted, the meaning of a scientific claim depends on the aspect of meta-methodology of which
it is a part. It is this contextualization of meaning and explanation, I think, that is most
representative of a pragmatic turn in epistemology, and the implication it has is to suggest that
beliefs are not simply true or false, rational or irrational, or wholly explicable by reasons or
by social causes.49

I have construed Bloor’s criticism of Lakatos and Laudan as essentially a reaction to the
unfounded faith in methodological nomotheticism among contemporary rationalistically-minded
philosophers of science. But such a reading need not, and on my view should not, lead to
acceptance of the alternative represented by strong programme historiography, with its
peculiar conception of interest-explanations and relationship to cognitive relativism. It can
instead more plausibly lead, I want to suggest, to viewing both Bloor’s demand for
historiographers to ‘maximize external history’50 and Lakatos’ more traditional demand for
them to ‘maximize internal history,’ as claims which lack the necessity their advocates
attached to these demands. The explanatory claims represented by the symmetry principle
and the arationality principle, that is to say, have a similarly impoverished epistemic basis,
insofar as they claim to represent methodological directives which compel historiographers to
accept either of them.

49 This point is well-made by Lelas (1985), who cites Mary Tiles on the multilayered conception of meaning.
50 Bloor (1988); compare H. Collins, who embraces “an implicit relativism in which the natural world has a
small or non-existent role in the construction of scientific knowledge.” Collins’ methodological directive is as
follows: “The approach we favor is to push the relativistic heuristic as far as possible: where it can be pushed no
further, ‘nature’ intrudes” (Collins 1981 a p. 3 and 438-9).
For this reason I have attempted to focus on shared assumptions in the Laudan/Bloor debate. The account of interest-imputation and the explanatory monism held by the SP is an ill-conceived response to the rationalism and explanatory dualism of the received view. As Fuller has put it, the internalist and the externalist share few common assumptions about historiographical method, and the few they share are probably false. A contemporary philosophy of the social sciences must attempt to redress both the influence of methodological nomotheticism, as well as the equally well-entrenched relativistic reactions to this influence upon philosophy. Clarification of differences between claims of epistemic primacy and issues of cognitive interest in explanation is one route that has not yet been seriously pursued. The final chapter of my dissertation will take up the task of outlining a functional pluralist account of historiographical explanation that differs significantly from both the dualistic orthodoxy as the current monistic reactions to this orthodoxy.
6.1 Towards a pancritical non-foundationalist metaphilosophy

My critique of logicism and historicism addressed one of the major grounds upon which the broad conflict between objectivism and relativism has been debated in contemporary philosophy. Yet scientific meta-methodology is only one locus of this broader philosophical dispute. Our examination could have focused on ethical or juridical inquiry, where issues of the normative force of standards are also debated by objectivists and relativists. As I have argued, the basic issues in the objectivism/relativism debate are of sufficient generality, and the available responses in meta-ethics, jurisprudence, and metascience are sufficiently parallel, that my conclusions would be little different had we taken either ethics or jurisprudence rather than science as the primary focus of study.

Clearly, this view reflects the pancritical normativism and pragmatically-oriented philosophy of inquiry underlying my dissertation. The selective foundationalism of the logical empiricists served to exacerbate a deeply fragmented conception of reason, a conception that
has philosophical roots in the deductivist spirit of modernist views about knowledge.¹ But the reaction of historicist relativism is not what I take to be the correct consequence of a pragmatic alternative to this modernist tradition either. This radicalized version of historicism, I believe, is a reaction that is to be expected among thinkers whose positivist heritage gives them no way of explaining *ampliative discourse* as anything more than arbitrary stipulation and convention (Goodman 1991 p. 2). As it might otherwise be put, this is a shared commitment to a particular version of philosophical skepticism. The formalists and the radical historicists share a commitment to interpret ampliative of non-deductive discourse in terms of a preconceived philosophical model. The formalists' model presses ampliative discourse into a recognizable form of induction; but in so doing it conceals the shift from natural-fact-talk to norm-talk, and thereby denatures the normative nature of their own discourse. The radical historicists have far less qualms about the rhetorical nature of theoretical discourse; they take the contrast between logic and rhetoric as debunking the metascientist's claim to objectivity, and revealing it as the appropriation of language in the service of political power. But in so doing they disconnect norm-talk from natural fact talk, and lose sense of theoretical support for norm governance.

Modernism affirms in principle 'one right view' about any cognitively meaningful question, and post-modernists have accordingly come to place much more significance in cognitive diversity. But answers to the controversies concerning consensus and dissensus in science do not come to us wearing their consequences on their sleeve. The jury is going to be

¹My critique supports the work of other recent normative epistemologists outside the objective historicist camp, such as F. Will. For Will the "bifurcation of reason" is a result of our Cartesian heritage in epistemology, and the difficulty of throwing off deductivist conceptions of knowledge. The bifurcation of reasoned thought and habit in Hume is related, and shows the deep involvement of our issue of explanation of standards-in-use with the background problem of induction. For Will as for myself, the correct way to move beyond this opposition is to broaden our understanding of the induction problem to include the issue of the growth of normative competency, and to deepen our recognition of the social character of norms and the plurality of explanatory perspectives.
out for a long time regarding some of these questions. Accordingly, we must guard against an over-simple identification of non-foundationalism and pluralism about human reason with "relativism" in the sense connoting cognitive egalitarianism or a framework-imprisoned character of reason. Neither non-foundationalism nor pluralism—tenets which are common to pragmatists—are synonymous with relativism in this sense.

Here my metaphilosophical views differ significantly from those of Richard Rorty, for what I view as the appropriate conception of the "consequences of pragmatism" is pancritical non-foundationalism, not the Rortian historicist anti-foundationalism. Rorty's metaphilosophy, I have argued elsewhere, in many ways reflects the attitudes that positivists themselves had towards non-foundational discourse. The radical anti-metaphysical stance that Rorty takes, and his account of what follows from a failure of foundationalism, have very much to do with the views held by positivists. The difference as I would put it is not the skeptical attitude exhibited in Rorty's metaphilosophy, but his view of its extension—that is, his view that the epistemological privileging of norms of thought over norms of action are groundless.

There is a serious difference in the attitudes raised between the foundationalist metaphilosophies and the non-foundationalist ones. But there is also, within this latter grouping, an important difference between pancritical non-foundationalism and historicist anti-foundationalism. A rejection of selective skepticism is something that I take to be a hallmark of the pragmatist philosophy of inquiry. But Rorty's assertion that his metaphilosophy

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represents the consequences of pragmatism is not necessary. What Rorty finds important in assessing the implications of pragmatism and the end of foundations is that normative philosophy like all normative discourse involves a vicious circularity.\footnote{Rorty (1991) "Solidarity or Objectivity?", p. 28-9: "The ritual invocation of the 'need to avoid relativism' is most comprehensible as an expression of the need to preserve certain habits of contemporary [life]. ...So the real question about relativism is whether these same habits of intellectual, social, and political life can be justified by a conception of rationality as criterionless muddling through, and by a pragmatist conception of truth... I think that the answer to this question is that the pragmatist cannot justify these habits without circularity, but then neither can the realist."} The realism and relativism on this view is overcome at last when our pragmatic sensibilities show us we must capitulate to think basic circularity. Like many who identify with relativism, my attempt has been to resituate norm governance in science within a broader and more varied class of social practices. But with the objectivists I maintain the difference between instances of norm governance which are philosophical and reflective, and those which are not.

Questions of this breadth, I have argued, need to be addressed by close attention to the problems of ampliative inference and "meta-level discourse" common to all forms of inquiry. Many indications of a pancritical treatment of these issues are evident within the pragmatist tradition, as well I believe as in the Continental tradition of critical theory. Since, as Rescher (1985) points out, the taking of philosophical positions involves \textit{cognitive evaluations} on the part of reflective thinkers, philosophical attitudes are important in the way that they can pre-structure the kinds of "substantive" consequences which thinkers draw from the 'death of foundations.' Rorty's attitude is clearly seen imbedded in his grand 'Either/Or' between objectivity and solidarity, and in his skepticism towards the normative aspects of social practices —in particular towards all attempts to develop normative "criteria" of theory, method, and goal selection in science and philosophy. The consequence of the Rortian stance, as he has recently stated it himself, is to reject all normative notions of "criteria," and to view
post-foundationalist human inquiry as a "criterionless muddling through." I can find no parallel between this conception and the spirit of Dewey’s work, which, while emphasizing cognitive innovation and experimentation, focuses crucially on the manner in which real and perceived consequences of alternative choices of goals, methods, and beliefs, lead to criteria of selection among available choices.

The problem of ampliative inference is too broad for me to provide any fully articulated response in this concluding chapter. Rather, what I will attempt to provide in the last few sections is merely an overview of some of the issues towards which a "pancritical turn" in metaphilosophy invite further attention to among philosophers, historians and social theorists. Thus, this chapter represents a brief outline of a research program for those pursuing post-logicist and post-historicist methods of inquiry into normative human practices.

6.2 Pragmatism, logic and learning

Both the logicist and historicist traditions bring with them metaphilosophical or valuative orientations that, when not made explicit, represent question-begging assumptions about the appropriate philosophical treatment of norm governance. Yet both do have some basis of truth: the former is correct in insisting that cognitive evaluation in science cannot be reduced to any set of social practices, while the latter is equally correct in insisting that it cannot be properly understood independently of actual social practices and historical conditions. But notice that both these positions –themselves cognitive evaluations of the conditions of "genuine" knowledge– are philosophically compatible. Incompatibility sets into the two statements only as philosophers fail to recognize the ‘higher order’ of values and meanings.
upon which the opposition is based. It is when this occurs that this opposition of values is transfigured into a theoretical conflict modelled on the two sides of the internal/external polarity. Rendering the theoretical issues as "metaphysical" and "epistemological" issues is the usual recourse, but one may just as easily make this transfiguration by arguing with Rorty that "putting the issue...in moral and political terms, rather than in epistemological or metaphilosophical terms, makes clearer what is at stake" (Rorty, ibid). Once this transfiguration begins, however, the question-begging cannot be avoided, for the model itself is what is so philosophically tendentious.

