The Complementarity of Dogmatism and Criticism and Its Function Within Tradition and Revolution: A Debate between Kuhn, Popper, Gadamer, and Blumenberg

Mark Tazelaar
Loyola University Chicago

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THE COMPLEMENTARITY OF DOGMATISM AND CRITICISM AND ITS FUNCTION WITHIN TRADITION AND REVOLUTION: A DEBATE BETWEEN KUHN, POPPER, GADAMER, AND BLUMENBERG

A DISSERTATION SUBMITTED TO THE FACULTY OF THE GRADUATE SCHOOL IN CANDIDACY FOR THE DEGREE OF DOCTOR OF PHILOSOPHY DEPARTMENT OF PHILOSOPHY

BY

MARK TAZELAAR

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CHAPTER ONE
INTRODUCTION

Thomas Kuhn is renowned for having affirmed both dogmatic and revolutionary aspects of scientific inquiry. His is a difficult--and some would say even incoherent--mix, however. Among the many central achievements of science and scientific method that Kuhn's mix purportedly threatens to undermine, few are as dear as the idea of progress. The questions which get presented to Kuhn are typically along the lines of the following: How can dogmatism open itself to the kind of revolutionary critique which claims to comprise science's most dramatic advances? And yet, how can revolutionary critique measure the success it claims, if not against a frame of reference which can bridge the gap opened by the revolutionary break, and thereby offer points of comparison?

The treatment of Kuhn's philosophy of science that will be presented in the first chapter attempts to correct certain misconceptions of his position, and prepares the way for a clearer statement of the kind of complementarity between dogmatism and revolution that can answer the two questions just stated above. Ultimately, for Kuhn--and as we shall see, for Blumenberg--the affirmation of progress requires an affirmation of complementarity between
revolutionary breaks and tradition continuity.

We will see that, for Kuhn, the dogmatic side of science--normal science--is not inert. Its offer of stability incorporates its own distinctive dynamic. Normal science is not, therefore, a stable, entrenched mass consistently eroded under the pressure of falsification or falsifying criticism. It is not simply a target for criticism.

For Kuhn, all of science proceeds by means of a logic of provocation and response, and it is within the terms of this logic that the nature and function of normal science should be understood. The adoption of a paradigm is simultaneously the displacement of another. But this choice is not simply according to falsificationist standards, but also according to the concrete standards imposed by each paradigm. Many commentators, as we will see, have missed the functional role Kuhn assigns to the previous paradigm both in provoking the emergence of a new paradigm, and in setting specific standards for what can qualify as a new paradigm.

In chapter two we will learn that Popper, against whom Kuhn may seem to be defining his position, also recognizes the dogmatism Kuhn has identified. Insofar as he does, he too must provide an account of the complementarity between dogmatism and criticism.

Popper interprets the role of dogmatism in inquiry as
helping to identify where the power of our accepted theories lie. The importance of this identification is that it allows for an assessment of the progress achieved through revolutions. Popper therefore does affirm that criticism remains accountable to the framework it would reject. However, as we will see, this very accountability raises serious questions about his beliefs in the steady availability of "framework-breaking" or "revolutions in permanence."

In chapter three we will see how the hermeneutic philosophy of Hans Georg Gadamer clarifies and develops the issue of the complementarity between dogmatism and criticism by means of his rehabilitation of the idea of tradition. Specifically, Gadamer's hermeneutics will allow us to see how a "framework" is not simply an obstacle to knowledge, a negative condition restricting our gaze, but is also a positive, enabling condition which can open us to, and serve as a medium for, new experiences--experiences which both falsify and confirm various aspects of our framework (tradition).

Gadamer's hermeneutics, however, for all its virtues in support of Kuhn's struggle against Popper's imperative for permanent revolution, ultimately fails to provide Kuhn with a viable concept of revolution. At best, Gadamer's understanding of the complementarity of dogmatism and criticism contextualizes that complementarity within a
reform-oriented (not a revolution-oriented) tradition. But Kuhn himself deploys the concept of revolution--and decidedly not the concept of reform.

In chapter four I will argue that the work of Hans Blumenberg underwrites the requisite notion of revolution that Kuhn requires. Blumenberg's work, I will suggest, in some senses builds upon the hermeneutics of Gadamer. Specifically, I will argue that Blumenberg's descriptions of a revolutionary text not only assist Kuhn, but develop the idea of the "classical" that has been central to Gadamer's work, but which has been subjected to much criticism for its conservative, and perhaps even reactionary and ideological implications.

Blumenberg will affirm both the logic of provocation and response (Kuhn), and the logic of question and answer (Gadamer), but develop these in important ways. Specifically, he will differentiate these logics (or, rather, this logic, since it is actually one) into two components: the concept of reoccupation, and the idea that the history of what leads up to an event conditions the history of that event's effects. By means of this differentiation, Blumenberg will both affirm the possibility of revolution as well as the requisite stable background against which any change must be measured. In short, he will establish his own complementarity between dogmatism and criticism.
Blumenberg's fundamental claim is that revolutions, as pathways out of aporetic situations (crisis), do not require the shattering of the identity of the dynamic which gave rise to the crisis situation—they do not, that is, require the shattering of the framework of answer positions to pressing questions. This does not mean that the questions (much less the answers) remain the same. Instead, new determinations of a question are possible by means of revolutionary answers (exemplary problem solutions, in Kuhn's terms). However—and this is crucial to Blumenberg's affirmation of continuity—such novel determinations of the questions often conceal the fact that these answers reoccupy old question positions. Revolutions, then, for Blumenberg (and for Kuhn), succeed when they reoccupy the positions of the old framework—not reconfirm them, as Gadamer would have it, nor cancel them, as Popper would.

Finally, I will argue in the conclusion that Blumenberg's concept of the reoccupation of the framework of answer positions satisfies the conditions for continuity against which progress can be identified.
CHAPTER TWO
REVOLUTIONARY SCIENCE AND ITS PROBLEMS

The focus of this chapter is a critical examination of the innovation most generally recognized as a significant contribution by Kuhn to the philosophy of science: the idea of normal science. For Kuhn, the rationality of scientific revolutions can only be explained on the basis of the dynamic represented by normal science. The idea of normal science, I will argue, permits Kuhn to modify the idea of "crisis"--the context within which revolutions take place. This crisis-context is not simply a free forum, without operative standards, within which incommensurable paradigms fight for the right to impose their own standards and compete for hegemony, nor is it a free-for-all in which isolated paradigms fail to communicate with another at the same time that they compete for attention.\footnote{Exactly what Kuhn might mean by "paradigm" has been the subject of considerable discussion. See, for example, Masterman (1970). By "paradigm" I will generally mean that sense specified by Kuhn according to which certain problem solutions serve a global normative status (vs. a merely local status as the particular solution they are), and become exemplary guides for research. It has been suggested by Thomas Nickles that this sense is Kuhn's contribution to the resolution of the problem of underdetermination (Callebaut 1993:52-3).} Rather, the crisis state already emerges with preliminarily operative
standards borne by the previous dominant paradigm, which establish burdens for competing paradigms to meet.

The chapter consists of three sections. The first section is primarily expository, although its purpose is to identify precisely whom Kuhn is attacking, since this is not as obvious as one might expect. For example, Kuhn's writing style creates ambiguities in the presentation of his thought, which makes the identification of what is new within it more challenging than it need be. His positions, for instance, sometimes appear to be more radical or innovative than they actually are. His characteristic 'mincing' of words, on the other hand, makes him appear more tentative. My own interpretation of Kuhn understands him to be considerably more conservative than his initial reception, and some subsequent ones as well, indicated. Many of Kuhn's more 'radical' statements about scientific method, I will show, appear to be such only because of the infallibilist versions of method against which he projects his own views. Does Kuhn take seriously the fallibilist accounts of scientific method that were already active when he wrote *The Structure of Scientific Revolutions*?^2^ This is the question which concludes the first section of the chapter. It will be treated at length only in the second chapter.

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^2^ For example, those of Popper, Lakatos, and Quine—-and, even, those of Peirce and Dewey.
The second section of the chapter pits Kuhn against Larry Laudan, who clearly believes that Kuhn's position falls into relativism. Against that position, Laudan offers his own problem-solving approach. Although I think Laudan's position misses the mark, I am not setting him up as a straw man. Rather, Laudan's misunderstanding of Kuhn's position provides the transition to an account of Kuhn's modification of the idea of crisis—a modification Laudan clearly misses.

The final section of the chapter explores in detail Kuhn's conception of the dynamic of normal science. The general purpose of this section is to highlight precisely what is at stake in Kuhn's rejection of Popper's conjecture & refutation approach to scientific activity. But more specifically, I will attempt to refute those interpretations of Kuhn which understand him to be positing paradigms as radically discontinuous, isolated monads, spontaneously and arbitrarily generated, and having no standards to share among others.\(^3\) I will argue that Kuhn rather explicitly makes it clear that paradigms arise in response to the expectations, achievements, and failures of the old paradigm. This will become important in chapters three and four of the dissertation, insofar the imperative--

\(^3\) Hans Blumenberg has himself interpreted Kuhn in this way, and, in so doing, overlooked one of the most promising areas of dialogue between them (Blumenberg 1983:465—I will provide a full citation of this passage later in this chapter. See also Blumenberg 1987:512). The development of this dialogue is one of the major aims of this paper.
affirmed by Gadamer and developed by Blumenberg—which instructs us to make the other's position as strong as possible is identified as being at the heart of the dynamic which generates and underwrites scientific revolutions.

**Whom is Kuhn Attacking?**

The groundwork for understanding what is unique to Kuhn's position can be prepared by examining the function played by anomaly in paradigm evaluation. Kuhn is "empiricist" enough to grant that a crucial factor in evaluating a paradigm is the existence of anomalies within it. For Kuhn, an anomaly is the violation of a paradigm-induced expectation (Kuhn 1970a:52-3). Kuhn states, "Insecurity is generated by the persistent failure of the puzzles of normal science to come out as they should" (68). Soon after, he expands on this "insecurity" by introducing the idea of "crisis"—"When....an anomaly comes to seem more than just another puzzle of normal science, the transition to crisis and to extraordinary science has begun" (82). Anomalies--violations of expectations--are therefore, first, a source of insecurity, and second, may become a source of crisis. Kuhn is not altogether clear when this transition is made, although it is clear that the transition can be made for different reasons (82). For example, a crisis state, he says, "is not, let us be clear, a response called forth by any and every anomaly.... experience has repeatedly shown that, in overwhelming proportion, these discrepancies
disappear upon closer scrutiny" (Kuhn, 1977:202). And again, "few anomalies resist persistent effort for very long" (203). He continues, however, "it may resist, and if it does, we may have the beginning of a 'crisis' or 'abnormal situation' affecting those in whose usual area of research the continuing discrepancy lies" (203).

Although Kuhn makes his point using terms that have special significance for him, his general point is not unique: although it is difficult to pin down precisely when an anomaly becomes threatening, it is clear that there is "no fundamental theoretical innovation in natural science whose enunciation has not been preceded by clear recognition....that something was the matter with the theory then in vogue" (206).

When an anomaly becomes more than a puzzle for normal science, the transition to crisis has begun (1970a:82). What happens in this transition is that a paradigm, in the course of its articulation by means of puzzle-solving, undergoes a proliferation of articulations in the face of persistent anomalies, manifesting a breakdown in the consensus over what exactly the paradigm is (83). This proliferation, together with the crisis that ensues, provide sufficient conditions for the emergence of a new paradigm (80, 84). However, neither the anomalies nor the crisis will lead the scientist to actually abandon a theory "until another one is suggested to replace it (Kuhn, 1977:211)."
Kuhn here reiterates claims made early in *Structure* that rejection of a paradigm is always based upon more than comparison of the paradigm with the world. The initial acceptance of a paradigm is always contextualized within a competition between paradigms. With this assertion, Kuhn directly broaches the problem of paradigm choice.

However, Kuhn is immediately faced with a problem. As he states:

"Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue.....The man who premises a paradigm when arguing in its defence can nonetheless provide a clear exhibit of what scientific practice will be like for those who adopt the new view of nature. That exhibit can be immensely persuasive, often compellingly so. Yet, whatever its force, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically for those who refuse to step into the circle. The premises and values shared by the two parties to a debate over paradigms are not sufficiently extensive for that. As in political revolutions, so in paradigm choice--there is no standard higher than the assent of the relevant community. (1970a:94)"

If comparison to the world is a necessary but not the lone or sufficient standard by which one judges the acceptability of a paradigm, but requires in addition a competition between and comparison of paradigmatic alternatives, then some standard for judging the competition is in order--what Kuhn here calls "evaluative procedures." But Kuhn says that the argument in defence of a paradigm cannot compel someone,
on the basis of logical necessity, to accept that paradigm, although the exhibit of what scientific practice will be like for those who adopt the new view can often be compellingly persuasive. In other words, evaluative procedures that would be sufficient to compel a "logically necessary" choice are, at best, internal to a paradigm; premises and values which are shared externally are insufficient to compel such a "logical" choice between paradigms.

The position at which Kuhn is directing his attack is an interpretation of the history of science, according to which scientific problems have been solved individually in a cumulative progression, so that scientific consensus concerning an acceptable solution has been relatively quick in coming.\(^4\) Kuhn acknowledges the intuition behind this interpretation when he states that his own position raises "the question of why, in the absence of binding criteria for scientific choice, both the number of solved scientific problems and the precision of individual problem solutions should increase so markedly with the passage of time" (Kuhn, 1977:320). But Kuhn attempts to answer this question by first posing one of his own, and takes as his point of

\(^4\) In Kuhn's mind proponents of the idea of cumulative progress might include someone like Popper. See, for example, Popper's statements in The Myth of the Framework (1994), p. 103. For a contrary view, however, see Lakatos (1970:92). In any case, it is important to note that Kuhn explicitly identifies the textbooks of science as perhaps the most practically effective proponent of the view.
departure for answering the initial question his answer to this question.

What...are the characteristics of a good scientific theory?... First, a theory should be accurate.... Second, a theory should be consistent.... Third, it should have broad scope.... Fourth, it should be simple.... Fifth, a theory should be fruitful of new research findings.... Together with others of much the same sort, they provide the shared basis for theory choice. (Kuhn, 1977:321-2)

This list comprises a fairly traditional list of the criteria used in evaluating theories, and as such provide no real source of contention. Kuhn, however, adds the crucial proviso that "individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases (322)." One historical example he provides concerns the acceptance of Copernican theory: the consistency criterion would have required an unequivocal defence of the geocentric tradition. The upshot of the answer to the question concerning the characteristics of a good scientific theory is that, although all scientists might agree on the list of criteria (and even that is doubtful, unless the list is kept very short), still the relative weights assigned to the criteria might differ from individual to individual, as may the application of those criteria in concrete cases (335).

This argument provides one element in Kuhn's answer to the question why the number and precision of scientific solutions increase. But the whole explanation requires more than reliance on the list of criteria characterizing a good
scientific theory.