A pragmatic account of warranting practices that avoids both objectivism and framework relativism would be one that stresses the importance of the concept of learning (Briskman 1987). A bootstrap theory, on my view, is most consistent when it advocates the revisability of postulates 'all the way down'; that is to say, when it stresses the defeasible status of posits at all levels of warranting or legitimation practices, and focuses attention as much on the possibility of improvement of ends as on the theoretical and methodological levels usually accorded the most attention. This it seems to me is most in line with Dewey's emphasis on the formation and criticism of ends, and leads to stressing the vital importance for human reasoners of their reflective choice of goals. As I believe Dewey saw, the most effective response to the classic "infinite regress" and "vicious circularity" objections (which skeptics have since antiquity brought to bear on all claims to normative objectivity) are found when the pragmatist maintains that the possibility of learning goes 'all the way down.'

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"See my "Methodological Pragmatism and the Bootstrap Theory", forthcoming The Philosophical Forum. I have chosen to leave out my discussions of the bootstrap account of warranting, in part because there appears to be widespread acceptance from pragmatists as far apart as Stich and Laudan that there is no vicious circularity involved in the bootstrap approach. See Laudan (1984), Rescher (1985), and Stich (1990) for a fuller development.
Shapere, Laudan, Burian and Rescher all argue that we ‘learn how to learn’ in the course of learning about the world. As Shapere put it, “…we learn what knowledge is as we attain knowledge, … we learn how to learn in the process of learning.” This implies that methodology can be informed by the content of scientific knowledge at a particular historical juncture. It is obvious that we must begin somewhere in our pursuit of knowledge, and that where we begin places us into a setting of historically conditioned background knowledge.

The bootstrapping approach to warranting which I advocate emphasizes that there are two extremes to avoid here: the privileging of our background knowledge as objective and indefeasible, and the denaturing of our background knowledge as merely the result of historical accident and social convention. The concept of learning, of course, is a normative concept, and so its utilization must not be allowed to pre-judge its content.

Richard Burian has commented that "One symptom of the retreat from autonomism [or logicism]...is the increasing proportion of work in the philosophy of science devoted to the philosophy of the particular sciences." Burian sees this partly as a retreat from the excesses of autonomism with its hierarchical view of science, but also as an attempt to improve the study of science, and to put philosophy back in closer interaction with actual practice and its history. In this sense it "is a sign of maturity, marking a real advance, that philosophers of science are now interested in and able to address the particular problems faced by scientists (both living and from the distant past) in ways that take account of various changes in evidential relations, observational abilities, background knowledge, theoretical languages, mathematical tools, etc" (p. 28).


6Burian (1986) "How not to talk about Conceptual Change in Science", p. 27. Burian like myself treats autonomism (logicism) and historicism as Scylla and Charybdis to be avoided in meta-methodology.
Burian insists that competing values underlie the alternative interpretations of theories and conceptual schemes which objectivists and relativists develop. The research project that he calls for is one that makes these values explicit, making "the relationship among these values, our analytical tools, and our interpretations of scientific theories...a prime target of philosophical investigation" (p. 26). With this I agree as well, but it points out, I think, that the contextualization of theory to disciplines has its limits, too. Indeed the call of contextualization is often accompanied by a deep skepticism towards any philosophy which reaches towards a general theory of human inquiry. *The relativizing of normative concerns to discipline-specific issues can, in its extreme form, reflect another version of framework relativism. It is partly this influence that has led to underestimating the extent to which problems of ethical, juridical, and scientific objectivity share a common philosophical basis.*

Burian's proposal seems to call for a middle path, one that fits well with the pragmatist tradition. Dewey, while he led philosophy toward a rejection of apriorism and toward a reunion of theory with practice, also valued highly the import of a general "theory of inquiry." While some of my concerns to develop a general philosophy of inquiry might seem out of line to thinkers already committed to a radical historicist philosophy, it is extremely important to emphasize that Dewey's pragmatism already supports the complementarity and balance between domain-specific research techniques, and general theory of inquiry.

On a related note, Laudan has recently paid attention to the extent to which framework relativists ignore connections between the problems of innovation, disagreement (dissensus), and consensus formation. Those who have championed the rejection of the unity of science hypothesis and the study of disensus, for instance, have often given only unsatisfactory 'book-learning' (Kuhn) or socio-political (Barnes and Bloor) accounts of consensus formation; it is also still widespread to speak of innovation without considering the epistemic aspects of
heuristic advice, treating it as only being explicable through recourse to the idiosyncratic and psychological considerations, or by "opportunism in context." Larry and Rachel Laudan (1989c) have suggested that philosophy of science must strive to make such interconnections clear, in order to redress reductionistic mistreatment of the issues of innovation and consensus formation.

The Laudans attempt to use the historical thesis of the "disunity of science" to solve the three problems of innovation, disagreement and consensus together. Their suggestion is that, once the foundationalist notions of unity of method and of epistemic standards are rejected, "the thesis of divergent epistemic standards within a scientific community --initially proposed...to explain the existence of long-term disagreements in science-- can be further exploited to solve the problem of innovation" (1989, p. 223-24). The "dominance" of one theory, that is, its ability to warrant high marks on a wider range of epistemic standards (than those accepted within the research tradition the new theory or hypothesis is most closely associated with), helps to explain consensus formation: "consensus about theories can occur in the presence of divergent epistemic standards, provided there is a theory which is dominant with respect to those standards. ...Scientists reach agreement about accepting a theory only if that theory is dominant over its rivals" (p. 226).

The Laudans' proposal seems admirable in its emphasis on the need to understand methodological standards --and hence choices made in relation to them-- in their own historical contexts. It thus puts the historiographer of scientific culture to the task of diachronical historiography. This clearly stands out in contrast to the radically anachronical reading of history that John Worrall engages in when defending (on a purportedly historical basis) his claim that the standards and ends of science have not changed over time. As L. Laudan pointed out in his noteworthy exchange with Worrall, the latter's commitment to the
unity of science thesis and to a foundationalism of standards and ends, leads him to some very suspect historiography. Note however that several more problems arise from the Laudans' position. With respect to theory appraisal, their hypotheses of \textit{divergent standards} and \textit{consensus by dominance} are different than Strong Programme treatment, which, "by ignoring the possibility of disparate epistemic standards in science, ...suppose(s) that —when scientists come to different judgments about theories— such differences necessarily betoken different social, economic or social interests at work" (p. 234). The Laudans' approach allows that such (sociological) factors \textit{may} play a role in science. But are the they correct in inferring that the "divergent theory appraisals \textit{per se} do not indicate the \textit{necessity} for postulating such factors"? (ibid).

Here is another aspect of the complex issues we discussed in the last chapter, and an indication of where the Laudans' thinking on these issues is positioned today. We need not delve much further here, since the line of insulation for internalist explanations seems to be drawn in much the same way that we already examined. But it should be noted that important parts of the issue seem to be obscured by the Laudans' narrow focus in this article on \textit{methodological} issues, and not also on \textit{axiological} ones. This accords with a relatively sharp distinction between methodological and axiological issues which Larry Laudan insists upon, but which M. Pera and others have objected to. On Laudan's own account, methodological rules of appraisal \textit{instrumentally} subserve theoretical goals, so that (being sure not to fall into

\footnote{This has been the focal point in the controversy between Dudley Shapere, Larry Laudan and critics such as John Worrall. See Worrall's "Fix it and be Damned: A Reply to Laudan", \textit{Brit. J. Phil. Sci.} 40 (1989), 376-88. See also Laudan's controversy with Shapere in Shapere \textit{Reason and the Search for Knowledge} (Dordrecht: Reidel Pub. Co., 1984), where it is Shapere who denies and Laudan who \textit{defends} the need for a set of quasi-foundationalist tenets.}
Kuhnian holism or co-variance assumptions about aims and methods) divergence of methodological standards is sometimes traceable to differences in long term theoretical goals.

This seems to fit the actual character of disagreement well, since much disagreement does appear to be a result of opposition at the axiological level; it is quite possible to conceive particular cases where instrumentalists and realists about the ends of science appraise two competing theories in a contrary fashion. But the analysis the Laudans give appears to place significant explanatory burden for actual and potential disagreement (both about and on the basis of methodological rules) on axiological decisions. While I tend to think this emphasis is correct, it should also be apparent that different axiological choices, that is, choice of different goals, in turn may well also be open to explanations in terms of "different social, economic or social interests at work"—indeed ends selections is standardly interpreted as among the areas most open to such analyses.

Now, as long as the radical historicist's argument leans heavily on the sheer logical possibility of cognitive diversity, it carries little weight. We can rightly insist that doubt about grounds for commensuration be translated into concrete criticism before it is taken seriously. Hence much is still accomplished by rejecting the presumed force of a prioristic arguments from the sheer possibility of diversity, or the possibility of not being able to make a non-arbitrary choice. The same may be said of the connection between these a prioristic arguments and the pernicious co-variance assumptions strong holists often make. It is only in the light of such concrete instances of competition that one can be expected to articulate what constitutes an improvement in a standard or rule. But if this is so, then epistemologists had
best become interested in the conditions under which theories, methods, aims become epistemic competitors (Briskman 1987).  

Thus there is nothing necessarily relativistic about the Laudans' historical thesis of divergence of standards. If their "objectivism" is taken only as a minimal denial of an aprioristic basis for the claims of universal cognitive egalitarianism, and if its support is based only on the claim that there is a possibility for non-arbitrary judgment between competitors at all levels of choice --theoretical, methodological, and axiological-- then objectivism is still supported. But this would appear to be a much weaker notion of objectivism than Laudan still seems to want to support in other recent writings. It must at least be concluded that the Laudans' explication of their hypothesis of divergent standards does little to dampen the focus of social epistemology onto the content of scientific belief. Indeed its effect seems to be just the opposite.

Similar questions attend the treatment of innovation. What the Laudans now claim is that "The hypothesis that divergent epistemic standards are always in place within a scientific community explains why some scientists come to utilize a theory long before many of their co-workers, and thus how innovation proceeds" (p. 26). But while different sets of standards might help explain the appeal of theories to different scientists, this is hardly a complete

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8 As Briskman put it, "learning can only take place within a context of 'evaluational' problems created by aim-pursuit and the articulation of competing theories -for it is only within such a problem-context that we need to articulate standards and rules at all." Since methodological rules are not generated in a vacuum, there is no guarantee that we will always be capable of articulating the basis for a non-arbitrary preference at some particular time \( t \). But what is important epistemologically is not that there is explicit criteria sufficient to resolve all present cases of epistemic competition, but that where genuine competition is present, the future holds the possibility that we may be able to discover further information that makes a non-arbitrary choice between these competitors possible at some point.

9 This treatment appears considerably less objectivistic than that given in earlier writings, which had focussed on the "rationality" of pursuing/accepting an out of favor or relatively less-well tested theory as question decidable by an objective, almost algorithmic standard of "rate of problem-solving success".
treatment of innovation. For even if rules are relativized to historical context, innovation is hardly to be covered by rule-following! Rule-following rationality generally involves choosing the "best supported" theory (1977), or the research tradition exemplifying the highest rate of problem-solving, but neither of these criteria capture such issues as the quintessentially abductive issue of choice of hypothesis for testing, or the decision to pursue a theory that is an underdog in terms of its record of success. Such decisions typically go beyond rule-following procedures and involve aspects of the idiosyncratic and imaginative character of particular scientists. The claim, then, that "the problem of innovation is just a special case of the problem of rational disagreement" seems somewhat hollow; the notion of rationality the Laudans here utilize appears to be limited to instances of rule-following inference, and hence to miss much of what is characteristic of instances of scientific innovation. It can explain accepting or pursuing different theories when those are the best supported according to the agent's preferred set of standards, but it cannot incorporate the kind of decisions that go beyond rule-following, and it is these latter which are typically the focus when scientists talk about abduction, originality, and creative insight. So here again the Laudans' hypothesis hardly is a sufficient solution to the problem they address, since it leads to further questions which invite if not demand supplementation by the life sciences, including cognitive development, the theory of learning, and decision theory, etc.

This of course is not to suggest treating scientific discovery and innovation in the psychologist way demanded by hypothetico-deductiveists. To some extent, the tension and balance between logic and learning is reflected also in pragmatic conceptions of the relationship between epistemic appraisal and heuristic advice. While this is not the place to address the broad issues of scientific discovery, it is clear that recent concern by philosophers of science to re-address these issues is partly explainable in terms of the effects of the
"historical turn." For the separation of two disparate contexts, "justification" and "discovery," is another aspect of the LE philosophy that was pre-structured by their commitment to a sharp logic/psychology distinction. Hence a reconstruction of scientific discovery would appear to be invited in the same way as are issues of reasoned connectedness and the tension between 'reason' and 'cause' in human affairs.

In a post logicist/historicist account of meta-methodology, much more besides warranting practices are considered important. Laudan is among those who have drawn renewed attention to the importance of heuristic advice in science. However Nickles and Sarkar, although agreeing in large measure with Laudan, both view his meta-methodology as not heuristic or forward-looking enough. The criticism comes because Laudan tends to judge heuristic promise or preliminary evaluation of a research tradition by its quantitative rate of problem-solving success. Nickles wants to bring advice into the arena of methodology. He argues that "...which methods do work better must be determined by an extensive analysis of scientific practice, from the study of human cognitive psychology and sociology, and from the economy of research."

Nickles is a normativist who attempts to forge a middle path between inductivism and historicism; but some of those who disagree with him also identify themselves as normative epistemologists. Although I want to emphasize the shared concerns of non-foundationalist

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10 The same sharp division between normative issues and psychology we saw Lakatos attempt to re-institute in the 1970s for instance, led him to treat "appraisal" as epistemic, but to deny that heuristic advice was; and this in turn led him, along with Popper, to treat heuristic advice in ways that cut it off from epistemological concerns and rendered it as part of the 'unfathomable' psychology of the individual.

11 Nickles says this quantitative approach led Laudan to "treat... all problems as difficulties which detract from, rather than add to, the merit of a research tradition. But one mark of fertility is precisely the opening up of new areas of investigation by generating new problems for research" (Nickles 1989, p. 178).

12 Nickles (1985) pp. 126. But both men recognize a need to avoid the two extremes, 1) that everything of heuristic interest is epistemic, and its contrary, 2) that no aspect of heuristics is epistemic.
normative epistemologists, a brief digression on this particular disagreement might be illuminating. Nickles’ critics generally prefer to restrict methodology to retrospective justification. The objections of these normativists, including Alvin Goldman and William Lycan, can be summarized broadly as follows: (1) Heuristic advice cannot be justified, or considered as a part of methodology, because the subject matter of methodology is epistemic standards, whereas the subject matter of heuristics is doxastic processes; (2) Questions about how to proceed on a ‘sketch’ or ‘program’ of discovery are too methodology-specific to be addressed in the kind of general theory of inquiry Nickles proposes.13

However, I view Lycan’s refusal to recognize heuristic advice as a legitimate aspect of methodology as dictated by his implicit assumption that such advice lacks the epistemic basis of justification had by methodological rules of theory-appraisal. Lycan’s argument is an essentially Kuhnian one, aimed against what he sees as a return to a ‘logic of discovery’ which was common eighteenth and nineteenth century inductivists. Yet he fails to see that his Kuhnian argument holds just as well against standards of retrospective appraisal as against what Nickles calls "generative justification." Lycan’s dismissal of Nickles’ "weakly generative justification," that is to say, appears deeply entangled with his own continued adherence to justificationist and foundationalist epistemology. The foundationalism is evident in the reductive account Lycan gives of "design stance psychology" and psycho-biology in his

13 See Alvin Goldman in defense of a thesis like (1); Goldman uses the distinction between "epistemic" and "doxastic" decision procedures to distance heuristic advice from epistemology. A normative theory of belief-warranting may be quite adequate and yet not applicable as a doxastic decision procedure; for the subject may not be able to tell from his first-person epistemic situation whether or not the conditions laid down by the normative theory are satisfied or not. William Lycan also notes this troublesome gap between normative theory and doxastic decisions and concludes that the latter cannot issue in any "recipe that real people can look up in a book and act on as practical advice in forming beliefs." Lycan says that such advice giving is "a worthy project, but one far too ambitious for me. Our set of canons is best construed, as I have meant it to be construed all along, as a normative theory of justification...and nothing more." W. Lycan (1985), "Epistemic Value", p. 141.
explanation of norms. Its justificationism is evident in the goal he sets for meta-methodology—that of developing what he calls a "fully general cognitive method" that is "topic neutral" and draws norms from purely formal and "epistemic" considerations. I think Blachowicz (1989) captures this kind of thinking and what I call the pancritical pragmatic response to it when he writes,

Discovery may involve justification, but justification certainly does not entail discovery, and it's justification that is the proper object of epistemological inquiry. In this way, the logic of discovery is dismissed, not because of the illegitimacy of any special inferential pattern it claims for itself, but because of its irrelevance. ...One might respond to this claim of irrelevance in many ways. A weak but important response is to accept both the de jure irrelevance of discovery for justification and the pre-eminence of justification in epistemology, but to stress the involvement of heuristics and questions relating to the economy of research in justification (Nickles makes this point clearly).  

14 Although I sympathize with Lycan's attempt to show the relevancy of psychobiology to the evolution of norms, I strongly disagree with his attempt "to reduce the valuative notions of epistemology to the teleological notions of the theory of organ systems." Lycan is a foundationalist in the sense that he reduces the study of norm generation and governance to "design-stance psychology." "What Mother Nature provides is good design, and its this valuative notion that is the ultimate source of our ordinary superficial valuative ideas of 'better explanation', 'rational inference', etc." Lycan (1985), pp. 150. On my view, biological "explanations" of normative commitments are not explanations for the emergence of any particular normative commitments, but do support what I have called the empirical case for the general reliability or competence of human's to perform effective norm governance. Thus the reductive character of Lycan's design-stance psychology gives a psycho-biological explanation of the development and normative force of methodological rules, but only at the cost of excluding from integration a broader range of social scientific input into the generation of the norms of scientific practices.

15 Note that the point is directed "to convince critics like Laudan of a philosophically interesting connection between the two." -Blachowicz (1989) "Discovery and Ampliative Inference", p. 451. James Blachowicz identifies a key issue of the debate between inductivists and hypothetico-deductivists as the presence and understanding of ampliative inferences. In so doing he makes important corrections to the approach pioneered by Nickles. The "theoretical turn" away from inductivist logics of discovery and towards consequentialist methodologies he sees as something of a mixed blessing. It has, to be sure, given rise to a growing appreciation of theoretical explanation in science. But he charges it has also led philosophers "to reject any account of the generation of theories from the facts (and of ampliative inference generally)..." (p. 439). As a middle ground between such consequentialism and the original spirit of 'inference from the phenomena', Blachowicz discusses those attempts at what he calls non-inductive "weakly generative" logics which embody little more than sophisticated processes of elimination. Such programs attempt to narrow the range of possible (or plausible) new hypotheses by utilizing social scientific input in order to explicate a wide variety of constraints on problem-solving.
For Nickles, a metascience responsive to scientist's needs must go beyond formal constraints and the "vapid generalizations" of justification theory. *Philosophers and sociologists alike must be concerned with the process of inquiry in its entirety, not just with its products.* Nickles is aware, as Lycan is not, that methodology does not require —indeed is better off without— a basis of content-neutrality. What Lycan misses is that Nickles' and Radnitzsky's expansion of the role of prescriptive advice goes together with a rejection of the justificationist view of rules that Lycan still adheres to. Nickles puts this point clearly in asserting that methodology involves a complex balance of general theory of inquiry and domain-specific standards: "...methodology is theory of inquiry rather than logic or epistemology in any narrow sense."16 "This means that methodology no longer will be a single, unitary subject but will, at the more interesting levels of detail, break down into domain and context-specific rules, practices, and advice."17

Nickles' approach clearly exhibits the relevance of social scientific input on norm-generation, and his non-reductionistic approach reinforces an empirical and experimental account of meta-methodology that is lost by Lycan. Judgments of the success of methods, as normative judgments, reflect a weighting-orientation of values and goals that may vary from one domain or practice to the next.