For that purpose one must go beyond the list of shared criteria to characteristics of the individuals who make the choice. One must, that is, deal with characteristics which vary from one scientist to another without thereby in the least jeopardizing their adherence to the canons that make science scientific. Though such canons do exist and should be discoverable, they are not by themselves sufficient to determine the decisions of individual scientists. For that purpose the shared canons must be fleshed out in ways that differ from one individual to another (my stress). (1977:324-5)

Since the set of criteria is itself too indeterminate and variable to sufficiently dictate a logically compelling choice, and therefore is no algorithm at all, one must turn to the historical record of scientific activity to determine how the criteria were appropriated, interpreted, and applied in particular cases. What is more, since the set of criteria is insufficient, one must, in turning to history, uncover the "individual factors" that have--together with the criteria--determined the choice of one paradigm over another. Affirming this "mixture" of objective and subjective factors is, by his own account, among the more revolutionary features of Kuhn's redescription of the image of science (1977:325). His reasons for saying this are perhaps due to the protracted debate about his version of

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Perhaps not simply most revolutionary, but as such also most effective. Despite his own reservations about their work, Kuhn has been a major influence upon the sociology of science, particularly the Edinburgh School, represented by Barnes and Bloor. Others within his scope of influence--and yet with whom he would seriously disagree--have been Joseph Rouse and Steve Fuller, to name just a few.
this mix. It is possible, however, that the debate is due more to the ambiguities in his position than to the revolutionary features of it.\textsuperscript{6} So, for example, Kuhn stated in the revised version of *SSR* (1970),

Nothing about that relatively familiar thesis [that theory choice is not simply a matter of deductive proof] implies either that there are no good reasons for being persuaded or that those reasons are not ultimately decisive for the group. Nor does it even imply that the reasons for choice are different from those usually listed by philosophers of science: accuracy, simplicity, fruitfulness, and the like. What it should suggest, however, is that such reasons function as values and that they can thus be differently applied, individually and collectively, by men who concur in honoring them. If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and, say scope in reaching a choice, neither can be convicted of a mistake. Nor is either being unscientific. There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision. (1970a:199-200)

Since the time he made this statement Kuhn has been at pains to stress that his identification of subjective factors in theory-choice should not be understood in a way that opposes "subjective" to "judgmental," such that the former would mean: factors which could not be subjected to dialogue (1977:337). As he states, there are good reasons for a decision, and these reasons may be no different than those already identified by philosophers of science. Kuhn only

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claims that these reasons, or criteria, must not be understood to function as "rules" which can be algorithmically applied in the self-same way. Their application, therefore, does not "prove" the judgment or choice being made. Rather, these criteria are values which can only "influence" the choice of one paradigm over another (331). They are ways of establishing and distributing burdens of proof. As values, their application may be ambiguous, but not without reason, and certainly not on that account arbitrary or irrational (Kuhn 1970b:262).

One might wonder here precisely how far Kuhn intends to depart from fallibilist understandings of scientific method. Kuhn is not clear about whom he has in mind when he speaks of a neutral algorithm that is logically compelling. If Kuhn is disparaging only those for whom a scientific method could be infallibly applied, then this criticism leaves Popper, Lakatos, Quine, (etc.) unscathed. For example, Kuhn's antagonists are precisely not the thinkers just mentioned when he states the following:

Before the group accepts it, a new theory has been tested over time by the research of a number of men, some working within it, others within its traditional rival. Such a mode of development, however, requires a decision process which permits rational men to disagree, and such disagreement

7 Once again, Popper explicitly denies that he is an infallibilist (Popper 1974:28). Perhaps equally important is a point I will develop in the next chapter: For Popper there is room for debate in science--its activities do not simply consist of conjectures and the testing of conjectures.
would be barred by the shared algorithm which philosophers have generally sought. If it were at hand, all conforming scientists would make the same decision at the same time. With standards for acceptance set too low, they would move from one attractive global viewpoint to another, never giving traditional theory an opportunity to supply equivalent attractions. With standards set higher, no one satisfying the criterion of rationality would be inclined to try out the new theory, to articulate it in ways which showed its fruitfulness or displayed its accuracy and scope.... What from one viewpoint may seem the looseness and imperfection of choice criteria conceived as rules may, when the same criteria are seen as values, appear as indispensable means of spreading the risk which the introduction of support of novelty always entails. (1977:322)

By means of this 'defense' Kuhn claims that his position concerning the status of these criteria as values, far from leading to the disintegration of the scientific enterprise, actually guarantees the viability of scientific activity. Given that new theories have generally been accepted only after considerable lengths of time and testing, Kuhn argues that this state of affairs should not be cause for concern. Kuhn in fact is arguing for an understanding of the decision-making procedures of science which does justice to the history of scientific activity--that is, one which does not turn the majority of our scientific ancestors into less rational creatures than we claim ourselves to be. There is no need to presuppose the existence of a neutral, permanent and objective algorithm for paradigm choice in order to make scientific activity appear rational. In fact, its imposition would actually make the vast majority of cases in the history of scientific achievement appear irrational.
Were we to impose more stringent standards upon scientific activity, then no scientific novelty could have survived the test of such standards, since no scientific novelty has ever unambiguously met every criteria immediately. Similarly, Kuhn argues that the standards could not be set too low—which is certainly one of the objections critics raise against his own position—since that would permit a constant proliferation of theories between which one could not, in a non-arbitrary way, choose. In short, Kuhn is aiming for a middle ground, in which judgment, as the application of shared values, is primary.⁸

This middle ground, however, is occupied by a number of thinkers. When Kuhn describes the adherents of an algorithm as being those for whom no rational disagreement is possible, and for whom every scientific decision would have to be made at the same time, then he would find no such adherents among the thinkers listed above. Kuhn, does believe he is attacking Popper, insofar as he appears to believe that the following can fairly be attributed to Popper:

a) Popper is, or can be treated as, a naive

⁸ I will take up this issue not only in the following section, but also at some length in the third chapter. At that point we will see that Richard J. Bernstein has drawn some interesting parallels between Kuhn's understanding of judgment and the Aristotelian understanding of phronesis. This parallel itself, furthermore, introduces the contribution of Gadamer to the discussion. The upshot of the parallels will be a better understanding of the role of tradition (and dogmatism) in scientific activity.
falsificationist (Kuhn, 1970b:14);

b) Popper believes that a falsified theory is not or cannot be used in scientific practice or in the search for a new theory (Kuhn, 1980:191); or,

c) Popper believes that scientists do or must act as though they were in a state of nature or epistemically original position (Callebaut, 1993:301).

I will argue in the next chapter that none of these positions can, in fact, be attributed to Popper.

**Objections to Kuhn: Larry Laudan**

Consideration of Laudan's critique in this section foreshadows the analysis that will take place in Chapter Three, under the heading, "The Dispute Over Values."

Laudan's main criticism is that Kuhn's position underwrites revolution as the breakdown of consensus, without explaining either how such consensus can be re-established or how the revolution can be understood as progressive. In Chapter Three we will see John Caputo make a similar point—though he does not by these means denigrate Kuhn, but rather links him to Derrida. On this reading, revolution is the perpetual dissolution of normal science into a state of anarchy, for which state shared values are ineffective in arbitrating judgments between theories. I will argue there that this completely overlooks two of the more obvious claims made by Kuhn: that values are not ineffective during revolutions, and that dogmatism and revolution must be seen
in their complementarity, not as isolatable episodes. Kuhn's worry with Caputo's understanding of scientific revolution, I will suggest, is the same worry Laudan expresses with respect to Kuhn: this understanding of revolution only underscores the impotence of revolutions--their perpetual and mutual cancellation--rather than their power. The potency of any revolution lies in its ability to recover the "middle ground," where, as I said above, judgment is primary.

The position that Kuhn carves out in this middle ground as being characteristic of scientific activity is attacked by Larry Laudan, who attempts to uncover the relativistic and incoherent aspects of Kuhn's position. In this regard he focuses on the following statements made by Kuhn:

Lifelong resistance [to a new theory]...is not a violation of scientific standards....Though the historian can always find men--Priestly, for instance--who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific (1970a:159).

And similarly,

The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced. Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition of normal science, is not a violation of scientific standards but an index to the nature of scientific research itself (151).

As far as Laudan is concerned, statements like the above smack of sheer relativism. When combined with Kuhn's
attempt to characterize the criteria for paradigm choice as values rather than rules, these statements actually undermine Kuhn's attempt to view history as a resource for understanding the nature of scientific activity. Despite the fact that Kuhn argues for a place for "objective" criteria for theory-choice, Laudan argues, he too strongly delimits the sufficiency of their applicability.

Consequently, he is left without a recourse--other than the dubious metaphor of "conversion"--to explain exactly how in the history of science so much consensus has come about (Laudan 1984:17-8). Kuhn's theory, in other words, may help to explain why disagreement breaks out among scientists, and even why such disagreement lasts as long as it does, but his theory provides no "mechanism" for consensus formation. Consensus, on this view, has been one of the most distinctive features of scientific activity. Accounting for consensus was actually one of the motivating reasons behind Kuhn's introduction of the concept of "paradigm": he does provide a picture of science as a consensual activity, which is one of the things he means by "normal science." But what he does not provide, Laudan claims, is a plausible account of the dynamic process by which the crisis state of scientific activity is transformed into the consensual state of normal science. The reason he cannot provide such a picture is because he has, by insisting on the incommensurability of standards governing the choice of
paradigms, removed any common rational foundation on which to shape consensus anew (Laudan, 1984:18).⁹

As an instance of this, Laudan points to Kuhn's position that different paradigms deal with different problems. In attempting to come to grips with the fact that in the history of science there has been explanatory loss as well as explanatory gain, Kuhn works himself into a position in which consensus cannot be explained (Kuhn, 1977:211). Kuhn states,

To the extent, as significant as it is incomplete, that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm will be shown to satisfy more or less the criteria it dictates for itself and to fall short of a few of those dictated by its opponent....Since no paradigm ever solves all the problems it defines and since no two paradigms leave all the same problems unsolved, paradigm debates always involve the question: Which problems is it more significant to have solved? Like the issue of competing standards, that question of values can be answered only in terms of criteria that lie outside of normal science altogether, and it is that recourse to external criteria that most obviously makes paradigm debates revolutionary (Kuhn, 1970a: 109-110).

According to Laudan, this position is rather extreme. Kuhn rejects the idea that science is progressive in the sense of

⁹ The incommensurability debate sustains a now voluminous primary and secondary literature. Among the leading voices in the debate are Davidson (1985), Putnam (1981), Feyerabend (1987), and Rorty (1979). Of central importance to my own understanding of this debate and its relationship to the figures and issues treated in this paper are Ingram (1993a, 1993b, 1995).
being cumulative. But in doing so he simultaneously
denigrates progress into something that can at best only be
instrumental. He fails to see that there is the
possibility of deploying the concept of progress in such a
way that it avoids, on one extreme, the assumption that it
be cumulative, and on the other extreme, that it be only
instrumental.

Laudan believes there is a way between these two
extremes. He says

Knowledge of the relative weight or the relative
number or problems can allow us to specify those
circumstances under which the growth of knowledge
can be progressive even when we lose the capacity to
solve certain problems. (Laudan 1977:150)

Laudan calls his position a "problem-solving approach" to
scientific activity. Contrary to Kuhn, Laudan believes that
one can "weigh" problems to determine their significance.
One of the most important means to weigh problems is, in
fact, to categorize much more explicitly than Kuhn has done
the various "kinds" of anomaly that confront any scientific
theory. Kuhn, it has been shown, is very general in his
analysis of anomaly, and is particularly imprecise when it
comes to determining when exactly an anomaly is or becomes a
source of crisis. In contrast, Laudan seeks to be
considerably more specific. Laudan's aim is a "calculus" to

\footnote{By "instrumental" I believe Laudan intends the
pejorative sense according to which science would merely
produce better means, or instruments, for independently
determined ends.}
determine the significance of scientific problems. At times, this calculus is stated very generally, such as when he says

The overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates (Laudan, 1977:68).

At other times, Laudan makes very specific proposals for a calculus (33ff.). In any case, his essential plea is that it must be possible, if we are to affirm the rationality of theory-choice, "to indicate at least the differences between those anomalies which are disastrous for a theory and those which are only a mild embarrassment" (37).

Does such a calculus hold any promise? On the one hand one can say that such a calculus would face problems similar to those faced by the utilitarian project of a calculus for moral decision-making.\textsuperscript{11} Kuhn in fact might object that Laudan is assuming much more commensurability between theories than is warranted. Would the calculus be a neutral instrument for weighing the significance of problems across paradigms, or would each paradigm first determine the significance of its own problems? That is, do we compare paradigms only after we have first--in abstraction from the paradigms themselves--"individually" weighed the problems within them, or do we compare paradigms after each paradigm

\textsuperscript{11} Cf. Steve Fuller, \textit{Social Epistemology} (Bloomington: University of Indiana, 1988), 103.
has derived the "sum total" of its problem-solving effectiveness by its own criteria?  

Summary

The point of this section was that there does seem to be some prima facie warrant for the idea that the relative significance of problems must be determined. Of course on this point Kuhn himself would agree. Laudan's project of establishing a calculus is highly questionable, however, particularly in light of the problems such projects have experienced in the past. The determination of relative significance is better accounted for, Kuhn argues, by the dynamic of normal science.

The Dynamic of Normal Science

The implicit context for the kind of scientific activity that has been discussed thus far has been what Popper calls "revolutions in permanence," the ideal of which is that scientists engage in "perpetual framework-breaking"

\[12\] This apparently naive question is of some importance, for in either case it would be difficult to adjudicate the weighing of a problem in a situation such as the following: when comparing one-on-one the solutions offered to a problem by two competing paradigms, one determines that solution A provided by paradigm 1 (P1) is "better"--because more accurate--than solution B provided by paradigm 2 (P2). Yet, the importance of solution B in P2 has significant implications for the acceptance of solutions within a completely different paradigm P3. How would one, in this example, go about making a determination of the weight of the problem to which these two opposing solutions were solutions? This question captures Kuhn's point about determining the relative value of different criteria such as, in this case, accuracy and scope.
(Popper, 1970:242). According to Popper, Kuhn suggests, "the scientist should try at all times to be a critic and a proliferator of alternate theories" (Kuhn, 1970b:243). On Kuhn's reading of Popper, proliferation can take place without falling prey to paralysis because of the efficacy of the principle of falsification. Significant variation and difference must be permitted--bold conjecture must be the defining mark of the serious scientist.

Kuhn's own idea of crisis differs significantly from this state of affairs. If one were to search for the operative standards that determine the significance of problems, then one would not conduct that search within a context like the one thus far characterized as a crisis-state. In other words, one does not first determine what the significant problems and standards are in the state of crisis. In the crisis state, preliminary determinations of such significance have already been made through the processes and products of normal science. This is why Kuhn is not satisfied with the characterization of crisis provided. It is also why he would not agree to Laudan's "calculus" for determining problem-significance. Such determinations for Kuhn are not made in abstraction from the context of crisis nor only after the crisis has arisen. For

13 But as we shall see in detail in the next chapter, Popper allows for a considerable amount of dogmatism in inquiry. His recognition of the need for such dogmatism is a primary point in his rejection of the criticism that he is a naive falsificationist.
that matter, in an assertion Kuhn aims directly at Lakatos, such determinations not made only after a research programme is in decline, in a period of stagnation, or has ceased to produce novelty (Kuhn 1980:190-1).\footnote{I will develop the implications of this claim at the end of this chapter.}

Therefore, for Kuhn, in order to understand the context of crisis--and the problems and standards operative within it--one must understand the dynamic of normal science. The dynamic of normal science, according to Kuhn, provides the "special occasions" when the scientist should adopt a "revolutionary" attitude. In other words, the scientist should not engage in constant framework-breaking, but should do so only when the time is right--when a special occasion presents itself.\footnote{The interpretation of Kuhn which pictures one period of normal science "breaking off" and being supplanted by a period of crisis, which in turn breaks off and is supplanted by a new period of normal science, etc., fails to recognize that for Kuhn normal science and the crisis-state can only be distinguished within the context of a general dynamic of scientific activity. Normal science and the crisis-state are not self-enclosed 'periods' successive upon one another; rather, they are better described as 'ways' of doing science, the description of which is not reducible to a relationship of temporal succession upon one another. Some temporal succession is identifiable, but only against the background of an integrating dynamic. Rouse makes this observation in drawing his own comparison of Kuhn with Heidegger. According to him, these 'ways' of doing science are "moods" or "dispositions," in a Heideggerian sense, of the scientific community (Rouse 1981, 277-8).}

The distinction and interaction between normal science and the context of crisis, suggested by Kuhn, is intended to
undermine a conception of crisis which makes of it the privileged characteristic of science. Furthermore, it elevates the dogmatic element beyond what someone like Popper permits. Kuhn describes his insistence on this dogmatic element as being "strategic." Popper describes Kuhn's kind of dogmatism as dangerous. On the other side of this rhetoric, significant differences become manifest.