Throughout this dissertation, I have laid the grounds for arguing that moving beyond the D-N model of explanation means abandoning the dualistic classification of empirical laws, and that this in turn would imply the need to radically re-think the contrasts between the rational and the social, logic and psychology, appraisal and discovery, and even science and science

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17Nickles (1985) pp. 27. But such a fully-general cognitive method is precisely what Nickles argues we cannot have. Indeed, many of the more interesting aspects of method are domain-specific, and thereby lack the generality Lycan seeks.
policy. But of all the divisions influenced by the LE bifurcation of reason, the most important from a perspective of concern over the issues of technology and knowledge-use is the division between pure science and science policy. We must now look more closely at the roles that normative and naturalistic factors have in the explanation of human action.

6.3 The explanation of normative decisions reconsidered

My approach to norm governance in science has been one which agrees with the attempt by some social theorists like Alan Garfinkel and Frederick Will to resituate it within a broader class of social practices. "Grasp of the social character of the broad norms of thought and action that are the central objects of philosophical concern, and an appreciation of the consequences of this character ...have been among the sorest needs in modern philosophy. For the natural tendency of a modern philosophical mind and personality is individualistic" (Will 1988, p. 35). Yet many research programs deriving from Kuhn's influence have attempted to reconstrue science as a social product, and these attempts are often neither well-thought out nor consistent. I believe then that S. Shapin is correct in stating that "The mere assertion that scientific knowledge 'has to do' with the social order or that it is 'not autonomous' is no longer interesting. We must now specify how, precisely, to treat scientific culture as social product" (from Woolgar 1981, p. 366).

But Shapin's question is, of course, much easier to ask than to answer. In the last chapter we saw that the Strong Programme in the SSK is one noted tradition that claims to supply an answer. While I have spent less effort in criticizing the interest-theories of the

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18 I have spent relatively little time dispelling the errors of the Strong Programme itself, but am in agreement with those SSK advocates who are turning more towards functional and ethnomethodological approaches. One extremely damaging point against the form of interest analysis Barnes and Bloor originally suggested was that, as Yearly has pointed out, "The search for a definitive sociological explanation of belief is frustrated by the essential
sociologists, this is partly because I believe Barnes' and Bloor's original statement of the
Strong Programme does not survive intact after the years 1981-1982, in which Woolgar, 
Laudan and Yearly all published quite devastating criticisms of it.

What Woolgar objects to specifically is that the objective of revealing the social character
of the content of scientific knowledge "has been interpreted as a programme of 'specifying'
the interests which 'give rise to' scientist's actions" (Woolgar p. 388). The objections to
Barnes and Bloor's program of interest-imputation are now generally well-accepted, and there
is a highly diverse range of recently suggested successor projects in science studies. The
original formulation of the programme, if one follows some of the criticisms in the articles
mentioned above, provides no cogent answer to Shapin's challenge. Nor is there any clear
consensus on which among the multiple successor programmes provides the most promising
modification or alternative.

In my own passing comments, I have also given reason to reject the "causality" thesis,
which is associated with the claim that the SP's interest imputations represent objective causal
motivators of human action. Failure to justify this tenet leads to rejecting the programme's

flexibility of interest imputation" (p. 387). By tracking Barnes' Habermasian-influenced account of instrumental
interests, Yearly argues he is still on the horns of a dilemma: "the assumption of an authentic instrumental interest
is sociologically unrewarding...The alternative proposal, that contingent factors influence the interpretation of
instrumentality, is empirically richer. However, by stressing the variable, interpretive connection between
knowledge and interests, this proposal cuts right across the epistemological debate. Unlike the other view of the
interests, this latter programme receives no a priori support from the interest theory of knowledge—indeed it is
rather at odds with it" (p. 361).

19 "The relationship between epistemological and sociological cognitive interests: Some ambiguities underlying
the use of interest theory in the study of scientific knowledge." In discussing Barnes' version of the
underdetermination problem as it affects cognitive evaluation, Yearly writes, "the ineffably social nature of
scientific activity may mean that the interpretation of 'values' or interests in specific contexts is available for
study. But it does not prove that this interpretive work is determined by isolable social variables. Indeed, such
straightforward determination is hardly conceivable if one admits that interests (such as instrumentality) are
themselves interpretively applied to cognitive decisions" (Yearly, p. 360).
interest-imputations as any kind of 'above the battle' descriptivist project that would support its claim to be a causally-oriented 'science of science.'

Of course, I have also discussed in depth the illegitimacy of the grounds upon which both Bloor and Laudan claim their historiographical projects to have methodological primacy or exclusivity for historiographers; this strikes at the status assigned to both symmetry and asymmetry principles as the pillars of alternate philosophies of explanation.

A failure to defend the causality thesis leads directly to a weakening of the status of the symmetry thesis. This I take as further support for my claim that strong programme methodology represents a tendentious claim of methodological exclusivity or primacy, and, in effect, effects a response in kind to the "autonomy" and "primacy" claims of those whose focus was the "internal history of science." To make the parallel more explicit, the autonomy or primacy of externalist historiography (explanation in terms of specifically societal interests and the exclusion or reduction of epistemic interests) is witnessed in numerous attempts to claim that epistemic interests are "epiphenomenal" outgrowths from societal interests, and on this basis, of no causal or explanatory value.

Now, in the last chapter I argued that Bloor's challenge to Laudan's version of naturalism is quite compelling. Yet Bloor's concept of naturalism, Woolgar (1981) has pointed out effectively, is no more consistent or clear. As Woolgar put the point, Barnes' naturalism

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20 Woolgar points out that this notion of naturalism is never explicated or clearly defined. Barnes for instance writes that 'The sociologist is concerned with the naturalistic understanding of what people take to be knowledge and not with the valuative assessment of what deserves so to be taken' (1978, p. 1). But it is never clear why this particular interest among sociologists is taken to be the only one supported by naturalism, and why Barnes can claim it as the basis for the sociologist's science of science. It is just assumed to some extent that naturalism means non-teleological (1978, p. 12 and 66; see Woolgar p. 370 for discussion). At the same time, however, to harken back to Burian's thesis, Barnes conception of naturalism brings in specific value judgments that pre-structure his own account of meaning and conceptual change.
is emphatically anti-philosophical in its desire to shun the concerns of philosophy and epistemology. But at the same time it espouses an unexplicated version of scientific method in its own explanatory format. In addition, there is a very definite commitment to one particular method for apprehending and explaining the nature of the phenomenon.\(^{21}\)

One conclusion here is that the notion of naturalism has been as much a stumbling block as a common ground between diverse approaches to science studies; we must look beyond its highly ambiguous treatment on both sides of the Laudan/Bloor exchange in order to get a better grasp on how norm governance is to be "naturalistically" understood. I have argued that there is an important sense in which the sociologists win the debate over the issue of temporal or ontological primacy, forcing objectivist philosophers to abandon the focus on "internal history" insofar as that notion reflects their own insulationist epistemology. But this does not at all settle the explanatory and methodological issues, or clarify the relationship between the specifically sociological (narrowed sense) interests, and epistemic interests. What is called for, I believe, is a careful reconceptualization of the relationship between sociologically and epistemologically-couched accounts of human norm-guided behavior, one

\(^{21}\) Woolgar, p. 369. According to Woolgar, Barnes' use of 'naturalism' is carried over into the invocation of interests as a primary explanatory resource; interests, at least of a sociological type, are taken to have an unproblematic existence, and to be of causal significance. Hence the symmetry thesis asserts indicates that the explanations sought are of a causal-type, and are supplied by sociological interests imputed to the agent. Part of the unproblematic existence granted to notions like "class interest", "professional interest" etc. entails a treatment for them that differs from that for other aspects of social practices. As Woolgar points out, for Barnes "Interests are not to be treated as 'actively constructed assemblages of conventions or meaningful cultural resources, to be understood and assessed in terms of their role in activity,' even though this is precisely the formula which Barnes says has to be applied to 'all representations, pictorial or verbal, realistic or abstract,' For Barnes, knowledge products and scientific events of all kinds fall under the rubric of socially constructed representations, but interests do not" (Woolgar p. 370, citing Barnes 1978, p. 9).
that is informed both by the human sciences, and by a clearer grasp of the problem of ampliative inference.  

Such a revision entails accepting in a non-trivial way the "social" and constructive character of norms, including norms relating to cognitive claims and valuative decisions on all kinds. It also means recasting the form and focus of interest-based explanations. Russell Keat and John Urry are two that help initiate this turn when they write, "...the conceptual connection between action-descriptions and reason-descriptions does not itself constitute an objection to the causal interpretation of reason-explanations. The connection is perfectly compatible with that interpretation, can be illuminatingly accounted for by it, provided that we also have an adequate analysis of the general relations between descriptions and causal explanations" (1975). But the analysis of the relations between descriptions and causal explanation that Keat and Urry see as missing is also what Alan Garfinkel directly addressed.

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22Henderson points out that to have realistically implementable norms, we must rely on cognitive science to inform us of how far the actual scientific reasoner falls short of the ideal of rationality.

23 We can locate epistemic values as part of a broader sphere of values we may call social, but this does not lead to viewing them as secondary in importance to those that are more basic on an evolutionary model. David Henderson defends the role of epistemologically-couched accounts in the context of a socialized epistemology, arguing that there are no valid grounds either for reduction or methodological exclusion of these accounts, and indeed further that both kinds of accounts are needed in order to reach the fullest understanding of the human subject. "Properly understood, a complete sociological account...is compatible with, and even contains, an important role for epistemological aspects of scientific events" (1990, p. 113). This new conception of the separation of questions of epistemic primacy from questions of cognitive interests in explanation is best summarized in two parallel points Henderson lays out: "(Firstly) although an epistemological discussion of knowledge interests appears to support the practice of according analytical primacy to interests, considerations from the theory of knowledge do not indicate what kinds of interests should be regarded as primary. Therefore, especially when the analyst recognizes only flexible, interpretive connections between interests and knowledge, epistemological considerations provide no form of practical constraint whatsoever on the kinds of interest explanation which are given.

Secondly, even when one examines a historical instance in considerable detail, it is apparent that many different empirically plausible interests can be ascribed to protagonists and their beliefs can be related to these interests in equally numerous ways. Accordingly the interest analyst is faced with a great variety of possible knowledge-to-interest connections and interest theory provides no algorithm for deciding between them" (p. 375).
Steve Fuller's development of "social epistemology" also has considerable parallels to Garfinkel's call for developing both an epistemology and an ethics of explanation. Fuller (1989) gives place to computational analysis and cognitive psychology in their role of leading to an empirically-adequate theory of the scientific reasoner. Such a theory he sees as a needed in turn, in order for there to be social control over scientific knowledge, and the power of policy-makers to increase social control over knowledge-producing social practices and institutions.