For Kuhn, when a theory that satisfies the requirements for being a good theory is made available, "the time for steady criticism and theory proliferation has passed" (Kuhn 1970b:246). Scientists could continue to proliferate theories and question fundamentals, but they typically do not. There are two reasons for this, representing positive and negative aspects of the dynamic of normal science. First, if scientists commit themselves to a theory, they gain the opportunity to "explore nature to an esoteric depth and detail otherwise unimaginable" (Kuhn 1970b:247). Second, they can indulge their 'dogmatism' in the confidence that it will actually function ultimately to reveal the weaknesses in the theory and thereby set the stage for future revolution.

Of course this preliminary description of normal science raises the question not simply of what allows the

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16 Note that the title of his critical response to Kuhn, "Normal Science and Its Dangers" (Popper 1970), is obviously intended to echo his famous book, The Open Society and Its Enemies.
scientist to stop questioning fundamentals and stop proliferating theories, but more importantly what rationally motivates the scientist to do so? Kuhn admits that scientists could try to engage in such activities, but that they could not engage in them constantly or permanently (Kuhn 1970b:242-3). Of course, many philosophers of science would agree with this.\(^{17}\) The question is: where does one go from that point?

In one sense, that scientists do not engage in constant proliferation is, for Kuhn, an individual decision based upon the freedom the scientist enjoys; in another sense, that scientists do not engage in such activities in the face of an available theory is a characteristic temperament of scientists reflective of community structure and its patterns of education. Neither of these senses, however, justifies the claim that they should not engage in "revolutions in permanence."

The motivations Kuhn uncovers are primarily psychological and sociological--for example, he points to various dispositions towards "risk-taking." These dispositions are molded through a community pattern of education that also determines the range of standards of tolerance. What are the reasons behind the determination of

\(^{17}\) This issue will be discussed more fully in the next chapter. Once again, the degree of difference between Kuhn and someone like Popper is not as wide as is sometimes suggested.
this range? The question arises whether epistemic motivations on the part of an individual scientist are actually legislated by the community—which may be fine and good. But is this legislation itself epistemically motivated? For example, community self-preservation and maintenance of professional identity are important values, but are they the fundamental standards legislating scientific research? When Kuhn speaks of the "ideological" nature of being alert to anomaly, one can justifiably suspect that he is more in sympathy with the Edinburgh school of the sociology of science than he would care to admit (Kuhn, 1970b:248). Is he in fact sliding down the slippery slope?

Furthermore, because Kuhn restricts the efficacy of testability in practice (e.g. falsifiability), particularly during those crisis states when theories are proliferating, his understanding of scientific activity threatens to undermine its rationality. Kuhn tries to mitigate this threat by disclosing the conditions necessary for a rational application of this principle. Unfortunately, the conditions disclosed seem to be too ideological to allow one to assign a rational motivation to such applications.

For example, Kuhn speaks of a "considerable resistance to paradigm change" within the practice of normal science, due to the fact that such practice has become "increasingly rigid." Resistors and innovators will "inevitably talk
through each other," so that a change in belief or commitment to a new paradigm is best described as a "conversion experience" or a "gestalt switch."

Many questions plague Kuhn's position here. If the dogmatism of normal science functions to reveal its own weaknesses, then the question is: to whom are such weaknesses revealed? Does an individual scientist escape dogmatism? If so, how? Does one ultimately escape dogmatism only by never having been dogmatic oneself? Kuhn comes off sounding this way when he speaks of paradigm change as taking place when one generation dies off and another takes its place. In this sense, then, scientists never do escape their dogmatism, though the next generation may (though they themselves would be fated for their own dogmas). But then science would seem to change only because of dysfunctions in the educational patterns (or indoctrination methods) of its community. This is the danger Popper sees in the idea of normal science: the devaluation of criticism in favor of (dysfunctional) educational systems.

Kuhn does provide more 'objective' reasons for taking anomaly seriously.

In science....novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation. Initially, only the anticipated and the usual are experienced even under circumstances where anomaly is later to be observed. Further acquaintance, however, does result in awareness of something wrong or does relate the effect to something that has gone wrong before.
That awareness of anomaly opens a period in which conceptual categories are adjusted until the initially anomalous has become the anticipated.....Let me now point out that, recognizing the process, we can at last begin to see why normal science, a pursuit not directed to novelties and tending at first to suppress them, should nevertheless be so effective in causing them to arise. (Kuhn, 1970a:64)

In other words, the dynamic of normal science itself, by which a paradigm is extended in its applications, leads to the emergence and recognition of novelty. Such novelty first "emerges" as anomaly, which gets transformed by the process from the unexpected to the anticipated. Kuhn says that the scientific community struggles to make the anomaly "law-like" (Kuhn, 1977:174). He states

Without the special apparatus that is constructed mainly for anticipated functions, the results that lead ultimately to novelty could not occur. And even when the apparatus exists, novelty ordinarily emerges only for the man who, knowing with precision what he should expect, is able to recognize that something has gone wrong. Anomaly appears only against the background provided by the paradigm. The more precise and far-reaching that paradigm is, the more sensitive an indicator it provides of anomaly and hence of an occasion for paradigm change. (Kuhn, 1970a:65) (my stress)

It is one thing, of course, to identify an anomaly. It is another thing to determine its significance. The status of anomaly remains ambiguous: its existence alone does not necessarily warrant a crisis--there is no clear way of determining when an anomaly should be taken seriously, when it should be overlooked, when it may in fact eventually be solved within the framework of the existing paradigm, how much time should be permitted to allow for the emergence of
such a solution, etc. Anomaly, then, does not always provide an occasion for paradigm change. The most one can say is that it may provide such an occasion.

Anomaly alone does not logically compel one to abandon a particular hypothesis, since there is no way of determining where exactly in the network of assumptions and hypotheses the problem exists which led one to make an incorrect prediction; additionally, there is no way of determining whether the problem rests with the hypothesis or with the assumptions underlying the hypothesis. Given an anomaly, therefore, one has no reason to abandon any particular hypothesis or, in Kuhn's case, paradigm. The upshot of the argument, then, is that neither anomaly alone can undermine commitment to a particular paradigm, nor should the solution to anomaly by another paradigm necessarily lead one to adopt it. Anomaly can, therefore, up to this point, serve as a point of connection between disparate paradigms, and may even provide the occasion for the emergence of a new paradigm, but it is not, either as solved or unsolved, sufficient to warrant a choice between paradigms.

Kuhn's position here is actually no different from that of other philosophers of science. For example, Popper's view would be that commitment to an old theory ('dogmatism') can be rationally motivated by the promise of potential epistemic gain. Rational motivation ends when one
defends an old theory on the basis of increasingly *ad hoc* additions and conditions. Likewise, bold conjecture is rationally motivated by similar considerations of epistemic gain.

For Popper, the requirements that must be satisfied by a scientist's expectations for epistemic gain are considerably less rigid than they are for Kuhn. Popper grants considerably more freedom to the scientist to make conjectures at any time. At least one reason for this is that any conjecture must be tested, and this testing is the final arbiter of a claim to any epistemic gain. For Kuhn, rational motivation is ultimately subject to a rigor of expectations issuing from the current practice of normal science.¹⁸ It is not so much that the freedom Popper grants to the scientist is necessarily impermissible, but more that it is neither an efficient way for science to achieve its aims, nor the way that scientists typically do go about their activities. Once again, Kuhn differs on

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¹⁸ Later we will see Blumenberg make a similar point regarding historiography:

That what is new in history cannot be arbitrary in each case, but rather is subject to a rigor of expectations and needs, is the condition of our being able to have such a thing as 'cognition' [Erkenntnis] of history at all. (Blumenberg 1983:466)
"strategic" grounds.\textsuperscript{19}

The differences between Kuhn and Popper on the function of conjecture are traceable, I suggest, to differences in their attitudes about underdetermination. The problem posed by underdetermination has two sides: a retrospective and a prospective side. The retrospective side of underdetermination raises the question concerning the justification of theories already available. The prospective side raises the question of what direction one can take in the future given retrospective underdetermination. The retrospective side of underdetermination, taken alone, would seem to paralyze scientific activity. What sufficient reason could there be for a scientist to move forward with her work?

Insofar as Popper is a firm believer in falsifiability, he believes that conjecture does not simply arise out of spontaneity, but is related to the falsification of a theory. Conjecture arises in response to where the shoe pinches, i.e., to specific problems in present theory

\textsuperscript{19} This is not to suggest that it is simply for strategic grounds, although Kuhn himself uses this language (1970b:243). Kuhn's Wittgensteinian-inspired approach to these issues differs in significant ways from Popper's more Kantian approach. Furthermore, Popper's imperative (which Kuhn calls both "ideological" [1970b:243] and "moral" [1970c:22]) to engage in revolutions in permanence (to ruthlessly conjecture) stands in remarkable contrast to, as we shall see, Kuhn's own more Aristotelian-inspired deference to the authority of the (scientific) community. This issue, particularly Popper's own understanding of the relationship of the autonomous conjecturer/critic to the community will be explored in the next chapter.
Now, in the dispute with Kuhn, Popper seems to say something quite different. In challenging Kuhn's talk about crises, Popper holds up the example of Einstein as one who did not make his conjectures in the context of a crisis. In fact, Einstein's conjectures were responses to asymmetries that no one else saw either as endangering current theory or as needing extermination. The asymmetries were merely "ugly." Now as strong an objection as this may be to Kuhn, it is also at least moderately problematic for Popper as well. For Popper, there is no reason why one scientist engages in bold conjecture in a context where no one else sees a problem, and why another scientist is more clearly problem-driven. There is nothing "reasonable" about the activity of making bold conjectures—it is simply a tradition of myth-making. One may question whether the motivation for epistemic gain might drive the conjectural act, but in this example the only motivation indicated is the aesthetic value of removing ugliness. I am not accusing Popper of blatant inconsistency or incoherence, but am simply trying to point out the level of sophistication at which the distinction between Kuhn and Popper becomes ascertainable. In the Einstein case, it appears that Popper grants considerable ground to the thesis that theory-change is primarily theory-driven. Of course Popper might respond that, not change, but only conjecture is theory driven.
Kuhn approaches the question of undetermination by asserting that puzzle-solving is the measure of scientific achievement. Specifically, Kuhn holds up the standard of exemplary problem solutions as the models by which future research can proceed. Undetermination means that a theory always says more than is warranted by nature. The theory is permitted this excess and will survive as long as it remains compatible with nature. In other words, a theory can always say "more" than nature warrants and still be considered a "fit" with nature as long as what is says is compatible with nature.

The preceding recognizes that for Kuhn knowledge is not secured inductively, from the ground up, by piecemeal accumulation of observations. Instead, the search for knowledge begins with conjecture--"imaginative posits, invented in one piece for application to nature" (Kuhn 1970c:10). The procedure of this search involves the "specification" or "articulation" of the theory by means of the application of exemplary problem solutions to gaps within the theory. These applications are what Kuhn calls puzzle-solving, and it is the dominant feature of normal science, by which scientists "explore nature to an esoteric depth and detail otherwise unimaginable."

It is important to realize that for Kuhn this process of application incorporates testing at a 'latent' level. In other words, puzzle-solving does involve testing, but only--
as Popper might put it—as a second-order tradition. The primary impetus in puzzle-solving is to fill in the gaps.

This race to apply the exemplars to numerous puzzles may seem to be just another case of trying to be successful without being sure whether your success is due to knowledge or chance. In other words, it may seem to be a case of extension by means of the path of least resistance. In such a case, the excess claims our theory makes may be compatible with nature insofar as they are not expressly refuted. But such excesses could not claim to be knowledge, insofar as many constructions of the imagination could be posited that could not be falsified.

Kuhn is not advocating this kind of path of least resistance. Instead, recognizing that the value of Popper's falsifiability principle (or, as Kuhn prefers to call it, the asymmetry principle) is its elevation of "resistance" into the surest marker of knowledge that we have, Kuhn is suggesting that the puzzle-solving path is the means to uncovering the most—and the most troublesome—resistance. Kuhn's point is simply that anomalies may occur at many points at many times, but no one believes that the mere existence of an anomaly refutes a theory. Furthermore, no one believes that the appearance of an anomaly even represents an immediate challenge to a theory. At best, one might say that it represents a potential challenge. But a scientist generally blames himself or other factors many
times over before he turns on the theory. Ultimately, though, the scientist may turn on the theory.

However, even when the scientist turns on the theory, the rules, problems, and puzzle-solutions which have guided that theory continue to operate as standards of achievement, establishing burdens of proof against which competitor paradigms must measure up. In this way, the old paradigm functions in the process of discovering a likely successor. It does this by establishing the standards and problems to which the successor must be a response. Kuhn states it in the following way.

Competing research programmes need not in principle display differences in fruitfulness. Whether or not they do, furthermore, at least one other criterion must be considered when choosing between them. A preferred programme is expected not only to produce new achievements but also to conserve the achievements of the programme it replaces. In that situation, the successes and failures of each programme provide the standards against which the other must be evaluated. Appraisal then becomes intrinsically relative, involving both programmes as well as nature from the start. (Kuhn, 1980:190)

On the following page, Kuhn continues the same line of argument as follows:

Whatever its difficulties, Sir Karl's [Popper] emphasis on falsification captured an aspect of scientific life that [Lakatos's] methodology of research programmes effectively ignores. A scientific theory embodies expectations about natural phenomena; those expectations can be disappointed; and, when they are, some scientists usually start to look for an alternate theory. In ignoring this function of severe anomaly, Lakatos's position has, I think, moved too far from Sir Karl's. But in a closely related respect it remains too close. Once a theory has been falsified (Sir Karl) or entered a degenerating phase (Lakatos), it
may be displaced by another theory. According to both these viewpoints, however, the existence of a potential replacement theory appears pure coincidence. Neither falsificationism nor Lakatos's methodology suggests how closely successive theories or research programmes relate to each other: the successor is usually conceived in response to, and is often also shaped by, particular difficulties encountered in the development of its predecessor.

(191)

For Kuhn, science proceeds by means of a logic of provocation and response, such that paradigm choice takes place within a context by which the adoption of one paradigm is simultaneously the displacement of another. 20 But this

20 We can now see how Blumenberg's criticism, to which I referred at the outset of this chapter, misses the mark. Blumenberg's comments can now be appreciated in full:

The theory of 'scientific revolotions' describes, for the most part correctly, the breakdown of dominant systems as a result of their immanent rigorism, the 'pedantic' disposition of every schoollike mode of thought, which leads with fateful inevitability to the self-uncovering of the marginal inconsistencies from which doubt and opposition break into the field. This conception of what historians have been pleased to call "downfalls" may be capable of generalization to a high level in relation to historical phenomena. But in relation to the new foundations called for afterward, to the preference given to the new "paradigm," this schema has no explanation whatever to offer. (Blumenberg 1983: 465)

Blumenberg goes on immediately to classify Kuhn's theory as fostering a "decisionist" approach to theory replacement. Whatever more serious oversights may have led to this particular interpretation, Paul Hoyningen-Huene has offered one possible source for the "decisionist" reproach:

In the German-speaking world, the apparently quasi-religious character of scientific revolutions became almost unavoidable with the translation of "conversion," or "to convert" by "Bekehrung" and "bekehren," respectively. (Hoyningen-Huene 1993:258)
choice is not simply according to falsificationist standards, but also according to the concrete standards imposed by each paradigm. Other commentators (i.e., in addition to Blumenberg) have missed the functional role Kuhn assigns to the previous paradigm both in provoking the emergence of a new paradigm, and in setting specific standards for what can qualify as a new paradigm. We will see in chapter three that John Caputo misses, or perhaps disregards, the functional role played by a previous paradigm.

The new paradigm, then, must satisfy the majority of the needs previously satisfied. The advantage to this procedure is that if a paradigm is simply abandoned at the first instance of anomaly, on the basis of a particular application of standards of testing, then, as Popper himself admits, the real power of the paradigm may never be discovered. The power of a paradigm is two-fold: its unfolding of the knowledge it may achieve, and its ability to eventually uncover those problems the solution to which will lead to significant advance. If a paradigm is abandoned before these problems are articulated, then science may proceed not only at a slower pace, but at a greater cost.

Once this misunderstanding is cleared up, it becomes easier to see the parallels between Kuhn's claims and Blumenberg's own provocation/response schema, which I will discuss in chapter four.
Summary

The treatment of Kuhn's philosophy of science presented in this chapter corrects certain misconceptions of his position, and prepares the way for a clearer statement of the complementarity of dogmatism and revolution. Ultimately, for Kuhn--and as we shall see, ultimately, for Blumenberg--the affirmation of progress requires an affirmation of complementarity between revolutionary breaks and tradition continuity.

Kuhn wants to affirm both dogmatic and revolutionary aspects of scientific inquiry. This is a difficult mix. How can dogmatism open itself to the kind of revolutionary critique which claims to comprise science's most dramatic advances? And yet, how can revolutionary critique measure the success it claims, if not against a frame of reference which can bridge the gap opened by the revolutionary break, and thereby offer points of comparison?

As we have seen throughout this chapter, the dogmatic side of science--normal science--is not inert. Its offer of stability incorporates its own distinctive dynamic. Normal science is not, therefore, the positing of a stable mass which then gets eroded by means of a constant barrage of criticism. It is not simply a target for criticism. Nor, finally, is it a passive system which avoids conflict. A central part of Kuhn's contribution is to highlight what is involved in the search for conflict. An important part
of that search is internal. As we shall see in the next chapter, conflict for Popper fosters the kind of competition within which the competencies and achievements of a theory (or lack thereof) can be revealed. Kuhn also appreciates the possibilities inherent in conflict, but is less sanguine than Popper about the prospects for a revolutionary conflict that is undertaken perpetually, or at any time. We will see in the next chapter that Popper himself, despite his loyalty to the credo "revolutions in permanence," does not dispense with a role for dogmatism. His own mix, however, may be no more satisfying than is Kuhn's.
CHAPTER THREE
POPPER: BETWEEN DOGMATISM AND ENTHUSIASM

Insofar as Popper also recognizes the dogmatism that Kuhn has identified, he too must affirm some mix or complementarity between dogmatism and criticism in inquiry. The purpose of this chapter is to investigate the kind of mix Popper offers.

Popper interprets the role of dogmatism in inquiry as helping to establish "where the real power of our theories lies" (Popper 1970:55). Although it would be too simple to say (as a kind of summary of the first chapter) that for Kuhn such a determination is only made from within the paradigm itself, by means of a purely internal dynamic, still the determination of its power is not achieved only by an external critique either--as Popper seems to suggest with his idea of "revolutions in permanence."

The importance of establishing the power of our theories, for Popper, lies in the contribution it makes to an assessment of the progress achieved through revolutionary theory change. Popper does not assess progress simply by measuring competing theories against nature; rather, an assessment of progress requires theory comparison. It is crucial, then, to be able to determine exactly what the
competing theories are. Popper states that a newly-accepted theory must always be able to explain fully the achievements of its predecessor (Popper 1994:12). It must be able to explain what the old theory had been. By the end of this chapter, we will be in a position to understand why Popper's thesis of permanent revolution--or perpetual framework breaking--risks failing to satisfy this very requirement: can revolutionary critique alone, as Popper understands it, make this determination?

If we approach Popper's statements concerning dogmatism only from the perspective of a strong version of the doctrine of falsifiability, then we might feel, as Kuhn evidently does, that these statements are concessions that threaten the integrity of Popper's position. If, however, we approach Popper's overall position keeping these concessions in view from the start, then we see that Popper's position takes on a subtlety that perhaps Kuhn has not adequately considered. This subtlety reveals considerably more overlap between Kuhn's and Popper's position than is normally recognized, and so makes the resulting differences between them more significant. In this section I will explore Popper position on dogmatism. In the next section I will explore his position on the severity of criticism, and the corresponding boldness of conjectures.
Popper often gives the impression that he is an unequivocal opponent of dogmatism. He often lumps dogmatism, ideology, intolerance, and intellectual fashions together. In so describing these terms he opposes them to "criticism," which is at the heart of rationality and the growth of knowledge. He states, "I hold that orthodoxy is the death of knowledge, since the growth of knowledge depends entirely on the existence of disagreement" (Popper, 1994:34). Most failures to advance knowledge are due to such orthodoxy. Popper therefore calls himself an "almost orthodox adherent of unorthodoxy" (34). Throughout his discussions of such issues, however, he also makes careful distinctions which indicate both that his position is more subtle than a mere surface reading would capture, and that he senses a certain tension within the distinction between dogmatism and criticism.

Popper's first 'concession' is an explicit one: "...I am an admirer of tradition" (34). Popper, in fact, is a strong supporter of tradition. He mentions this as one of the points of overlap between Kuhn and himself that Kuhn seems to have missed (Schilpp, 1974:1195, n201). He not only wrote an article entitled, "Towards a Rational Theory of Tradition," but also refers to the "critical tradition" that the earliest Greek philosophers introduced (Popper, 1989:126, 149; 1994:42).
But Popper not only acknowledges an appreciation for tradition, he correspondingly modifies his conception of dogmatism. He states that "there is even something like a methodological justification for individual scientists to be dogmatic and biased" (1994:94). He claims that "a limited amount of dogmatism is necessary for progress" (1994:16; 1989:49). Thus, he begins to 'blame' not dogmatism as such, but "intolerant dogmatism" for being a main obstacle to science. The weight shifts from a distinction between dogmatism and criticism to a difference between tolerance and intolerance. Tenaciously defending a belief is an intellectual virtue, but such tenacity must always be coupled with tolerance (1994:45). Intolerant dogmatism presumably is defined as a disposition unwilling to modify, correct, or even give up one's theory.

Popper provides yet another indication of the nuance involved when he states that the critical attitude itself "shares with the dogmatic attitude the quick adoption of a schema of expectations...but...is ready to modify it, to correct it, and even to give it up" (1989:49). The "adoption" of a schema by criticism cannot here be understood as being "addicted" (1994:53), which is characteristic of intolerant dogmatism. Instead, this kind of critical adoption does not wish to "be caught" in a mental prison. One only "accepts" or "commits" to a theory for the time being: that is, tentatively. Popper claims, in
fact, that the entire question of theory "acceptance" is over-rated (Popper 1994:102). Even after a period of sustained criticism one should not become too enamoured with a theory. He sees the idea of an accepted theory as a "residue of the dreams of authoritarian science prevailing in the days when people thought that we were just on the verge of completing the task of science" (103).¹

And yet, Popper also points out that one must be careful not to adopt a theory too lightly either, since it would seem to make little sense to speak of "adoption" at all without some sense of commitment. The positive sense of this warning was indicated above--namely, that theories do need to be tenaciously defended. Put negatively, Popper identifies a different extreme--at the opposite end of dogmatism, as it were--that he characterizes as "following intellectual fashion." The fashion-oriented are attentive to the "latest cry," and uncritically accept the currently-ruling fad. They are "swayed by fashions," and "fear to be regarded as laggards" (57). Here Popper touches upon what could be called the intellectual vice of "jumping ship," which indicates a lack of loyalty or an over-inflated fear of being 'caught' in error. Such people give up too

¹ Compare these comments with those of Imre Lakatos:

Belief may be a regrettably unavoidable biological weakness to be kept under the control of criticism: but commitment for Popper is an outright crime. (Lakatos 1970: 92)
quickly, and do not try to salvage or defend a position to which they had committed themselves. At the first sight of a schema's failure to live up to one's expectations, it is abandoned. This is the vice of the intellectual avant-garde. Rather than risk being behind the times, such people are "enthusiasts."

I said that such fashion-conscious individuals are at the opposite extreme of intolerant dogmatists. If intolerant dogmatists refuse to give up their theory, then the enthusiasts are too quick to give up their theory. If the intolerant dogmatist is insulated from criticism, then the enthusiast is overly-sensitive to possible criticism. What makes them very similar is that neither group is willing to engage in the effort to modify and correct their theory. For Popper, this means that neither group is willing to engage in the activity of criticism. Dogmatism and enthusiasm are really two sides of the same coin.

For Popper dogmatism and enthusiasm are species of relativism. The dogmatist retreats into a position that cannot (or at least will not) be falsified, and the enthusiast is driven to new positions constantly, without the benefit of knowing whether its new position is an advance upon the old one—that is, without the benefit of having learned from its mistakes. Between these two positions Popper inserts his understanding of the critical
tradition. This tradition champions the belief that we can learn from our mistakes. Perhaps more importantly here, this tradition acknowledges the possibility of its own error (unlike intolerant dogmatism) and elevates the value of this possibility (unlike enthusiasm).

Popper himself is sometimes understood as being much closer on the spectrum to the enthusiast than to the dogmatist. For example, whereas the dogmatist is not open to criticism in any way, shape or form, the enthusiast at least acknowledges criticism. However, the form this acknowledgement takes form distinguishes Popper from it, insofar as it could be characterized vulgarly as positing that "the truth is merely the latest lie that hasn't yet been exposed." Such a "bad faith" disposition toward present conjectures motivates a desire to be agile enough never to get "caught" in the error. Error is inevitable and necessary on this understanding, but we nonetheless ceaselessly try to escape from this necessity. And of course the easiest way to effect such perpetual escapes is

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2 Insofar as Popper is a believer in progress, then, he does affirm (as we saw Kuhn and Blumenberg affirm in the last chapter) that there must be the background stability of a constant frame of reference for comparing progress. It is also important to note that, for Popper, theories are not simply compared or measured directly against reality—as Kuhn seems to interpret him. Finally, Popper's own emphasis on a critical tradition, and particularly on the difficulty of transplanting a tradition where one did not formerly exist, becomes extremely important for a fair understanding and appraisal of his position. See "Towards a Rational Theory of Tradition" (Popper 1989), p. 121.
to always remain uncommitted to any scheme of expectations.

Some may hear a bit of Popper in these remarks: the difference may not be as sharp as Popper would like to think it is. However, what makes things interesting is that, as this section has attempted to show, the difference between Popper and the dogmatist is no more sharp. The way that Popper would like to draw the sharp distinction in both cases is through his emphasis on the "critical tradition," the tradition of trial and error.³ We will see in the next section, however, that both the severity of criticism and the boldness of conjectures remain intimately related with both of these extremes. The importance of these relationships, I would suggest, is that they indicate that Popper's position does not rest exclusively on the prospects of a 'naive' falsificationist strategy--as Kuhn, for example, believes.

Rebuttal as the Conjecture to Stop Defending a Theory

Two statements Popper makes in different contexts will eventually be important for discovering a link between his and Kuhn's work. The first statement, that a scientist should "leave it to others to fit his contribution into the

³ An emphasis that Popper also shares with Gadamer, although, as we will see, there are considerable differences. Specifically, Gadamer identifies Popper's emphasis only with the "deliberate" side of human experience, and not enough from its "suffering" side (a side in which Kuhn might feel more at home). See Truth and Method (Gadamer 1990), p. 353n.299.
framework of scientific knowledge" is the only one recognized by Kuhn (Popper, 1970:51). The spirit of this statement indicates that an individual scientist considering a conjecture should first test whether that conjecture can be falsified. That scientist should subject the conjecture to the most severe tests, and only if it passes these tests should it be opened to the criticism of others. It is then up to these others to criticize and test the conjecture, and, if they fail to falsify the conjecture through these exercises, they should then try to find a way to "tie it in" to other conjectures that have stood up to testing and criticism.

It must be noted that Popper prefaced this description of scientific activity by stating that a conjecture is offered in response to a particular problem or problem-situation. In another context, Popper advises a young, would-be scientist to go study the field, to find out what scientists are working on (1989:129). The young scientist learns the problem-situation through such study. The anticipated result is that this scientist will take on a problem that has arisen within the field and is relevant to the field. Any conjecture should then be in response to such a problem.

Of course one of the problems this young scientist faces is getting others to take notice of the conjecture made. Even if the conjecture has passed the scrutiny of the
young scientist, will it be entertained as a possible solution to the problem by would-be critics? One must be careful here not distort what Popper is talking about. He does not take the route that is being heavily explored today--namely, to investigate the extent to which it is for sociological reasons that one gets heard. In other words, it is not because one has the right degree, or is a member in good standing in the right associations, etc., which provide the conduit for being heard. It is not the discipline to which one belongs that makes the difference, for problems cut right across disciplines (1989:67). As important as the other conditions are, for practical reasons, we must not fall into the trap of consigning the problems or the problem-situation to one discipline or another. There is no "natural" link between a discipline and its problems.

For Popper, to suggest that it is ultimately for either sociological or disciplinary reasons that science proceeds as it does is to suggest that science has little choice but to be either dogmatic or enthusiastic in its approach.

The individual scientist must not be primarily focused on the discipline, or on what, at one time, Kuhn called the disciplinary matrix. One's primary commitment must be to the problem--one needs to develop one's understanding of the problem, and show that one has been attentive to it. The essence of scientific activity, for Popper, is that
scientists, taken individually or as a group, must be directed to problem-situations, and organize themselves around such situations (rather than the reverse, which is what Popper considers the sociological position to be). Scientists are not "guardians" of the problems (which is precisely what he understands Kuhn's scientists to be, insofar as Popper hears an echo of Plato's "closed society" in Kuhn's normal science). 4

Were we to grant Popper, for the time being, this description of the scientific attitude, we would still face at least two important questions: How bold can the individual scientist be in her conjectures concerning this problem? How aware must she be of the relationship of her conjecture to those conjectures that have already passed severe tests in the field? Both of these questions, I suggest, pertain to the meaning of the expression, "let others tie it in."

There is no doubt that boldness is a primary virtue for Popper. He often advises the scientist to be bold in making conjectures, as well as to make bold conjectures. The first characteristic is one with which few would argue, although Popper's application of it is a bit vague, since boldness can be a characteristic of both the revolutionary-minded scientist hoping to make a significant contribution to the

4 Again, note the differences between Popper's Kantian, and Kuhn's Aristotelian approaches to this issue.
field, and the dogmatic-minded scientist insisting upon holding the ground against hostile criticism. Both scientists are putting themselves forward, as it were.

The second characteristic, however—making conjectures which are themselves bold—is the more controversial of the two, and is a focus of Kuhn's disagreement with Popper's position. Many of Popper's statements claim that the duty of the scientist is to make the boldest conjecture possible, so that at any time there should be a large number of bold conjectures available for testing and criticism. All of these conjectures would push the limits of what can be known to maximum strain. However, even here one always hooks up with a certain understanding of the problem-situation, and attempts to build upon gains previously made. A conjecture, in other words, always responds to both a problem-situation and to previous conjectures that have so far sustained severe testing.

In order to see that the boldness of which Popper speaks is not quite so radical as he sometimes makes it appear to be, we can link the boldness of conjectures to the second of the two statements which link Popper to Kuhn.