An empirically-informed theory of the scientific reasoner is seen by Fuller as valuable not only for philosophy of science in its traditional normative concerns, but for a socially concerned image of philosophy; that is, one that contextualizes the causal and normative concerns of inquirers, but asks incessantly 'why this goal or interest, and not this other?'. These axiological decisions include the "pragmatic" ones of policy makers and political scientists — those whose decisions concern the human consequences of scientific practice and technology as social institutions. The work of Garfinkel and Fuller in the philosophy of explanation deserve some further scrutiny here, because they indicate many of the best directions in the needed revision of normative epistemology.25

24 Another serious attempt to reconceive the form and focus of interest-based explanations by David Henderson in his two articles, "The Role and Limitations of Rationalizing Explanation in the Social Sciences" (1989), and "On the Sociology of Science and the Continuing Importance of Epistemologically Couched Accounts" (1990).

25 "There is a tendency on the part of philosophers who recognize that translation is a part of a larger explanatory endeavor to conceive of explanation as basically a matter of showing the rationality of what is thought and done. This result is to be expected to the extent that the writer finds little to social scientific explanation other than translation or interpretation. In such a case 'meanings' or 'implications' come to carry the explanatory weight, and the result is a more or less implicit reliance on rationalizing explanation. It is only as one finds a substantial role for psychological or sociological theory that one can avoid unwittingly falling into making an exclusivity claim for rationalizing explanation" (Henderson, 1989, p. 270).
Garfinkel (1981) has written at length on the need for a new philosophy of explanation, one that does not feature a single formal model for all explanation, as did the D-N account.26 His central argument involves the idea that "choosing one explanatory frame over another has value presuppositions and value consequences." Garfinkel talks about the relativity of explanation to particularized problem or question situations, or more specifically, to certain "contrast spaces" presupposed in the asking of "why" and "how" questions. To be relevant and successful, this answer must assume the same context or "contrast space" that the question intends; otherwise the answer is not an answer to the same question the inquirer with. He argues that, since such contrast spaces delineate what consequences or aspects of the object of inquiry require explanation, and there is selectivity in adopting any explanatory frame, a philosophy of explanation is needed that recognizes the value presuppositions and consequences of different explanatory frames. Garfinkel attaches great importance to making the valuative presuppositions in discussions of causality and explanatory method explicit. Only by studying the shifts and dislocations in explanatory frames can we ever hope to clarify the web of explanatory frames available in the human sciences. Without this, it would be impossible to determine even when two explanations are inconsistent with each other, irrelevant to each other, complementary to each other, whether one is reducible to the other, whether one presupposes the other, and whether there are grounds for holding one is superior to the other.

Garfinkel is clear that the correct response to the situation of the valuative dimension of explanations is not to concede framework relativism, in this case the view that there is no

26 "It is something of a scandal how little attention has been paid to this need by traditional philosophy...This is not just an oversight. The philosophy of science, for the first half of this century, was dominated by logical positivism, an approach that featured a single formal model for all explanation: all explanation was seen as formal deduction of sentences from general laws" (Garfinkel, p. 18).
objectivity in deciding whether an explanation is good or whether one explanation is better than another. There is a suppressed major premise in the thought of those who make this move. That premise, of course, is that there is nothing to say about whether one value is better than another. The alternative to this that Garfinkel develops acknowledges the possibility of both an epistemology and an ethics of explanation. We can’t hide the partiality (in a double sense) of our explanations, and so should respond to the valuative selectivity that goes into the asking of the questions to which causal explanations respond. By focusing on the general and more specific human purposes in seeking an explanation, it is often possible to develop criteria for deciding upon what makes one explanation superior to another. We begin to have a new, pluralistic basis for a philosophy of explanation as soon as we begin to focus critically upon the value presuppositions of our own interests in seeking explanations.27

In his development of a pluralistic but non-relativistic account of explanatory frames, Garfinkel shows us how explanations are far more functionalistic in character than they are often portrayed to be. This fits quite well with my own intentions. There seem to be insuperable problems with frames that rely on ascribing or imputing particular contentful interests to individual agents. The content of the interests that agents actually have it seems to me must often be bracketed, both because it is as difficult to ascribe interests as to ascribe beliefs, and because the interests that agents actually have lack the discrete or particular character that many figures in both philosophy and sociology have claimed to find in them. I am not saying anything like the cynic’s statement that a man always has two reasons for a

27 Especially in the courts of law, explanations focus on things that agent’s had active control over, so that the contrast space involves decisions to act or not to act. Legal decisions often involve a general theory of public interest (p. 158). The tie in between scientific explanation and science policy also raises practical interests as desiderata of good explanations. Social theory for instance has an interest in eradicating various social conditions like poverty and unemployment; this kind of interest affects whether macro/micro, structural/individual explanations should be sought.
decision, a good reason and the real reason; I am neither assuming nor dismissing the agents own reasons. I do not think this bracketing means we must or should give up on trying to distinguish "good" reasons for a decision from the real reasons. But it does lead to talking about the diverse personal and social functions that weighted sets of interests (or even one and the same interest) can serve, and the consequences these have.

Most explanatory frames take their contrast space from different functions that interests serve, so it is best to place the functions of institutions and social systems up front. If this is correct, functionalism helps clarify the grounds of the methodological pluralism, as well as the normative force of methodological directives. If this is an appropriate response to the overemphasis on interest ascriptions by both objective historicists and, then our problem involves how to reconstruct or rehabilitate on a pluralistic basis the relationship between sociologically and epistemologically-couched accounts. On this problem I have three points to make.

First, a distinction can and should be made between actual agent-interests, and those purified virtues such as simplicity, comprehensiveness, etc. In a very real sense, I want to hold that we never have an interest in any such virtues, but that the virtues and vices are abstractions form the interests agents do have. The latter may indeed be something we often will want to bracket, given the difficulties I have already alleged in interest-ascribing explanations.

Second, there is another distinction to be kept in mind: a distinction between ascribing causal significance to (individuated) interests in doxastic decisions, and explaining preferences among reflective agents by arguing for the compatibility of the agent's decisions with preferences made on the basis of norms of evidencing. Again, the effect of the distinction is
not to divide the content of belief, but to avoid the category error of assimilating problems about doxastic decisions with normative issues of evaluation or warrant.

Third, this must be a pluralistic functionalism that avoids the metaphysical and epistemological extravagances of early twentieth-century structuralism. As I previously said, a primary reason why the "functionalist" label draws disdainful responses among social scientists today is that its previous manifestations (for instance in Merton's thought) have been deeply pre-structured by the bifurcated explanatory framework I call methodological nomotheticism.28

The pluralist about explanation looks for the most comprehensive account of human action not by finding one 'right' description, but by holding together a multiplicity of divergent methodological perspectives.29 As Collingwood suggested interpretation and explanation are based on a methodologically selective focus upon necessary conditions, and the philosophical problems are partly ones we make for ourselves by forgetting their partiality and treating them as the explanation. Seeing them in this tradition stretching from Collingwood to Garfinkel, the notions of descriptive and causal accounts of human action are themselves transformed. But this notion of holding together diverse causal stories should not be confused with the ideal of the "complete" explanation found in Hempel's account. The possibilities that emerge differ dramatically from Hempel's view, and from Mill's idea that "the real cause of the phenomenon is the assemblage of all its conditions. Garfinkel argues

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28 This is where it seems to me that Henderson's account remains inadequate. He argues against a tyranny of rationalizing explanations only by arguing the importance of "irrationalizing" explanations for a certain class of actions, and the account of the "parallel" between sociologically and epistemologically-couched accounts also still smacks of the bifurcations affected by Mertonian sociology.

29 See for instance Little (1991) Varieties of Social Explanation. Rescher sees the grounds of methodological pluralism in the different valuative perspectives that can be taken towards subject matter. The grounds for methodological pluralism is are also discussed in depth by Thomas and by Roth (1987).
that this is the archetype that becomes transformed into the logical empiricist's idea of *causal sufficiency*, and into the D-N model of explanation as deduction from empirical laws and initial conditions.\(^{30}\)

This view of explanation as selective means that various methodological programs of study tell us something about the interests of the inquirers as well as about subjects under study. But this does not mean giving up on causal concerns: the question may still be what factors tipped the balance of events so as to produce the known outcome; yet the outcome or consequences focused upon also may reflect selective interpretation. Nor does it mean that we cannot assess alternative methodological programs, or separate competing from complementary perspectives, or adjudicate among extant competitors in any particular case. And it emphatically is not license for imposing the inquirer’s metaphysics or ideology as a replacement for dispassionate empirical procedures where the latter are useful.\(^{31}\) My view licenses neither 1) the *materialist* assumption that the values accepted by social groups are of little explanatory power, or 2) the materialist assumption that explaining scientific knowledge

\(^{30}\) Garfinkel argues that the Hempelian notion of a "full explanation" is not viable or useful, since it presumes an infinitely large contrast space, so large that the questions we pose would lose all sense. The inquirers must have a less opaque focus if they are to know how to decide which necessary conditions are more important than others. Furthermore, he finds that the notion of an "empirical law" about behavior should also be contextualized. Laws speak neither of all possible objects, nor of all possible worlds; "This is especially significant in social theory, for there the laws are typically ones whose domain of validity is quite limited and whose projection across differences in time, place, culture, or social structure is at best hazardous" (p. 149).

\(^{31}\) As Thomas points out, one important question concerning methodological naturalism and anti-naturalism in the human sciences regards how far the subject matter of the human sciences is special in the sense of inviting or requiring the inquirer to employ a *theory of human nature*. I agree with him that there is no way in principle to repudiate the value of methodologies leaning on such theories, but that this is not license to the sociology of someone like C. Wright Mills. We also cannot discount Thomas’ arguments that although naturalistic methods have no *a priori* exclusive validity, the human sciences may still be more likely to make rapid progress in problem-solving by *bracketing* such issues and proceeding along more strictly "naturalistic" lines. Finally, we can attempt to naturalize the understanding of human nature itself, by separating metaphysical issues of an *essential* human nature and relying more heavily on cognitive science to provide the basis for an empirically grounded conception.
as far as possible in social terms legitimates reducing causally significant interests to ‘material’ conditions, or some other restrictive set of interests that excludes processes of ratiocinative or reflective nature (Keat, 1989).