Popper suggests that an important threat to a scientist's continuing viability as a scientist is that a problem-situation might 'pass you by' (1974:23). Now on first glance this might seem to indicate that it is of primary importance for the individual scientist to "keep up"
with the latest trends or developments. However, Popper is wary of the virtue of "keeping up," since it seems to be a value most often esteemed by enthusiasts--i.e., those who favor intellectual fashions and fads. For people such as these, the vice corresponding to the virtue of keeping up is that of being a "laggard." The virtue of keeping up, taken in isolation, can lead to the mentality of the avant-garde.

What, then, does Popper mean by warning that the scientist should not let the problem-situation pass her by? I would suggest that the warning provides room for that 'legitimate' dogmatic impulse which Popper acknowledges, which includes not only the character of boldly and tenaciously defending a "pet theory," but now, here, also a certain inertia. It may seem strange to associate intellectual inertia with the name of Popper, but the context of the warning against letting the problem-situation pass one by suggests this very strategy.

Popper suggests that one can become so overly-attentive to developing precision in one's position, that in the meantime the problem-situation may have developed in a different direction than has your own work. Popper is suggesting that you should not attempt to boldly develop your 'resources' too early or too quickly--out of fear of being unprepared to cope with problems and objections. 5

5 By using the word 'resource' here I am not suggesting that the scientist, in Popper's view, should be understood as 'withholding' or 'deferring' an account (as a
Instead, by being a bit inert you may develop your position and have a contribution to make only when you are called upon to do so--that is, you may develop precision at the right time precision is called for. In the meantime, you should tarry for a response to what you have offered, and/or remain open to the developments in the problem-situation which actually take place (and not simply to those which have been anticipated by you). You must allow yourself the chance to be given a "reception." 6

If this is in fact an accurate interpretation of Popper's position, then Popper should be located differently than in the place standing at the opposite extreme of dogmatism. Popper would stand between what he calls an "intolerant dogmatism" and an enthusiastic avant-gardism.

deconstructist might argue)--but simply as giving himself over (as a resourceful conversation partner) to the reaction of the community--i.e., as opening himself to an exchange. This certainly foreshadows Gadamer's work, but for another interesting discussion of this, see Stanley Cavell, This New Yet Unapproachable America (Albuquerque: Living Batch, 1989), p. 23ff.

6 "Reception," we will see, is a technical term in Blumenberg's vocabulary. I will discuss it in Chapter Four within the context of the "reception" of Copernicus's theory. In brief, the issue hinges on what leads a scientist to expect that she will be "given" a "reception?" For Kuhn, normal science routinely provides this framework. For Gadamer, it is tradition. But what about in revolutionary periods? What is it that would lead Copernicus to expect that his proposal might not just be heard, but received--and hence, in Kuhn's terms, "normalize" relations? Popper, of course, would not like the term "normalized relations," nor the "routine" provision of a framework, though his position here seems to call for some such thing (Popper 1974:1152).
One could interpret both of Popper's statements—"let others fit it in," and "don't let the problem situation pass you by"—as advice to always be on the cutting edge of things, part of the vanguard. And yet, being part of the cutting edge might actually have to be understood a bit more modestly, as incorporating a dogmatic element: an element which values tarrying at the spot where one is; an element that balances the tendency that reflects an oversensitivity to refutation; an element that—as Kuhn might put it—ultimately sees anomalies only where there are anomalies to be found.

Another way to approach this aspect of Popper's position is by way of the unfortunate duality that Kuhn imposes on himself and Popper as a means to set himself off from the latter. Kuhn uses the language of challenger and challenged, where presumably in the situations most characteristic of scientific activity Kuhn believes the scientist sees himself as challenged (by some puzzle, through his understanding of the theory—or lack thereof), whereas Popper believes the scientist sees himself as the challenger (of the theory, in the name of the problem) (Kuhn 1970c:5n.1). In other words, says Kuhn, when a difficulty is encountered the scientist normally accuses himself, not the theory. Popper, on the other hand, is all too willing to abandon the theory. This is why Kuhn sees it as such a mark against Popper's position to concede that falsification
cannot be conclusive: it undercuts the legitimacy of Popper's advice to scientists to abandon a theory in difficulty. It is clear from this that Kuhn sees Popper primarily as an enthusiast.

Were this to be his position, then Popper would face some difficult questions. For one, how is it that the scientist achieves this status as challenger, particularly since Popper himself suggests that the normal course of study for the scientist is to begin by looking for challenges, by looking for problems in the field. The first challenge for the young, would-be scientist is to find the challenges the field finds relevant, and to come to understand these challenges. But how is it that such a challenged young person soon finds herself to be the challenger--how has a problem become so clear to her that she can henceforth speak so boldly concerning it?

The duality between challenger and challenged, however, cannot be successfully applied to Popper's position--particularly for the ends to which Kuhn dedicates it. But the failure of this application is quite instructive, for it helps to uncover how the scientist operates no less as, alternately, challenger and challenged in Popper's scheme than in Kuhn's. Specifically, for Popper the scientist (whether young or established) does not have direct access to a problem--at least not the kind of access through which that scientist could take it upon herself to "speak in its
name." For this reason, it is not the disposition to conjecture (boldly or otherwise) that is important for Popper, so much as the disposition to remain focussed on a problem. But, according to Popper, that disposition is precisely the problem! How does one address a problem? How does one identify it, get clear about it?

According to Popper, one certainly must make a conjecture which purports to solve the problem--a conjecture which, he says, will probably fail. But between the conjecture and the judgment of failure Popper does insert--contrary to Kuhn's reading of him--a significant amount of scientific activity, activity which cannot simply be described under the category of "testing"--as Kuhn understands Popper to mean it: subjecting the theory to maximum strain. To say that the theory is constantly being maximally strained is a kind of hyperbole which conceals the fact that the theory has not been "given over" to testing because it has not yet (if the conjecturing scientist has followed Popper's advice) been completely worked out in all its possibilities.

For Popper, then, the idea of criticism is much more complex than some parts of his writings seem to suggest, and certainly more complex than Kuhn would seem to indicate. Specifically, criticism plays upon what Kuhn might call a "strategy": the scientist who makes a conjecture must be open to criticism in the sense that she must leave herself
open to the response of other scientists--must put herself in a position against which criticism might be offered. But being open to criticism includes not having already 'overdeveloped' or exhausted your resources on your own anticipations of the direction the problem will take, but rather expending those resources in an exchange opened at the proper time. In this sense, criticism doesn't open a space for itself, but is also dependent upon the opening provided by the scientist making the conjecture. Both conjecture and criticism are led by the developing problem-situation. The success of criticism relies upon the reciprocity of quid pro quo. In this scenario, falsification would still remain "knowing what it would take for you to give up your theory...," but this "knowing in advance...." is tempered by the fact that the conjecture is only developed to a certain point before it is opened to criticism by adherents of the established scientific community/tradition.

I would suggest that Popper recognizes that the virtue of "opening yourself to criticism" incorporates a constraint on the boldness of our conjectures. If Popper seems to favor severe criticism of conjectures, this is less because he feels that we are in the position to act at any time as challengers to our theories in the name of the problems, than because he recognizes that our conjectures are always constrained significantly by a strategy which necessarily
"holds back" in order to open itself to criticism, and to the demands of the developing problem situation. We can "afford" criticism of the conjectures we favor. But we also need to open ourselves to criticism, since our conjectures are made in response to an understanding of the problem-situation that is always inadequate.

To understand Popper's position, it is again important to see it suspended between two opposites. On one hand, Popper is an enemy of any description of scientific activity which would reduce that activity to some "routine" or another. This is the threat he identifies with Kuhn's position. At best, such a routine is only effective as "applied science." As such, it falls short of the criticism and novelty requisite for scientific advance. But on the other hand, Popper also recognizes that conjecture can also be "too bold" or even a bit "wild." When Popper speaks in these terms, he indicates that advance in science is not constrained only by ideology or human failing. That is, although Popper focuses most on the obstacles to advance presented by ideology and human failing, there are other, positive constraints operative within conjecture and refutation that must be recognized, not just these negative ones: specifically, the question/answer framework as currently established by the scientific community (the "field").

If the foregoing presents a fair and more nuanced
reading of Popper than is sometimes offered, then the question which can be addressed to Popper is the following:

Having affirmed the necessary function of the "field," how much or how little can one disregard it as a constraint on the conjectures that could plausibly be entertained, criticized, and tested at any time? Of course one may grant to Popper that the "field" is not reducible to any particular discipline, since problems may cut across many disciplines at any one time. And yet, the field does not seem to be reducible to just the problems either. We have no independent access to the problems as individuals, but recognize that the problem might always move in an unanticipated direction through the efforts of others. Popper does affirm the role of a discipline in introducing a scientist to the field, and it does not seem that "a" discipline ever becomes dispensible. At the very least, the discipline seems instrumental to criticism.

At one point he calls his view a romantic ideal or a heroic ideal of science (1974:977). Scientific activity is represented best through the bold activity of its heroes. When Popper speaks this way, the role of the field or discipline--the scientific "community" itself--seems to be seriously subordinated. On the other hand, this rhetoric about boldness should not disguise the fact that Popper admits that naive falsification cannot underwrite the activities such heroic boldness requires. But the important
question here is: why would such heroic boldness not become linked to a supersensitivity to refutation? For Popper it must not be so linked--but in order to avoid this link boldness must, rather paradoxically, open itself toward dogmatism. Popper even associates this kind of dogmatism with debate (1974:). Dogmatism, it would seem, is at the very core of criticism. It is not an external extra added to it--a supplement; so much so, in fact, is dogmatism necessary to criticism, that the results of debate are never quite decisive: "As always, science is conjecture. You have to conjecture when to stop defending a favourite theory, and when to try a new one" (1974:984).

Now, if conjecture should be bold, then it seems that not only should one's conjectures themselves be bold, but the act of conjecturing should be bold; in other words, one should boldly conjecture that one is in need of a new bold conjecture. But even here, one would not do this at the price of debate--if one were to ALWAYS determine that boldness is in order, then there would be no way of separating this from supersensitivity to refutation. There seems to be needed a kind of courage to suffer through a debate with the tradition,7 to toughen one's sensitivity, and to discover the possibilities inherent in the theory you (force yourself to) defend and/or attack. Boldness, then,

7 Perhaps this is, after all, the suffering side of experience to which Gadamer seeks to draw our attention.
is not always simply a framework breaker. It is possible that boldness is a virtue that knows when to defend and when to attack.

**Aristarchus: A Case Study**

Popper does not seem to have settled in his own mind exactly what constitutes boldness and when boldness has become overextended—what he calls a bit wild. For example, he says at one point that the hypotheses of Aristarchus "seem to have been too bold, and they were soon forgotten" (1994:43). On this reading, it is possible to conjecture too boldly, so much so that the hypothesis is not seriously entertained, has no effect, and is soon forgotten. Yet at another point, Popper says that the theory could not be accused of being too bold—at least not for everyone—and Popper cites Seleucus' support for the hypothesis as support. He goes on to say,

And yet, for some obscure reason, only a few brief reports of the theory have survived. Here is a glaring case of the only too frequent failure to keep alternative ideas alive.

Whatever the details of the explanation, the failure was probably due to dogmatism and intolerance. But new ideas should be regarded as precious, and should be carefully nursed—especially if they seem to be a bit wild. I do not suggest that we should be eager to accept new ideas just for the sake of their newness. But we should be anxious not to suppress a new idea even if it does not appear to us to be very good. (1994:14).

Two comments are in order here. First, it is clear that Popper's main concern is that alternatives ideas must
be kept alive. According to Popper, ideas are "only too rare," and may easily, though not justifiably, be neglected. One of Popper's biggest worries is that specialization and "the publication explosion may kill ideas" (14). Ideas may easily get submerged in the flood of publication. Neither the scientific spirit nor the critical tradition are guaranteed. They can be lost. In fact, Popper seems to be quite worried that they are presently being sacrificed (1994:51). I think that one must link Popper's concern for the availability of alternatives with his "heroic" view of science: because ideas can easily be submerged, we need heroes and heroic efforts to continually bring forth new ideas. We need bold conjectures proposed by bold conjecturers.

The second comment pertains to Popper's claim that failure to keep alternatives alive is "probably due" to dogmatism and intolerance. One can credit Popper for not making the claim too strong, but at the same time may question whether the gap opened up between the arena where conjectures are made and that where intolerant dogmatism reigns is not too wide. Might there not be other reasons, factors, or influences involved that could assist in the effort to account for the neglect of alternative ideas?

In other words, one might ask why this failure is necessarily or always illegitimate--the mark of bad science. We might even ask, in light of the above statement that one
must conjecture when to stop defending a theory, a Popperian question: *why couldn't it be that the failure to keep alternative ideas alive is due to making the conjecture that one can make do without these ideas? Why should this conjecture be ruled out, or be considered a bad conjecture?* How is it that this conjecture is one that should not have been made? Is some mistake made in conjecturing that one can do without these conjectures (ideas)? Was, for example, some mistake made in not following Aristarchus' conjecture? Doesn't the very process of science involve the conjecture, the risk, that one can make do without a particular conjecture?

To repeat, this is not to say that intolerance plays no part, but it also need not account for the entire failure. In short, the failure to keep alternative ideas alive can sometimes be traced to the process of scientific activity itself, just as it can sometimes be traced to intolerant dogmatism.

Of course, to say that one rejects a conjecture because one has made the conjecture that one can do without it, must include the proviso that one has reasons for doing so. The presence of such reasons would separate this rejection from a rejection due solely to intolerant dogmatism. Here we seem to reach one of those points where Popper wants to distance himself from Kuhn, for Popper seems to believe that Kuhn's vision of scientific activity permits (and even
praises) intolerant dogmatism--i.e., dogmatism without reasons. In other words, on Popper's reading of Kuhn, the failure to keep alternatives alive is not because of the presence of good reasons, but only because of a dogmatic spirit.

But, from Kuhn's perspective, if Popper does recognize that there can be legitimate reasons for failing to keep alternative alive, then he owes us a more detailed account of how this is not only an important characteristic of scientific activity, but actually serves that activity. Kuhn's description of normal science, I will argue, attempts to provide just such an account. I will return to this issue in the last chapter, after the contributions made by Gadamer and Blumenberg have been incorporated into the discussion.

Summary

As we have seen, Popper grants a central place to dogmatism. In fact, we found dogmatism to be at the very heart of revolutionary criticism. For neither Popper nor for Kuhn is dogmatism a passive target for criticism (an acquiescence or complacency in a present, satisfying, and determinable achievement), nor is it a failure to engage in criticism. Criticism remains accountable to the framework it would reject, insofar as it must offer an account of that framework--what it has been, what it has achieved--and to preserve within itself the competencies to match or surpass
those achievements.

We saw that for Popper one has to conjecture when to stop defending a pet theory. Refutation of a theory always includes an element of conjecture. One conjectures that one's determination of what the theory is legitimates trying a new conjecture. But unless the conjecture to reject is irreversible (and Popper says nothing to indicate he believes it is necessarily irreversible), it must remain open to the criticism of the community of defenders. It cannot simply accuse these defenders of an intolerant dogmatism.

What is important about this is that it raises a question about the steady availability of framework-breaking. Popper acknowledges that we are "prisoners caught in the framework of our theories" (Popper 1970:56), though he believes we can break free of any framework at any time. Is it only an "attitude" toward our present condition which decides whether we will passively remain within a framework or break free from it?

It would seem that for Popper more is involved than simply an attitude. Popper would probably say that to be open to criticism is already to be disposed toward breaking free of a framework. But being open to criticism does not mean that one cannot legitimately be a defender of a framework, as Popper admits. Furthermore, being disposed toward breaking free of a framework is not something which
Kuhn rejects (Kuhn 1970b:242). Far less does he believe that normal science immunizes itself against criticism--for Kuhn does leave the door open for the revolutionary impulse at any and all times (248).

If we understand Popper's point about the value of the revolutionary disposition as intending the generation of criticism from outside the framework, then we might understand Kuhn's point as intending the generation of criticism from within the framework. This means not seeing a framework only as an obstacle to progress--as a dogmatic shackle weighing down the critical spirit--but as an enabling condition that makes criticism possible. Failure to recognize the enabling function of a framework, we saw, is the characteristic failure of "enthusiasm." No less than the intolerant dogmatist, the enthusiast immunizes itself from the criticism of that from which it broke.

As we will see in the next chapter, an appreciation for the enabling function of a framework--an appreciation for tradition--is what Gadamer contributes to this issue.
CHAPTER FOUR

GADAMER'S REHABILITATION OF TRADITION

Were we to begin to define Gadamer's notion of tradition in the terms used at the end of the last chapter--terms familiar to Popper and Kuhn--we might identify it initially as a framework which is not just an obstacle to progress, but a positive enabling condition for it. In Kuhn's terms, it is a framework which "must be lived with and explored" (1970b:242). The rehabilitation of our understanding of tradition has been a central feature of Gadamer's hermeneutics.

In this chapter we will begin to see how Gadamer's hermeneutics clarifies the necessary and positive role dogmatism and tradition play in the activity of criticism. First, I will confirm Richard J. Bernstein's interpretation of the parallel between Kuhn's and Gadamer's views on judgment (phronesis), over against John Caputo's conjunction of Kuhn with Jacques Derrida. On Caputo's reading, normal science fully dissolves into a state of revolution, within which state the values once shared by scientists become "utterly ineffective." Kuhn's position, I will argue, suggests no such meltdown of judgment, nor of the conditions supporting it.

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Second, I will argue that Gadamer's analysis of the structure of experience is important for showing how both normal and revolutionary science involve experiences that are both falsifying and confirming. For Gadamer, the process of experience is essentially negative, though falsification can also confirm aspects of tradition. Tradition passes on that which has not been falsified. In Popper's terms, the expectations by which we are guided have withstood every test to which we have subjected them.

Third, the essentially negative structure of experience is articulated through the model of dialogue--through a process of question and answer. "We cannot have experiences without asking questions" (Gadamer 1990:362). We experience through the openness breached by asking a question--an openness which is not unlimited, however: that is, not without horizon or framework (362).

In these three ways Gadamer's hermeneutics develops Kuhn's position on the dogmatism implicit in normal science. The problem which will arise, however, is that Gadamer's hermeneutics leaves little room for the deployment of the concept of revolution--which is a central affirmation of Kuhn's philosophy of science. By way of anticipation of the points to be made in the final chapter, I offer the following assessment of some of the differences between these two.

Although Kuhn's claims concerning the dynamic of normal
science share with Gadamer the belief that criticism can be generated from within a framework, Kuhn departs from Gadamer in insisting that criticism does not simply reform that framework. For Gadamer, the framework of tradition is renewed by means of a fusion of horizons that rises "to a higher universality that overcomes not only our own particularity but also that of the other" (1990:305). For Kuhn, on the other hand, the framework finally becomes incapable of sustaining the very questions and answers it originally asserted to be within its competence, and thereby undergoes a crisis in identity--so much so, in fact, that scientists find it difficult to understand what the framework is: in what its achievements consist, and where its competencies lie (Kuhn 1980:190-1). The dogmatic impulse aims at the restoration of this identity. The revolutionary impulse also serves to identify what the old framework was--and insofar as this is the case, there is considerable overlap in motivation and activity. However, the revolutionary impulse ultimately manifests itself not in the service of reform, but rather in order to identify that over against which it may identify itself. For a hermeneutic development and clarification of these very same points, we will need to turn, in the last chapter, to the work of Hans Blumenberg.

The Dispute over Values

The similarity of Kuhn's position on values and
Gadamer's appropriation of the Aristotelian idea of *phronesis* has been noted by many commentators, most notably Bernstein (1983) and Rorty (1979). As Bernstein points out, *phronesis* is a kind of practical rationality that involves deliberation and choice (1983:54). Moreover, the judgments characteristic of *phronesis* do not so much reflect the application of rules, but rather the application of values. Whereas judgment according to rules involves the subsumption of the particular under the universal, judgment according to values involves the identification and interpretation of the universal appropriate to the particular. Furthermore, just as *phronesis* is developed within a community or *polis*, so for Kuhn the deliberations guided by values are most effective within the practice of normal science.

In what follows I will not challenge these particular similarities, but rather test their limits. Before doing so, however, it may be helpful at this point to briefly introduce the orientation of Gadamer's work and its relevance to the issues developed in the previous two chapters.

The title of Gadamer's main work, *Truth and Method*, is a bit deceiving, insofar as Gadamer does not attempt within its pages to find a method proper to the human sciences--far less to claim to have discovered such a method; rather, Gadamer attempts to show that the concern with a need for "method" provoked an attempt by the human sciences to model
themselves after the pattern of the natural sciences—the "method" of which was seen as one of its most significant accomplishments. The modelling of the human sciences after the fashion of the natural sciences thus arose from a common concern—the foreignness [Fremdheit] we feel in relationship to the world (Gadamer 1990:65).\(^1\) It is important to see that Gadamer is not simply claiming that the methodological aspirations of the natural sciences were transferred to the human sciences, as a model of success. Instead, he is arguing that if such a transference did take place, it is somewhat understandable as a response due to the similarity between their concern with foreignness.

Gadamer's claim is that, particularly in the human sciences, method does not overcome our foreignness, but actually exacerbates it.\(^2\) Particularly in the human sciences, method alienates the knower from her own "historicity," and from her own standpoint within tradition (a point particularly important for the issue of this paper). The primary focus of Gadamer's criticism is this

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\(^1\) Bernstein calls this the Cartesian anxiety (1983:16ff.)

\(^2\) The very deployment of method both contributes to and reveals the contours of our foreignness. Rather than it being the case, then, that we should never have gone down the path of "method," Gadamer seems to be suggesting that following this path has served an important hermeneutical function—has served to reveal hidden dimensions of meaning, or hidden dimensions of our foreignness.
alienation. ³ The upshot of Gadamer's criticism is that the self-alienating concern for method that has characterized both the natural and human sciences has resulted in the relativism of historicism, and the objectivization of tradition into a dead, non-authoritative, museum-piece curiosity. As Richard J. Bernstein characterizes it: we find ourselves in an unenviable predicament between objectivism and relativism.⁴

With this as a background, we can now turn the issue of values and their role in scientific activity.

As I have already pointed out, for Kuhn the concept of crisis plays a central role in his account of the dynamic of scientific activity, a role which is set-off against the stability characteristic of normal science. For Gadamer, there is no such balance. Nor, for that matter, is there for Derrida. To put the issue somewhat formulaically: if for Gadamer phronesis is always operative and always effective, such that one may question if there ever are crises, and if for Derrida phronesis is neither always operative nor always effective, such that crises are prevalent and persistent, then for Kuhn phronesis is always

³ Note, then, that the standpoint for Gadamer's own criticism is a position within tradition, at which point our alienation has been revealed through our own contributions to, and participation in, its intensification.

operative and effective, but not in the same way during crisis as it is during periods of relative stability (normal science).

For Gadamer, the relevance to hermeneutics of Aristotle's analysis of phronesis lies the task of "application" which both share (1990:315). Specifically, both are concerned with the task of "applying something universal to a particular situation" (312). For Aristotle, according to Gadamer, "the task of making a moral decision is that of doing the right thing in a particular situation--i.e., seeing what is right within the situation and grasping it" (317). But the task of application is more problematic than this (since this description of phronesis does not adequately distinguish it from techne): "For we can only apply something that we already have; but we do not possess moral knowledge in such a way that we already have it and then apply it to specific situations" (317). Instead, our knowledge of a law is "productively determined by" its application to a particular case (38). The task of application in hermeneutics is remarkably the same, in that the appropriation of tradition by understanding is a productive determination of its possibilities, by means of its application to our situation.\(^5\)

Although Bernstein's analysis links Gadamer's and

\(^5\) This is why Gadamer calls understanding "participating in an event of tradition" (290).
Kuhn's positions, he does direct the question to Gadamer whether, in fact, **phronesis** can be sufficient and effective in times of crisis. Bernstein points out that **phronesis** presupposes not only the existence of a community, but also the existence of **nomoi** within the community (1983:157). Might we not experience a crisis concerning what norms phronesis should draw upon? Might not the problem of the interpretation (or application) of norms be extended in some cases to the problems of the shared acceptance and stability of the norms themselves (157)? Specification of norms through application is one thing, but might not confusion run so deep as to press the question which norms are the appropriate ones to specify (158)? Such problems, says Bernstein, raise the question concerning the conditions for the operation of **phronesis**.

John Caputo focuses his attack at precisely this point. According to Caputo, there are points in the history of science when the **phronesis** of the scientist will not do, because what that **phronesis** presupposes is no longer effective (Caputo 216). He states,

**Phronesis** functions only within an existing framework, an established paradigm. It is a fundamentally conservative notion in the best sense of that word, that is, it knows how to keep something alive, to renew it in changing circumstances but always within the compass of an established order. It requires a stable paradigm, a more or less fixed order. Aristotle conceived of the functioning of **phronesis** within a fundamentally stable polis, not within a period of revolutionary conflict. (Caputo 217)
In a letter to Bernstein which the latter appended to his *Beyond Objectivism and Relativism*, Gadamer for his part questions whether we are ever in a state characterizable as collapse. He says, "The conflict of traditions we have today does not seem to me to be anything exceptional" (Bernstein 1983:264). He also claims that "the displacement of human reality never goes so far that no forms of solidarity exist any longer." As my presentation of Gadamer's philosophical hermeneutics proceeds in more detail, it will become clear why Gadamer cannot permit, much less imagine, a situation in which the breakdown of community, or the paradigmatic, normative framework, could be complete. The solidarity and consensus of "community" is never simply the accomplishment of the members of the community. The community always points beyond itself and draws upon resources that surpass what the community realizes at any time. The community always stands within a relationship to what Gadamer calls *die Wirkungsgeschichte*, a relationship which above all else must remain "open," and of which we must become "conscious."

According to Caputo, however, Kuhn departs dramatically from Gadamer when he argues that the criteria upon which *phronesis* draws—the criteria characterizing a good theory: accuracy, scope, consistency, etc.—are in times of crisis "utterly ineffective" in resolving such conflict (Caputo 218). In fact, these criteria "are of no
practical use," presumably because they are indeterminate—quite unlike "the determinacy of an Aristotelian virtue" (218). Caputo draws three conclusions from this:

First, rather than search for more determinate criteria with the aid of which phronesis could carry out its function,

...we must confess the play, for what causes phronesis to founder is just the unavailability of criteria. It is only after a free argument has played itself out that we can afterward, with a logic that limps along lamely after the fact, reconstruct what sort of moves reason made which won the day. And it is only afterward, after the conditions for a new wave of normal science have been forged, that phronesis can again have a place...(Caputo 310, n10).

Phronesis is ineffective for deliberation and cannot provide reasons for the choices made. Whatever the nature of the "free argument," it is not phronetic. We can only reconstruct reasons after the fact—presumably from the perspective of the winner.

Second, what is at stake in such times of conflict "is not only a particular paradigm but science itself (219)."

And with this, Kuhn's position becomes very close to that of Derrida. It is what Caputo calls "Kuhn's most Parisian moment (220)."

Normal science flourishes because of its authoritarian practices. But that is not an objection to normal science for Kuhn or Derrida. Il faut la verite. Science flourishes because of the

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Here I refer the reader back to the debate with Laudan. For a more nuanced reading of the "determinacy" implied by Aristotelian virtue, see Gadamer 1990:312-324.
violence with which it enforces its paradigm (which is a Parisian way of saying that the scientific community is organized around a paradigm which it believes in) and because of the violence that the paradigm inflicts on the world (a Parisian way of saying a conceptual framework, a way of seeing things). (220)

Science, then, is characterizable as a "violence"—against both itself, in its communal aspect, and the world. Caputo hesitates here, however, recognizing that this characterization seems closer to Feyerabend than to Kuhn. He therefore puts conditions on the convergence.

If Kuhn thinks that there actually is a time when "normal science" is peacefully settled into place and has the faith of everyone, then a Derridean would suspect that that is a simplification (just as he would suspect Heidegger's epochal units). If, on the other hand, Kuhn thinks that normal science is honored more in the breach than in the observance, then Derrida would not object to this idea. (221)

Caputo recognizes and challenges the position Kuhn might like to adopt here—a position less radical than that of Derrida and considerably more Gadamerian. The challenge is whether there ever is, in fact, a time when normal science actually exists, and, if so, whether such an existence can claim anything more for itself than the violence by which it imposes itself.

Third, for Kuhn the dynamic of science does not lead to a tighter convergence or match between paradigm and reality. What this means, for Caputo, is that "the shift from one paradigm to another does not stem from a deeper insight into nature but from a shift of strategy in coping with the puzzles faced by the scientist, in coping with the
flux (221)." Kuhn "intimates," therefore, that the projections of science are "fictions," which do not disclose the things themselves, but rather issue in "social consensus" and "pragmatic will-to-power" (221-2). In short, Kuhn's position, on Caputo's reading, represents a serious departure from that of Gadamer. If Kuhn wishes to retain proximity to Gadamer's end of the spectrum, then the general challenge to him—as the summation and thrust of the three conclusions given above—is how to account for the dynamic of science, and especially of normal science, as being something other or more than imposed violence.

Kuhn's position, in fact, does retain some proximity to Gadamer's. His claims, to that effect, are two-fold. First, the criteria characterizing a good paradigm are not indeterminate such that they are "utterly ineffective" or "of no practical use" apart from that paradigm. Second, science does not simply impose paradigms on nature by an act of violence only euphemistically called agreement. Nature cannot simply be forced into conceptual boxes, and certainly not arbitrarily so.

Concerning the first point, Kuhn argues that if the criteria were as indeterminate, ineffective, and useless as Caputo suggests, then the objections levelled at Kuhn of "mob psychology" would be warranted. But for Kuhn, "one characteristic of a mob is its rejection of values which its members ordinarily share. Done by scientists, the result
would be the end of their science, and the Lysenko case suggests that it would be.... If the specialists' group behaves as a mob, renouncing its normal values, then science is already past saving" (Kuhn 1970b:263). For Kuhn, then, the indeterminacy of values neither warrants their rejection, nor indicates their practical uselessness and utter ineffectiveness. Furthermore, in the conflict between paradigms, science itself is not at stake, although the nature of a particular science may be. If scientists act like a mob, science would not be "at stake," it would already be at an end and past saving. Finally, if the decisions made during conflicts are given only a "historical character," or "are made only with 'hindsight,'" such that we can only reconstruct the results from the perspective of the victors, then the function of values within the process—upon which Kuhn insists—is denied, and one is left with Caputo's nebulous "free argument."

Concerning the second point, for Kuhn might does not make right. Caputo's interpretation puts great weight upon the statement in The Structure of Scientific Revolutions that normal science is "a strenuous and devoted attempt to force nature into the conceptual boxes supplied by

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7 As we will see, part of Blumenberg's contribution to the issue is that, even if science were somehow at stake at this level, there would still remain a continuity of formal, indeterminate positions which do initially exercise a determinate influence--by taking on a specific meaning content.
professional education" (Kuhn 1970:5). Later in that work Kuhn weakens the claim somewhat, claiming that "that enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies" (24). However, Caputo overlooks certain other attempts made by Kuhn to clarify his position. Kuhn denies that scientists "first decide what they agree about and then enforce it both on their colleagues and on nature" (Kuhn 1970b:260).