If it is true that human interests serve a wide variety of functions for human individual and communal existence, then the task for inquirers employing interest-based methodologies is to assimilate their explanatory focus to one or another of the real functions that interests serve. Such interest explanations are causal in the rehabilitated sense suggested: there are multiple factors that we might call ‘causal,’ but they reflect in part the type of question (‘Why,’ ‘How,’ etc.) the inquirer is concerned to answer, which themselves involve the selective consequences that the inquirers is concerned to explain. As humans, we are concerned beings, as should not be less concerned about the effect of decisions on history, than on the effect of history on decisions!

At least where sociological and epistemological interests are concerned, interest imputations do not rely on empirical laws so much as on functional attributions. We may legitimately, for example, be interested in which economic, political or epistemological, etc. factors are involved in tipping the scale towards a certain behavior, without being tempted to reify these functional interests of the inquirer into sui generis or sufficient causes. The diachronical and the anachronical, the nomothetic and the ideographic, micro and macro view, symmetrical or asymmetrical: these are some of the parameters that the functionalist thinks can often be set with rational warrant once a contrast space for explanation has been chosen.

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Footnote: Fuller’s approach tend to be structural functionalist in orientation, relating the evaluation of actual agents’ cognitive performance to "the contributions that the performance makes to the overall maintenance of the knowledge system" (Fuller 1989, p. 89). Note that dysteleological study is traditionally concerned with processes that thwart the achievement of purposes, either evolutionary of human.
They are not theories we must choose between prior to settling the question of the focus of our own interests in explanation.

I believe it is the re-emergence of functionalism in a pluralistic context that has led to renewed interest on the importance of the nomothetic/ideographic distinction in discussions of historiographical methods. The new context for this distinction in historiographical inquiry appears pluralistic and sensitive to the manner in which theoretical explanatory and particularizing perspectives can be mutually-complimentary (Fuller 1989 and 1991 forthcoming). This renewed interest marks an important change from the way the distinction has been treated in the philosophy of the social sciences. Windelband’s original conception of the distinction was one between kinds of sciences, and this the pluralists find most objectionable.33

There is an important question about the distinctiveness or non-distinctiveness of the subject-matter of the human sciences. While these are not issues that I have undertaken to examine, one implication of my view should be clear. When we take the primary criteria for the settling of methodological decisions to be the inquirer’s varying focuses and interests in explanation, and not (or not merely) the nature of the subject matter under study, then the possible mutual compatibility of nomothetic and ideographic perspectives re-emerges. Clearly, this moves towards disconnecting the grounds for methodological pluralism from any necessary epistemological differences between kinds of science, or metaphysical differences in their subject matter.

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33 Manicas notes that the problem of ‘naturalism’ has been needlessly confused by failure to be clear about whether the differences between the physical and social sciences is methodological, or also epistemological and ontological (1987, p. 128).
6.4 The human reasoner and the sciences of man

In the last chapter, it became clear that where Laudan stood in 1977 on the issue of historical explanation was not far from the position of Hempel. To be sure, Laudan emphasized historical explanation, but the type of rationalizing explanations he favored in history of science was not the alternative to the D-N account that Dray sought. Rather, consistent with Hempel’s own treatment of rationalizing explanation, the attempt was to make rationalizing explanations conform to a somewhat reformed D-N format, by ascribing rationality to human agents and providing empirical law-like generalizations about the types of decisions rational agents make (Hempel 1965; Jarvie 1964).

To some extent, this attempt to make individual rationality theory subserve a covering law model of explanation still seems present in Laudan’s recent emphasis on sharply dividing the questions of "progress" and of "rationality." It is merely instrumental or means-end rationality that drives his minimalist logic of the situation. Laudan’s minimalist account of rationality is an explicitly instrumental or non-normative account; rationality is defined in terms of making utility-maximizing decisions, and the form of these instrumental decisions are thought to be capturable by objective, law-like statements. Two apparent consequences of this theory which Steve Fuller has recently drawn attention to, are 1) that its formalistic character appears to make an empirical study into the actual character and limitations of human reasoners irrelevant to philosophy of science, and 2) that it affects a ‘trade off’ between rationality and progress that, by restricting the normative dimension of science to a theory of progress, leads to ignoring many of the traditional concerns often associated with the theory.
of rationality, as well as the more specific normative concerns of Fuller’s own "social epistemology." 34

If Larry Laudan (1987) had his way, philosophers would salvage the concept of scientific progress at the expense of the concept of rationality. You might say that in this way proponents of naturalistic and proponents of normative approaches to the philosophy of science would reach a state of peaceful coexistence: the normative elements would retreat to their home turf of present-day cognitive interests, whereas the naturalist elements would be allowed free passage in providing historical episodes that, in retrospect, can be shown to have (or not have) promoted those interests. The rhetoric of reconciliation aside, Laudan is really restricting the normative dimension of the philosophy of science to the only area where it would seem to be ineliminable: namely, where it concerns our own interests in wanting to understand the history of science. These interests constitute an implicit theory of progress, which once articulated, can explain why some past theory choices appear historically significant, others less so, and still others ironically so. 35

Laudan’s account of philosophy of science as a normative enterprise has apparently led him to the position that it does not need to seriously concern itself with what Fuller calls an empirical theory of the scientific reasoner. If this is correct, an irony and self-inconsistency might be alleged. For Laudan himself championed the historicist criticism of the logicists for

34 "My overall appraisal of Laudan’s strategy, assuming that it can be carried through, is that it gives up rationality much too quickly, seeing a tradeoff between rationality and progress where none need be seen. Rather, both a theory of the historian’s reasoning (the “progress” account) and a theory of the scientist’s reasoning (the "rationality" account) are needed.... Laudan, by neatly parceling out the historian’s hindsight into a theory of "progress," rather than might less misleadingly be called a theory of "meta-rationality," tactfully sidesteps the less flattering side of his normative naturalism, namely, that the "rationality" he reserves for past scientists is nothing more than their successful pursuit of relatively immediate goals that bear only adventitiously on the overall aims of the history of science. In other words, the scientist’s adaptability to her life-situation is inevitably treated as a symptom of her historical shortsightedness." - Fuller (1989), p. 54-55.

35 Fuller, p. 53. "On a more positive note, Laudan’s efforts to distance progress from rationality perform something of a service in demystifying much of the Hegelian rhetoric associated with Lakatos’ "rational reconstruction" of the history of science. Laudan’s new project is called normative naturalism, and it makes the thrust of the Lakatosian project more evident by showing that the sort of judgments that Lakatos tried to tap by reconstructing what a scientist would have decided under ideal epistemic conditions (i.e. with regard to both evidence and cognitive aims) is best cashed out in terms of historical meta-judgments: i.e. today’s historian’s evaluation of the impact that the scientist has had in facilitating the development of current science." — Fuller (1989) p. 54-5.
thinking that because philosophy of science was seen as a *normative* enterprise, it did not have to pay serious attention to the empirical record of science—that is to say, to *history of science.* The objection then would be that the grounds on which Laudan claims empirical studies of human reasoning limitations and propensities to be merely peripheral to the normative concerns of philosophy of science, is structurally analogous to the grounds on which the logical empiricists claimed *history of science* to be peripheral.

I only raise this as a *possible* problem for Laudan, since I know of nowhere where he actually disparages the role of the cognitive psychology and sociology; rather he only appears to marginalize it in his discussions of normative concerns, through his adoption of the minimalist account of rationality with its narrowly instrumental focus. It is clear, at any rate, that Laudan’s account of normative issues does not integrate the social and biological sciences to the extent that many recent philosophers including Goldman, Giere, Stich, and Kitcher, have argued they should be integrated. With this latter group, one might contend that the objective historicist’s emphasis on ‘learning how to learn’ about the warranting of belief and about heuristic advice and effective methods, should lead to putting philosophy into closer contact with decision theory (Levi 1986), and psychological theories of cognitive development.

On another of Fuller’s objections, I think Laudan has a much better response. I can make no sense of Fuller’s claim that Laudan’s attempt to demystify the natural/normative distinction is ultimately "a mixed blessing," or that "Laudan’s revised research program gives a new sense of legitimacy to precisely those features of the Lakatosian project—its Whiggish disregard of what the actual scientists thought—features that were originally considered most

\[36\text{See Helge Kragh (1987) An Introduction to the Historiography of Science, Ch. 1 and 2 on changing attitudes of empiricists towards history of science.}\]
objectionable" (p. 55). This I think misunderstands Laudan’s revisions. Laudan’s conception of the theory of progress fits well with methodological pragmatism and does not support anachronical historiography as Fuller suggests. The problems with the division between progress and rationality, I suggest, are primarily still with the conception of rationality that stands as the backbone of Laudan’s meta-historiography. The problems point back to Laudan’s neglect of the importance of a theory of the reasoner informed by the social and biological sciences.37

Fuller’s view that rationality is a normative concept and should not be reduced to merely instrumental meaning seems appropriate to me, but his identification of a “theory of the scientific reasoner” with a “theory of meta-rationality” addressing explicitly normative issues, appears to fall back on some of the very confusions which Laudan’s division of issues was meant to avoid. It is not clear, for instance, what connection Fuller sees between a diachronical study of history, and the richly normative conception of rationality theory he sees flowing out from it. What Fuller calls the abandoned project of a theory of the scientific reasoner (complementary to a theory of the historian’s reasoning), need not be equated with a normative “theory of meta-rationality.”38 I think Laudan would be right in responding that insofar as the former is glossed as an empirical project conducted by cognitive science, this

37 Note that I don’t read the methodological implications of Laudan’s minimalist view of rationality as capable of supplying a “logic” of decision in any formal sense. As a satisficer about rationality, I view these instrumental considerations as limiting cases or mere necessary conditions for rationality; explanation of the ampliative decisions involved in actual choice needs to be supplemented both by a theory of progress and by knowledge of actual human reasoning propensities and limitations.

38 In order to switch from science evaluation to science policy, however, and thus to re-introduce Fuller’s own strongly normative project, it is important to see that cognitive science and not merely historiographical research has evidential bearing. By excluding this, Fuller argues internalist history of science “has unwittingly promoted a more diminished sense of the normative” (135).
identification appears illicit and involves Fuller in more of the problems of the "omnibus" theory of rationality than he would want to concede.\textsuperscript{39}

This does not mean that the normative questions Fuller’s social epistemology addresses are unimportant ones, as I'm sure Laudan would agree. Although the way they are glossed differs dramatically, the effect of both accounts is to throw very direct focus upon the issues of ends-choice. While Laudan discusses this mostly with respect to axiology of science, or choice of long and short term cognitive ends, there is no logical conflict as far as I can see between this focus on ends-choice and the social epistemologist’s emphasis on socially-responsible choice of goals crucial for knowledge-producing practices and institutions. But the organization of these issues, in which philosophers continue to waffle back and forth between diachronical historiography and normative concerns, remains a source of great confusion. Laudan and Fuller share a common problem, which each merely confronts quite differently: Laudan by claiming to derive a normative "theory of progress" from a naturalistic account of rationality and history, and Fuller by claiming to derive a normative "theory of meta-rationality" from a naturalistic account "theory of the scientific reasoner." In the next section I attempt to overcome some of these confusions by placing the discussion of naturalistic and normative issues on a basis significantly different from those that Laudan and Fuller respectively utilize.\textsuperscript{40} The alternative basis I will discuss involves a development of

\textsuperscript{39}As a point that could be pursued, Habermas' typology of three distinct \textit{aspects or senses} of rationality (1980) point to another way in which the importance of instrumental rationality to diachronical historiographical methods and causal explanations might still be recognized and separated, yet contextualized within a broader conception that also supports normative conceptions of rationality. Habermas contrasts four "pure cases of social action," each with a sense of rationality corresponding to it. Laudan treats Habermas' typology as just another way of glossing the omnibus theory, and refuses to speak of rationality with respect to choice of aims, but only in respect of choice of means.

\textsuperscript{40}The confusion I am attempting to take on by contrasting rationality theory with the competence/performance distinction seems to be quite apparent in Gibbard's thought: "When we settle the thing to do, then in my sense we have thereby made up our minds that it is the rational thing to do." "All norms, we might say, are primarily
the competence/performance distinction, but I want first to conclude this section (and preface the next) with a brief comment on the classical distinction between subjective and objective justification.

There is continuing importance to be attached to the classical contrast between ‘subjective’ and ‘objective’ justification in ethics and jurisprudence, as well as in epistemology.41 In some sense it is this way of dividing the natural and the normative—as old as philosophy itself—that Peirce attempted to rehabilitate through his development of the dialectical relationship between the actual, historically-situated reasoner, and the ‘ideal community of inquiry.’ But this subjective/objective distinction has never been well-understood in Peirce’s work, and raises issues that need to be explicitly addressed in the explanatory foci of contemporary sociology and philosophy.

For one thing, we now view decision-making in science as a comparative matter, and as involving circumstances imposed by conditions of uncertainty and cognitive economy (Rescher, 1989). It is natural limitations of time and resource that place inquirers initially into contexts of cognitive economy. Choose they must, in any situation of economy, so that the prioratising of both preferred means and ends is an end-product as well as a

norms of rationality. The various different kinds of norms governing a thing—moral norms, aesthetic norms, norms of propriety—are each norms for the rationality of some one kind of attitude one can have towards it. Just as moral norms are norms for the rationality of guilt and resentment, so aesthetic norms are norms for the rationality of kinds of aesthetic appreciation. Norms of propriety are norms for the rationality of shock, so that something is improper if it makes sense to be shocked by it. (A. Gibbard, Wise Choices, Apt Feelings: A Theory of Normative Judgment. Cambridge: Harvard University Press, 1990, p. 50-51.) It should be clear that I see no necessity to thinking in the way Gibbard suggests, and that I see deep-seated misconceptions in dealing with norms in this way. Norms of rationality, rather, are one among other ways of framing issues of normative performance, and it is a mistake to disconnect them from the competence/performance distinction and render them primary in the way Gibbard suggests.

41See for instance R. Feldman (1988) "Subjective and Objective Justification in Ethics and Epistemology." Feldman argues that "the similarity between ethical and epistemological justification is not as great" as some other recent writers have suggested.
presupposition of human economic and aesthetic reasoning. Normative performance standards are pertinent whenever they have appropriateness in adjudicating conflicts of interest or resolving decision-making in a context of cognitive economy.

There is also an important difference between 1) study of what past scientific reasoners actually took as evidential ground for their beliefs and decision, and 2) study of what ideally rational scientific reasoners would upon reflection (and given more knowledge or different biases) take as good reasons for their individual and collective decisions. But few of the truly interesting questions, for either sociologists or epistemologists, concern subjective and objective contexts in these standard, polarized senses. Those who study scientific culture no longer have need for ideal knowers and unreflective believers in the senses early-twentieth century philosophy manifests, and so should send these two back together to the same world of fancy from which they come. It is largely between these polarized extremes that most of the interesting questions arise. For instance, 1) the social scientific questions about what real scientific (and moral) reasoners actually do upon reflection when presented with problems requiring inference and decision, and 2) epistemological questions about what methodological and inferential strategies sound reasoners should prefer upon reflection when presented with such problems. Hence, while the dialectic of subjective and objective warranting of belief is of continuing importance, its rehabilitation in the new philosophy of explanation is essential.

6.5 Competence and performance in norm governance

Do we as philosophers set the aim of science…or do we ask scientists what their aims are? Or might we instead attribute aims to them which are consonant with their behavior?…A comprehensive naturalistic theory should at least include an account of
their behavior after reflection...The ought/is distinction can be naturalized, but its shadow will still remain, perhaps as a distinction between performance and competence.\textsuperscript{42}

Noretta Koertge asks some questions of well-directed concern for philosophers and social scientists. Her passing comment on the importance of the competence/performance distinction is one that I agree with. But the position that would appear to be indicated in her comment requires further modification. The natural and normative issues the inquirer faces are not modelable by the division between competence and performance, but are more likely to be modeled as two differentiable senses of "performance." Lets begin by investigating competence first, and follow this with a discussion of the relationship between competence assumptions and performance ascriptions.

Any critical project, however fallibilist and pragmatic, couldn't get off the ground if we're entertaining serious doubts about even the basic competence of reason to perform certain tasks associated with cognitive inquiry. The issue of what human agents can do is the issue of competence. Like anthropologists studying exotic cultures, historiographers of a normative practice are interpreters, and as such are routinely put in a position of contemplating assumptions about human competencies, such as those ascribing to agents the competence to speak 'mostly truths,' be coherent, rational, clear, etc. For without at least some such qualities, we befuddle our own ability to interpret behavior, including change of belief. It seems otherwise impossible to make sense of actual human agents as having intentionally ascribable beliefs and interests. If reasoning is a process in which beliefs are formed and modified, and beliefs must be intentionally ascribable to be of use to historiographers and anthropologists, we could not intentionally ascribe cognitive states to agents who lacked rationality altogether. Following a recent argument by Stephen Stich

(1990) to this effect, we see that the *can do* questions are part of a philosophical anthropology of man, but concern conceptual requirements of the inquirer in his relation to his focus of study, as much as the nature of the object studied.\footnote{J. Brown speaks correctly, I think, about the need to develop a philosophical anthropology of scientific culture. It was about the form of the inquiry, rather than the need to develop it, that I disagree with Brown. This is what Stich argues in his revision of the logic of belief ascription: "If, as Davidson insists, 'charity is forced on us,' this is because there is a conceptual link between being rational, on the one hand, and having 'contentful' or intentionally describable cognitive states, on the other. What we want to know is why this conceptual link occurs." -*The Fragmentation of Reason*, 1989, p. 30. Stich's argument for postulating charity principles (in terms of the need to make possible intentional ascription) seems correct, but his treatment of the content of the rationality assumption still appears ambiguous; if so, it is because he is implicitly equivocating between aspects of the problem that ought to be kept distinct: namely, *competence assumptions* as the basis for ascriptions, and normative or prescriptive *performance standards* as the basis for evaluations. Stich's account is influenced by pragmatism and by his extensive work in the cognitive sciences. Like Henderson, he disagrees with the Davidsonian/Jarvian tradition making extensive use of the notion of 'ideal rationality.' This use he thinks is not forced on us as a condition of having a workable theory of translation. He says that, like Quine, he is "notably noncommittal on the question of how much rationality is required for intentional description," or how far a person can get "from perfect rationality before he no longer counts as having any beliefs at all" (ibid).} 

We must, as Laudan insists, protect against the Lakatosian confusion of anachronistic evaluations with actual history, a confusion that results in the methodological directive that historical agents should be interpreted as suffering from a high degree the 'false consciousness' about the basis of their own decisions. But we must also protect against other accounts which make intentional ascription a matter of merely looking, or merely asking for and uncritically accepting the agent's own stated reasons for their actions. We can formalize some of this by saying that there again are extremes to be avoided in this aspect of the interpreter's problem: 1) routinely doubting or ignoring the agent's professed reason for his or her own action (and overriding them with intentional ascriptions of our own making) (*behavioral reductionism*), and 2) routinely and uncritically accepting a) the agent's own account, or b) behavioristic ascriptions purported to be consonant with behavior (*behavioral descriptivism*).
Those who utilize the competence/performance distinction to effect a middle ground between these methodological extremes typically affirm the important role of intentional ascriptions. But they also treat ascriptions of human competencies as working hypotheses or "charity principles." This is to side with critics like Stich and David Henderson in criticism of the Jarvie/Davidson treatment of charity principles. Can do issues have a profound effect on the methodological assumptions inquirers make, and help them to focus attention on an aspect of culture that can be approached empirically. We may come to deny the rationality, coherence, etc. of a particular historical agent or his discursive texts, but if we have granted the charity principles in good faith, we will do so only at the end of extensive empirical investigation. The same point might be made by stressing that the decision to start out by assuming scientists and other historical figures are typically rational is a methodological one; the primary role of such competency-ascriptions is simply that of heuristic devices that get empirical/historiographical research off the ground. In this role, they are not conceptual truths or principles that have legitimacy apart from the role they play in facilitating explanation from particular methodological perspectives.