Later in the same essay Kuhn states,

By the same token, no part of the argument here or in my book implies that scientists may choose any theory they like so long as they agree in their choice and thereafter enforce it. Most of the puzzles of normal science are directly presented by nature, and all involve nature indirectly. Though different solutions have been received as valid at different times, nature cannot be forced into an arbitrary set of conceptual boxes. On the contrary, the history of proto-science shows that normal science is possible only with very special boxes, and the history of developed science shows that nature will not indefinitely be confined in any set which scientists have constructed so far. (Kuhn 1970b:263)

Kuhn's position is either incoherent or more complex and delicate than Caputo suggests.

Rather than seeking a balance between Gadamer and Derrida, it might now appear that Kuhn's position is not only proximal to Gadamer's, but actually squares with it. If the shared values are not rejected, then whereof does one speak of "crisis?" Are scientific values a firm underlayer of support, so that crisis (or revolution) is only a
surface-level phenomenon, or perhaps even only the proverbial "matter of (subjective) perception?" What makes Kuhn's position on values any different from that of Gadamer? What led Caputo, moreover, to believe that for Kuhn such values were utterly ineffective and of no practical use? Could Caputo have been so mistaken about Kuhn's position?

The mistake may lie in Caputo's characterization of utter ineffectiveness. For Kuhn, the values are not utterly ineffective, although they may not be persistently and substantially effective in their applications within scientific practice. Both the mistake of Caputo's interpretation and the difference from Gadamer's position rests in the problem of the function of values. The indeterminacy of values does not simply warrant their rejection. Such indeterminacy, rather, must lead one to focus on their function both within normal science and the crisis situation. The shared values upon which phronesis draws have an "abstract-universal" moment (ala Hegel)--but the determination of their meaning carried out independently of a given historical moment or scientific community is not very full or rich.

More importantly, a focus on the "substantive" development over time of a particular value's meaning cannot account for all the specific determinations made through that time frame. One must turn to the function of the value
within the process of deliberation and decision in order to uncover the rationality operative in scientific activity, as well as the place of values within it. So, for example, a "dogmatic" stance—whether about a theory or a value—is not simply adopted at will, but rather serves a necessary function within scientific activity. Although Kuhn sometimes speaks of this function as "only" a strategy, he also claims that it is essential to scientific activity and progress. As essential, it is not "simply" a sociological characterization of community-structure. The function dictates the adoption, not only of the "dogmatic" attitude characteristic of normal science, but of the "crisis" attitude as well.

How then does Kuhn's position differ from Gadamer's? Fundamentally in that Gadamer's hermeneutics leaves little room for the deployment of idea of revolution. Kuhn does account for the possibility of crises in the identity of the community (tradition) and revolutionary re-constitutions of such identity (as well as of the values which will guide that community). However, such a crisis does not mean that the values once shared by the community have become utterly ineffective. Instead, as Blumenberg suggests, they now gravitate around positions central to determining what the community was (what the old paradigm was—what content occupied these positions), and what the new community purports to be (what content now reoccupies these
positions). In short, as we shall see in the next chapter, the kind of meaning-content that comes to occupy a particular position—the kind of substitution that becomes possible (and which is part of the meaning of the word "paradigm")—can have a revolutionary effect on the application of values (their selection and specification).

The challenges issuing from both Gadamer's and Derrida's positions should now be clear. Kuhn's attempt to maintain a balance which incorporates both stability and instability into the process of scientific activity is challenged from both sides: by the position which sees all such stability as imposed violence, and by the position which sees any such instability as something considerably less than crisis.

Gadamer's Analysis of the Structure of Experience

Two sections of Truth and Method are particularly important for showing how both normal (and revolutionary) science involve experiences that both falsify and confirm various aspects of tradition—such that confidence in tradition is not simply affirmed dogmatically, without reasons, but is confirmed by continuing experience.8 The first section, "The Concept of Experience and the Essence of the Hermeneutic Experience," develops Gadamer's

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8 I refer the reader here to the discussion at the end of the last chapter.
understanding of the "negativity" of experience. The second section, "The Model of Platonic Dialectic," develops the idea of an "openness to" experience, provoked by its essential negativity, by means of the articulation of a model of critical dialogue. In short, the falsification and confirmation of tradition is an achievement of dialogue.

According to Gadamer, the process of experience is essentially negative (353). In this regard Gadamer himself acknowledges a relationship to Popper's idea of conjecture and refutation (353n.299). The word "experience" has two different senses for us in everyday use: there are the experiences that conform to our expectations, and the new experiences that occur to us. The former sense of experience relates to repetition and confirmation. These allow us to predict what was previously unexpected. The latter sense of experience--the sense Gadamer calls "genuine"--is the negative sense: the sense which affirms that our expectations are violated. Gadamer says, "If a new experience of an object occurs to us, this means that hietherto we have not seen the thing correctly and now know it better. Thus the negativity of experience has a curiously productive meaning (353)." In Popper's terms, one learns that one was mistaken.

New experience is negative experience. "Only something different and unexpected can provide someone who has experience with a new one (353)." Negative, productive
experience can only occur once. Having occurred, at this point repetition of the experience supports the negative experience. In other words, experience in the former of the two senses spoken of above takes over from and confirms experience in the latter sense. The "taking over" should be understood as a process: experience is engaged in a cycle of expectation and violation, of hope and disappointment.

But Gadamer goes further. Even a cyclical understanding of the process of experience might lead one to believe that repetition and confirmation is the privileged member in the process: "It is true, of course, that part of the nature of experience is to be continually confirmed; it is, as it were, acquired only by being repeated" (353). Nevertheless, Gadamer says that negative experience is "experience is the genuine sense" (353). In other words, negative experience is more than a troublesome occasion or obstacle keeping one from getting on with the process of repetition. It serves as more than a "corrective" to the process. The reason why we might see negative experience as only a troublesome obstacle, according to Gadamer, is that we are tempted to conceive of experience in terms of something that surpasses it, or in terms of the end or final state of experience—a state in which experience itself would be obviated. Instead of succumbing to this temptation, we should see that "the truth of experience always implies an orientation toward new experience. That is why a person who is called experienced
has become so not only through experiences but is also open to new experience (355)."

The end of experience consists in being experienced. Being experienced consists in openness. Being experienced means to be open to new experience--expecting violations, expecting the unexpected. "The consummation of his experience, the perfection that we call "being experienced," does not consist in the fact that someone already knows everything and knows better than anyone else. Rather, the experienced person proves to be, on the contrary, someone who is radically undogmatic; who, because of the many experiences he has had and the knowledge he has drawn from them, is particularly well equipped to have new experiences and to learn from them. The dialectic of experience has its proper fulfillment not in definitive knowledge but in the openness to experience that is made possible by experience itself" (355). Negative experience is "genuine" experience insofar as "every experience worthy of the name thwarts an expectation (356)."

Genuine experience reverts back upon the being of the one who is experienced. "Experience is experience of human finitude (357)." It is "that whereby man becomes aware of his finiteness (357)." "Genuine experience is experience of one's own historicity (357)." In negative experience, we experience our finitude insofar as our expectations are violated, our hopes are disappointed, our plans go
unfulfilled. "The experienced man knows that all foresight is limited and all plans uncertain (357)." Gadamer continues,

In him is realized the truth value of experience. If it is characteristic of every phase of the process of experience that the experienced person acquires a new openness to new experiences, this is certainly true of the idea of being perfectly experienced. It does not mean that experience has ceased and a higher form of knowledge is reached (Hegel), but that for the first time experience fully and truly is. In it all dogmatism, which proceeds from the soaring desires of the human heart, reaches an absolute barrier. Experience teaches us to acknowledge the real. The genuine result of experience, then—as of all desire to know—is to know what is. But "what is," here, is not this or that thing, but "what cannot be destroyed" (Ranke) (357).

In this passage Gadamer's Popperian affinities shine through. The experienced person is the one whose posture is open, a stance that could easily be appreciated by the author of The Open Society and Its Enemies. Furthermore, the result of experience is to know what is, what cannot be destroyed—which echoes Popper's structure of conjecture and refutation. That which stands against all of our efforts to refute it—which remains inviolate—is that which can be characterised as what is.

But more needs to be said about the structure of openness, and this can be accomplished by means of a

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Although, as pointed out earlier, one must not overlook the "suffering side" of experience which Gadamer affirms as essential to "openness," one example of which we discussed earlier—the openness required to suffer through a debate with tradition.
question directed at Gadamer's understanding of negative experience: Does Gadamer recognize, as Kuhn and Popper do, that not every negative experience is falsifying? Of course, Gadamer could say that an experience is identifiable as negative only if it does falsify, but this would not answer the question of how to discriminate between an experience which is not falsifying (and yet which could not be said to be confirming) and an experience which could be said to be falsifying. Could it be that Gadamer's interpretation of negative experience remains "naive?"

Gadamer's analysis of the structure of experience makes the claim that negative experience is primary. Genuine experience is bilateral: we not only call something into question by means of our expectations but we ourselves are called into question by the violation of those expectations. The transition is thereby made from the analysis of experience to the logic of question and answer.

In the section entitled, "The Model of Platonic Dialogue," Gadamer begins an analysis of what he will later call the logic of question and answer. Gadamer recognizes that what he has characterized as negative experience is a variation on what Hegel called determinate negation. But Gadamer differs from Hegel insofar as he insists that experience does not have "knowledge" as its final state—that is, the end of experience is not knowledge, and the perfection of experience is not perfect knowledge, in which
experience would be obviated; rather, the perfection of experience is being perfectly experienced, and that means being open to experience--knowing that one does not know. To help him explicate the logical structure of the openness to experience, Gadamer turns to Socrates.

Being open to experience--knowing that one does not know--has the structure of a question. We saw earlier that for Gadamer negative experience consists in the violation or disappointment of expectations. The question which arose at that point was whether Gadamer's understanding of negative experience is naive. Gadamer states,

> We cannot have experiences without asking questions. Recognizing that an object is different, and not as we first thought, obviously presupposes the question whether it was this or that. From a logical point of view, the openness essential to experience is precisely the openness of being either this or that. It has the structure of a question. And just as the dialectical negativity of experience culminates in the idea of being perfectly experienced--i.e., being aware of our finitude and limitedness--so also the logical form of the question and the negativity that is part of it culminate in a radical negativity: the knowledge of not knowing. (362)

We might say then, that the openness characteristic of genuine experience represents a moment of hesitation, through which we raise a question. It represents an intermediate stage between the steady course of old expectations that are confirmed and the disconfirmation that alters that course. This hesitation indicates that we know that we do not know, and this hesitation takes the form of a question. Therefore, it is not simply negative experience
which calls our expectations into question.

It is not clear here, however, whether hesitation precedes experience or is called forth in experience (or is simply the "culmination" of a lifetime of experience). Does one approach every experience with hesitancy, or is it only a negative experience that calls forth hesitancy—a hesitancy which keeps one from the precipitancy of a hasty conclusion?

Gadamer states that "the structure of the question is implicit in all experience. We cannot have experiences without asking questions" (362). This would seem to imply that being open to experience means always approaching experience with questions. Gadamer follows up with the claim, "Recognizing that an object is different, and not as we first thought, obviously presupposes the question whether it was this or that." Recognizing an experience as negative, as a violation, requires that one approach experience with questions. One could not recognize difference if one was not open to it. In other words, one does not first recognize difference or violation, and then assume the stance of the questioner. Nor does one first recognize difference, and then hesitate, only raising a question as the result of such hesitation.

On one hand, then, we always bring questions to experience: we are always open to whether something is this or that—we are, that is, open to alternatives. Yet
positive experience does not question us in turn; only negative experience does. This reciprocal questioning is what makes experience genuine, and productive. Being open to experience means that one is oriented toward expecting the unexpected. What then is the point of hesitancy? It keeps one from being precipitant?--It allows alternatives to come into view.

**The Model of Platonic Dialogue**

Being open to the negativity of genuine experience corresponds, according to Gadamer, to the problem of asking questions. The great insight of the Platonic dialogues, according to Gadamer, is that they show that it is more difficult to ask questions than to give answers. The difficulty with asking questions is that one must determine which are the right questions to ask. Questions give a conversation direction, so the problem is: how can one guide the conversation in the right direction. How does one find the right questions to ask? Gadamer states, "All questioning and desire to know presuppose a knowledge that one does not know; so much so, indeed, that a particular lack of knowledge leads to a particular question (366)."

Being open to what is in question means recognizing that the answer is not settled. The answer is questionable. Gadamer, therefore, is not espousing "open" questions, if "open" is taken to mean questions that are boundless. A question must be concretized--it must become a particular
question.

Concretizing the question involves seeing the answer to the question as something that is not settled, as something that is not "decisive" (363). Recognizing this means recognizing that the answer lacks something. The goal of questioning is to find a question which will help one make up (at least part of) this lack.

When Gadamer takes his next step, however, he introduces a shift: whereas questioning presupposes that one does not know, the difficulty that faces the person trying to ask the right question "lies in knowing what one does not know" (366). If one is going to address a particular lack of knowledge through a particular question, then one needs to know something of what is lacking about the knowledge (the answer) one has available. If a question is not going to be a floating question, but is to be concrete or particular, then it will have to speak to the available answer. Gadamer continues, "Plato shows in an unforgettable way where the difficulty lies in knowing what one does not know. It is the power of opinion against which it is so hard to obtain an admission of ignorance. It is opinion that suppresses questions. Opinion has a curious tendency to propagate itself....How, then, can ignorance be admitted and questions arise?" (366).

Gadamer is not saying that opinion is unwilling to admit ignorance in general, as a possibility, but rather that it
is unwilling to admit ignorance at any particular point. Opinion may be willing to admit the possibility of error, but finds it difficult to identify at what point error occurs. It is not so much that opinion suppresses the possibility of questions, so much as opinion suppresses the application of any particular question. Gadamer refers to this as the "smooth front" of popular opinion: There may be many questions, but they are aligned in a front of equal urgency; there is no way of determining which among them, if any, is the right question to pursue.

In the face of this situation, Gadamer accounts for the way a question arises in the following terms.

Let us say that [a question] can occur only in the way any idea occurs to us. It is true that we do speak of ideas occurring to us less in regard to questions than to answers--e.g., the solution of problems; and by this we mean to say that there is no methodical way to arrive at the solution. But we also know that such ideas do not occur to us entirely unexpectedly. They always presuppose an orientation toward an area of openness from which the idea can occur--i.e., they presuppose questions. The real nature of the sudden idea is perhaps less that a solution occurs to us like an answer to a riddle than that a question occurs to us that breaks through into the open and thereby makes an answer possible. Every sudden idea has the structure of a question. But the sudden occurrence of the question is already a breach in the smooth front of popular opinion. Hence we say that a question too "occurs" to us, that it "arises" or "presents itself" more than that we raise or present it" (366).

A question, then, breaks through into the open, breaching a front. The question overpowers the power of opinion to suppress it. But does the occurrence of the question open its own critical space? Is it "entirely
unexpected?" The subtlety of Gadamer's position is worthy of investigation.

In the passage, Gadamer compares the occurrence of a question to the occurrence of an idea. But, as he says, the occurrence of an idea presupposes questions. An idea's occurrence is not entirely unexpected. Gadamer then compares an idea to a question. An idea has the structure of a question. But if this is so, then doesn't a question which arises in the face of opinion presuppose questions already in play (just as an idea presupposes the area of openness marked out by questions)? The question which arises presupposes other questions. The newly-arisen question needs the openness which questions already have provided. The questions already in play have provided that openness, which makes the appearance of the new question not entirely unexpected.