Clarifying this methodological status is crucial to our understanding of the status of normative standards, including those of reflective reasoning. Questions about what people "actually do" and "should do" are both subservient to what humans "can do." Causal descriptions and prescriptive evaluations of human action are both aspects of performance, as I understand them. Were the assumptions of clarity, rationality, and the like viewed as empirical or conceptual truths derivable analytically (for example, from the definition of man as a "rational animal") or empirically (inductively), the status of performance standards would

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become problematic, if not completely mystified. How then could an inquirer, in a particular historical or social scientific inquiry, ever recognize any exceptions to the rule?

Now, postulational techniques serve important functions for a wide variety of normative practices. Their useful functions furthermore include those that they serve in relation to valuative standards, and not merely to empirical hypotheses. In the social sciences, for example, methodological presumptions concern which direction error should take where conditions of uncertainty and potentiality for error are facts of life for interpreters of a culture. This presumptive or stipulational character of methodological directives (in the interpretation of human action) is contextualized by the interests in explanation which the inquirer has. But within this context they help provide a basis for non-arbitrary preferences. In a recent symposium "On Norms in Moral and Social Theory" published in Ethics, Edna Ullmann-Margalit develops this role of "propositional presumptions" and the valuative contexts in which they arise.

In the legal theory of evidence the notion of presumption of law plays an important role. The presumption of law is propositional: it is a presumption that such and such is the case. For example, there is a presumption that a person who, without reasonable explanation, has not been heard from for at least seven years is dead. ... A central thesis is that presumptions operate as corrective devices which regulate in advance the direction of error, where error are believed to be inevitable. A presumption, on this view, reflects a social decision as to which sort of error is least acceptable on grounds of moral values and social attitudes and goals.... And since the presumptive rule takes one of two possible alternative forms, the choice between the pair of contrary presumptions must be based, among other things, on considerations of values, goals, and attitudes. 45

Clearly, "actually do" and "should do" questions impinge over issues of what direction error of interpretation should take if error is unavoidable. Treating the ascription of traits to historical agents as "propositional presumptions" avoids the dogma of attaching necessity to them as essential characteristics of the human reasoner. Postulated in this way and explicitly stated, such decisions are oftentimes shared by inquirers coming from diverse methodological perspectives. I think much of what I am saying is already accepted thought in social theory. The need to avoid behavioral reductionism means the inquirers must not give up trying to distinguish "actually do" and "should do" questions. But the need to avoid behavioral descriptivism keeps inquirers aware of the valuative assumptions embedded in the particular explanatory frameworks they select. Both the epistemologist and the sociologist of science want—indeed depend upon—distinguishing between deciding whether the reasons proffered by agents are the causes of their decisions, and whether these constitute good or bad reasons for the decisions (Keat and Urry, p. 205). Once this shared assumption is recognized, the philosopher and sociologist should be able to agree on the more specific forms of performance to be contrasted.

The basic competence assumption that concerns me is the same which Frederick Will (1988) has discussed at length, competence in "norm governance." This is a competence to effectively govern our own standard-choosing and standard-revising activities in any viable normative practice. The skeptical or relativist challenge, as Will sees it, is a challenge to human "inductive competence," the competence to effectively govern man's own normative practices. In everyday life, there is no sharp dichotomy to be drawn between human cognitive development and normative learning; we take this competence as a given, and rarely

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46 See Will p. 136 on the need to integrate the relation between 1) patterns of action or procedure, and 2) criticism applied to these.
think to question the concepts of progress and learning that have their source in this human capacity. We might question one or another particular definition of progress or account of learning, but only on the basis of a debilitating skepticism would we doubt altogether the human competence for learning.

Empirical evidence of such a competence in humans is found at a basic biological level in our species' survival skills, and more importantly by our unique ability to have developed beyond direct genetic coding of behavior by our genes. Our ways of judging between sound and faulty warranting of beliefs go back to our basic inductive capacity, and to practical standards of success in life's everyday undertakings. Those critics are mistaken who try to claim that standards of success of radically theory-dependent and not tied to species-shared interests. There is, as Rescher and a long tradition of pragmatic thinkers have maintained, a strong connection between theoretical problems and problems of practice, between judgments of theoretical success and judgments of practical problem-solving. But even the empirical evidence for competence in norm governance, rooted in basic pragmatic problem-solving and our ability to survive biologically, is not enough to lead us to viewing this competence in norm governance as a universal truth about mankind. The status of the assumption of "governance-competence" is that of a kind of basic charity principle that makes possible the interpretation of human actions and decisions that are shaped through or respond to norms.

This conception of the relationship between performance ascriptions and competence assumptions agrees with Shapere's statement, "We may arrive at a stage where there is no reason to doubt that the concepts used are perfectly adequate for dealing with the world (or our communicative goals): but neither the necessity nor the impossibility of arriving at such a

47Rescher, Methodological Pragmatism, p. 89-90. "...it is effectively impossible that success should crown the products of systematically error-producing cognitive procedures...Inquiry procedures which systematically underwrite success-conducive theses thus deserve to be credited with a significant measure of rational warrant."
stage can be established in advance" (Shapere, 1984, p. 220). This is in no way debilitating for epistemologists, since it allows a weak evidential import from case study (reconstructions) to the choice between normative meta-methodologies (research traditions, etc.). *I see no reason why this evidence must come only from diachronical historiography, since Sarkar has discussed how hypothetical or future-oriented case study might also be of benefit.*

If competence assumptions involve issues which are primarily conceptual, the divisions within performance are those which involve primarily naturalistic and normative aspects of the inquirers' relationship with their objects of study. It would be counter-productive to hang onto the division between causal and evaluative judgment to represent this new account, but that tension certainly remains. What the Greeks called *aitia* were the conditions responsible for the occurrence of something. It is curious that aetiology, the study of types of explanations, got off to a pluralistic start in western philosophy (through Aristotle's influential fourfold typology of causes), while axiology did not. Perhaps this was a consequence of a reductionism or hierarchical model Aristotle held of "final causes," that militated against pluralism. However it may have been in its philosophical roots, the D-N model of explanation popular among 20th-century philosophers has worked to sunder the connection between an inquirer's interests in explanation, and the ascriptive and explanatory methods that inquirer employs. What Fuller suggests as a new focus for research is what he calls "axioaetiotics," the study of the values-causes nexus. Of course, this focus only seems possible at all only if one retains the possibility of distinguishing between "actually do" and "should do" issues. Fuller's analysis also begins from the shared tenet of epistemologists and sociologists of science. Fuller believes that the "normative naturalists" like Laudan, Goldman and Giere are speaking to this issue, but that they and others need to raise the issue of the relationship of values and causes to self-reflectiveness in order to adequately surmount the
tension between the normative and naturalistic as it affects our conceptions of methodology and explanation in the human sciences.

Historiographers utilizing symmetrical perspectives can also take solace in the competence/performance model, because it wears the valuative character of performance standards on its sleeve, and leaves the emergence and change of particular performance standards open to sociological as well as philosophical investigation. *The drawing of "parameters of belief" can be seen quite clearly in the context of the competence\performance model to involve conceptual commitments.* Bracketing certain of these sets the stage for "impartial" explanations which are indifferent to the kind of normative performance standards that concern epistemologists.

This is enough to indicate why I take the competence\performance distinction to be of highest potential as a vehicle for putting to rest long-standing confusions about the relationships between naturalistic and normative aspects of methodology. The model it provides is only that—a model—and cannot legislate what the difference between a natural and normative consideration is, but only help distinguish them in context. But I suspect that this model reflects a possible common ground for philosophers, sociologists and historians in discussions of methodology. My suggestion is for historiographers, whether pursuing symmetrical or asymmetrical programs, to develop their methods and explanatory focus in a manner consistent with this model.

A reconstruction of the relationship of the natural and the normative dimensions of the human sciences may—hopefully—even lead to profound implications in the practical sphere. The values we hold dear in the real world of social and political action are not, as some have thought, indifferent to the understanding we have of the philosophical grounds for our judgments of value. The sharp contrasts between logic and psychology, and between the
rational and the social have constricted theoretical thought about science and technology. But the oversharp contrast between science and science policy has been far more damaging in practical terms. In this sense, the reconstruction I have urged at the metaphilosophical level of discourse may serve a more urgent social need for broad-based understanding among concerned human agents of the power and authority that institutionally adopted norms have. These include especially the human consequences of normative decision-making concerning science and technology. For we live in a world in which technical knowledge and instrumental skills have developed at an unprecedented rate, but in which both the Socratic or practical wisdom and institutional support for controlling this knowledge remains grievously -- and perhaps even lethally to the human race and the planet-- underdeveloped. The consequence of this work --to readdress Fuller's call for a return of philosophy to social relevance-- should be that of leading back to a propitious and expectant conception of the intellectual support for proposed revisions in the norms of our knowledge-producing social practices and institutions. Here it seems to me is the practical imperative for the adoption of the pancritical attitude or orientation. The relativism that is popular today is ill-suited to address the pressing social issues of knowledge-production and control, and must give way to a more critical attitude if there is to be any significant improvement in the social control of science and technology.

To conclude, the displacement of logicist with historicist conceptions of meta-methodology has not been highly progressive, in large part because there remain so many of the logicist's problematic skepticism in the main branches of the historicist school. A pancritical turn that goes beyond the limitations of historicism and logicism would reconceive the balance between knowledge and skepticism through the development of bootstrapping methods of legitimation, criticism, and improvement of norms. For theoretical science, this
means moving beyond what I hope to have persuaded the reader is an unhealthy polarity of objectivism and relativism, a seeking a reunion of the 'logic' of science with the learning that has gone on in history. I close then in agreement with Keat that there is something that must be cultivated in the partial truths of both historicism and logicism if this unhealthy polarity is to be surmounted:

In the end, then, historicism and logicism may still converge. We may still be able to construct a philosophy of science that derives both from the learning that has gone on in history and from a more general logical and epistemological framework. Logic or epistemology of themselves would never have had the resources to come up with anything like the complex and pluralistic scientific methodology that has characterized the natural sciences in recent centuries. On the other hand, that methodology is not just a contingent empirical fact. 48

48 Keat p. 52.


263


265


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