It seems possible, then, that opinion is not entirely closed off to questioning. It is possible that the nature of opinion makes room for questions. It is possible that the nature of opinion makes it not entirely unexpected that questions will arise. Either that, or the questions that are already in play have their source elsewhere. But that means not only that questions are always already in play, opening spaces in opposition to opinion, but even more importantly for this discussion it means that Gadamer has not accounted for how ignorance is admitted and questions
arise. It would seem that ignorance in some sense is always admitted and questions have already arisen. But to tie this back to our point of departure, this would mean that in some sense we already know that we do not know. But would admitting ignorance and the existence of questions already in play mean that we know, in some sense, what we do not know? In terms of Gadamer's concept of experience, would even our opinion have experienced negation--so that it is in some sense open, wanting to know; or, to take the other alternative, despite our opinion, we have experienced negation--so that we are in some way in conflict with ourselves: the closedness of opinion being in constant conflict with the openness of another side of ourselves (the side which wants to know)?

Judging by what Gadamer says, he does recognize a conflict between closedness and openness, no matter where that conflict might be located. He says, "there is something peculiar about this art [of questioning]. We have seen that it is reserved to the person who wants to know--i.e., who already has questions. The art of questioning is not the art of resisting the pressure of opinion; it already presupposes this freedom" (366). Questioning arises because opinion has already been resisted. So opinion is not openness--it does not expect questions or challenges. Opinion is not engaged in the search to know. Gadamer continues on the next page: "As the art of asking
questions, dialectic proves its value because only the person who knows how to ask questions is able to persist in his questioning, which involves being able to preserve his orientation toward openness" (367). Opinion, then, is that which threatens to overcome questioning. But the freedom to resist this threat--that resists this threat--is always presupposed by questioning. Only if we do not persist in questioning does the freedom to question become concealed.

However, Gadamer suggests that such persistence is not an easy matter. In fact, it is quite difficult, for he says that "only the person who knows how to ask questions is able to persist..." But that is precisely the problem with which we began: how does one determine the right questions? Here it is no longer sufficient to say that, "A person skilled in the 'art' of questioning is a person who can prevent questions from being suppressed by the dominant opinion" (367). Prevention of suppression is a rather weak indicator of one's knowledge of the art of questioning. In fact, there is a circularity here, insofar as Gadamer states that only the one who knows how to ask questions will persist, and yet the proof of that knowledge is in the persistence of questioning in the face of suppression. For Gadamer this circularity is not vicious, but rather hermeneutic, and it is inherent in all experience: only by being open will we truly experience; but, given our situatedness within a tradition that constitutes the medium of our experience, it
is only by experiencing (interpreting, applying), that we attain openness toward possibilities.

How then, does questioning proceed? In this context Gadamer affirms, as a condition for genuine dialogue, the requirement that the real strength of the other person's position must be brought out. He says

To conduct a conversation means to allow oneself to be conducted by the subject matter to which the partners in the dialogue are oriented. It requires that one does not try to argue the other person down but that one really considers the weight of the other's opinion......A person who possesses this art [of questioning] will himself search for everything in favor of an opinion. Dialectic consists not in trying to discover the weakness of what is said, but in bringing out its real strength. (367)

Bringing out the real strength of the other's position proceeds by means of a process which legitimates and then limits. In other words, one seeks to discover and acknowledge the truth and power of the other's position, but then correspondingly delimits the scope and depth of its applications. Both movements are part of the determination of "real strength." Delimitation, in fact, is the determination of the question to which the position is an answer--a determination of the question that this answer is "in a position" to answer.

Summary

It may seem odd that Popper and Gadamer would agree that genuine criticism (or dialogue) requires one to bring out the strengths of the other's position, and yet draw such
very different conclusions about the implications of this requirement for the possibility of revolution. It would seem less odd if one were to take note of the fact that Gadamer also sees little promise in the idea of progress. The limits of the applicability of Gadamer's hermeneutics to Kuhn's philosophy of science may be reached when one runs up against this idea. As we have seen throughout this paper, the entire issue of the complementarity of tradition and revolution is deeply vested in the issue of progress. For Popper, scientific advance is achieved through perpetual revolutions. Kuhn himself claims that his understanding of scientific revolutions is not meant to imply that no claim to progress can be made. Gadamer, on the other hand, warrants no such deployment of the idea of progress. At best tradition is reformed and restored, and parochial prejudices are overcome. Experience is not progressive, and none of its achievements warrants the name "progress." The truth that is appropriated through the critical reception of tradition must be won anew by each generation. There is, says Gadamer, no progress, but only participation (Gadamer 1986:6).

One must turn to Hans Blumenberg to find a hermeneutic philosophy that makes room for the idea of progress (though as we shall see, it is "relative" progress: progress relative to preceding problems). His position, we will see, denies that every overcoming of communication breakdown is
ultimately only a restoration and reconfirmation of tradition, a shedding away of parochial prejudices by means of their engagement through dialogue.
The last chapter ended at the point where Gadamer affirmed, as a condition for genuine dialogue, the requirement that the real strength of what the other has said must be brought out. Only when interlocutors do not focus simply on exposing weaknesses, but instead give due weight to the strengths of each other's opinion—that is, enter into a genuine conversation—can a conversation be properly led by, and oriented to, the matter at issue (die Sache) handed down by tradition. The question which will be addressed by the end of this chapter, however, is: does the satisfaction of this requirement actually suppress crises and revolutions?¹

Hans Blumenberg develops Gadamer's philosophical hermeneutics, I will argue, in the direction of its possibilities for underwriting the concepts of crisis and revolution.

¹ As we shall see, the "suppression" spoken of here does not only pertain to the present--i.e., the suppression of our own revolutionary or critical impulses--but also pertains to the past: do we, as participants in a Gadamerian conversation, suppress our openness to revolutionary phenomena of the past, that is, our openness to contrary, dissident voices? Do we perhaps even fail to hear the revolutionary, or emancipatory, claims of the voices we do attend to?
revolution. To support this claim, I will begin the chapter by exploring the one facet of Gadamer's position that seems to open the space for the kind of operation that is required for revolutionary claims--namely, his understanding of the "classical." In short, I will forge a link between Gadamer's description of the operations of the classic text and Blumenberg's description of the operations of a revolutionary text.

Gadamer's concept of the classical, however, has been subjected to much criticism. Insofar as Gadamer's idea of the classic seems to foster the image of tradition as "a self-activating movement of imperishable substances" (Jauss 1982:64), his position can easily be understood as one which makes us ideological supporters of traditions and texts.2 The superiority of the classic consists in its privileged relationship to the matter at issue, on the basis of which privilege it can elicit this matter into presence directly before us. Gadamer's position leaves us with little choice, according to this criticism, but to submit to such a superior power of access.3

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3 Note the parallels between this criticism of Gadamer and that levelled by Popper at Kuhn: The 'normal' scientist, in my view, has been taught badly. I believe, and so do many others, that all teaching on the University level (and if possible below) should be training and
Whether these criticisms are justified or not, I will argue that Blumenberg's position develops Gadamer's hermeneutics in a direction which can avoid them, while simultaneously affirming the intuitions Gadamer expresses about the operations of the classic. This development, furthermore, makes way for the deployment of the concept of theoretical revolution, which can support and strengthen the Kuhnian conception of revolution.

**Gadamer's Understanding of the Classical**

In the section of *Truth and Method* in which he analyzes the "Example of the Classical" (Gadamer 1990, 285-90), one gets a glimpse of what Gadamer means by the historical nature of understanding. He states, "We might say that the classical is a truly historical category, precisely because it is more than a concept of a period or of a historical style, and yet it nevertheless does not try to be the concept of a suprahistorical value" (287). In speaking of the classical, one must avoid the two extremes of reducing it merely to a style characteristic of a period of history, or of projecting it into a "supra-historical" realm. Between these extremes lies Gadamer's understanding of the essence of the historical.

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encouragement in critical thinking. The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. (Popper 1970:52-3)
[The classical] does not refer to a quality that we ascribe to particular historical phenomena but to a notable mode of being historical: the historical process of preservation that, through constantly proving itself, allows something true to come into being. It is not at all the case, as the historical mode of thought would have us believe, that the value judgment which accords something the status of a classic was in fact destroyed by historical reflection and its criticism of all teleological construals of the process of history. Rather, through this criticism the value judgment implicit in the concept of the classical acquires a new, special legitimacy. The classical is something that resists historical criticism because its historical dominion, the binding power of the validity that is preserved and handed down, precedes all historical reflection and continues in it. (287)

The qualitative judgment which describes a text as being "classic" does not identify a suprahistorical quality that surpasses or escapes its historical mode of being. Neither does this judgment restrict the "classic" to an expression of a particular historical period or style. Instead, the "classic" refers to a historical "process of preservation" in which the text retains the "binding power of its validity" through the movement of history. The power of the historicist critique was that it undermined the dogma of suprahistorical values. Its weakness, however, was that it reduced all valuations to the historical concepts of "period" and "style."

Gadamer is not a traditional historicist, although he maintains the importance of the mode of being historical. For him, the qualitative judgment which describes a text as "classic" recognizes that, precisely through the historical
process and as a historical phenomenon, the "classic" presents itself to consciousness as "something enduring," which "preserves itself precisely because it is significant in itself and interprets itself"--"it says something to the present as if it were said specifically to it." The text presents a "significance that cannot be lost and that is independent of all the circumstances of time--a kind of timeless present that is contemporaneous with every other present." Gadamer goes so far as to say that, "the classical, then, is certainly something 'timeless,' but this timelessness is a mode of historical being" (288, 290).

Gadamer is struggling to articulate a position between the alternatives of the "suprahistorical" and the "historicist." To that end he uses language borrowed from the tradition of each, even when it leads to paradoxical formulations or prima facie contradictions. Only by recognizing his struggle can one attempt to understand a

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4 The term "historicist" means something different for Gadamer than it does for Popper. By "historicism" Gadamer understands the tradition represented by someone like Dilthey, for whom the task of historical understanding would involve the recovery or reconstruction of the original life-world of a particular historical agent, having as its aim understanding that agent as she understood herself. As I suggested in the last chapter, the self-transposition that is required by this task arises from the attempt to overcome our foreignness with respect to the past. The negation of temporal distance, which the accomplishment of this task implies, follows upon the negative evaluation of such distance as being only an obstacle to understanding. A central feature of Gadamer's hermeneutics (a feature he shares with Kuhn, as I pointed out earlier) is the affirmation of temporal distance as a condition enabling understanding, not simply blocking it.
statement like, "timelessness is a mode of historical being," as being something other than nonsense.

The complexity of his struggle is indicated by the use he makes of the concept of the "world" of the classical and our "belonging" to it (290). In positing the classical phenomenon as being historical, Gadamer fights against the historicist tendency (characteristic of Dilthey) to reduce the understanding of that phenomenon to the reconstruction of a past world. "Our understanding," he states, "will always retain the consciousness that we too belong to that world, and correlatively, that the work too belongs to our world" (290). Because Gadamer wants to recognize the "constitutive" character of the historical without being historicist, and simultaneously recognize the "timelessness" of the classical without resorting to the suprahistorical, he interprets the "world" of the classical in a manner which permits the needed elements of each extreme to function together--specifically, the "process-like" character of the historical and the "self-preserving" character of the suprahistorical are conjoined in our recognition of the "classical" world. This "world" is subject to historical processes, and yet is preserved in its historical mode of being, such that this world is also "our" world. The classical proves itself through a process of preservation that "speaks directly" to past and present "worlds." In fact, this power to speak directly is "fundamentally
This exposition of Gadamer's position may be misleading, however, insofar as the stress has been placed too heavily on the "objective" side of the problem. "World" should not be understood as an "object"—especially as a "historical object" for historical consciousness. Such an understanding would lead back once more to either the suprahistorical extreme or the historicist extreme, each of which Gadamer wants to avoid. To that extent, both the suprahistorical and the historicist positions stem from a common objectivism. Central to Gadamer's struggle to avoid this objectivism is the idea of "belonging."

He says, for example, that "cultural consciousness manifests an ultimate community and sharing with the world from which a classical work speaks" (290). This world, for consciousness, is something in which we "share" and with which we are in "community." It is not first of all an object over against us. The world is carried along with the work, and the work opens its world before it. We belong to that world just as the work belongs to our world. The historical movement which constitutes "belonging," in fact, characterizes understanding itself. "Understanding is to be thought of less as a subjective act than as participating in

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5 To understand the world as an "object" is a symptom, in Gadamer's mind, of the objectivism characteristic of the Enlightenment. For additional comments, the reader is referred back to the introduction of Gadamer's hermeneutics at the beginning of Chapter Three.
an event of tradition, a process of transmission in which past and present are constantly mediated" (290). Understanding participates and shares in a process of transmission--it is neither the intuition of a suprahistorical realm nor the objective delineation of unique historical periods. Both alternatives rest upon the supposition of human nature as an "unhistorical substratum." Therefore, not only does the classic refer to a "notable mode of being historical," but human nature itself refers to a "notable mode of being historical" (287). Both the classic and the human stand between the suprahistorical and historicist extremes.

This, then, gives one some idea of what Gadamer means by the idea of "belonging." The classic text, its world, our world, we ourselves belong to a tradition--a process of transmission in which each of these elements is constantly "mediated." This idea of "mediation" tempers the power of a text to "speak directly." Attributing such power to a text may lead to the misunderstandings of the suprahistorical extreme. The classic text may speak directly to our world because it belongs to our world--because it belongs to a process of transmission in which our world also shares and participates. But the "voice" of the text must be mediated for our understanding through the activity of our understanding. In other words, our understanding is not passive.
In this regard Gadamer appeals to the "hermeneutic circle." In the hermeneutic circle, our understanding is active in anticipating the meaning of the text. Such anticipations, however, do not originate in an activity of subjectivity, but rather proceed "from the commonality that binds us to the tradition" (293). The anticipatory activity of understanding, therefore, is a projection of meaning--but such projection is grounded upon the appropriation of the possibilities inherent within the tradition. In short, our understanding both appropriates meanings from the tradition and projects meanings upon the traditionary text. In so doing, our understanding "participate[s] in the evolution of tradition, and hence further determine[s] it" (293). For Gadamer, then, tradition is not a "permanent precondition," but is a process of transmission in which we participate and which we produce.

Problems with Gadamer's Model of the Classical

Critics such as Hans Robert Jauss (1982) have identified two main problems with Gadamer's concept of the classical: First, on this understanding of the classic, it would seem that the address the classic text makes to us stands outside of the logic of question and answer that Gadamer is at such pains to demonstrate. The classic text would be different from other texts insofar as the interpreter would not need to "seek" the question to which the text itself is the answer--the question would seem to
have always already delivered and validated itself as a matter with which we should be concerned. Second, this understanding of the classic conflicts with his own conception of the role of effective history in understanding. Specifically, in so elevating the status of the classic, does not Gadamer undermine the tension—that is, the temporal distance⁶--between the text and the present that he affirms for the relationship of every other text to a present? (Jauss 1982:29-32)

Gadamer's possible response might begin by rejecting the suggestion that, in appropriating the classic, we turn directly to it for answers to our questions. Not only is Gadamer sensitive to questions of anachronism (reading things back into the past in order to make that past seem less foreign) and distortion (ripping the text out of its historical horizon so that it may be free to speak to us), he is also not suggesting that our relationship to any text--classic or traditionary--is primarily "therapeutic."⑦ If the classic still has something to say to us, then its

⁶ We saw earlier that Gadamer considers temporal distance to be not merely an obstacle to understanding (and a cause of communication breakdown), but an enabling condition. The peculiar nature of temporal distance therefore makes it a productive ground for understanding. The criticism forwarded here is that Gadamer is forsaking the productivity for understanding of a tension he affirms elsewhere.

⁷ By "therapeutic" I mean the suggestion that a classic serves us best when it aids us in treating the questions that trouble us.
claims do not simply provide answers to our questions. When Gadamer says that the classical says something to us as if it were said specifically to us, he is not suggesting that the classic text speaks directly to our questions. In a sense, our questions are inappropriate if we use them as the means to directly hear the classic text in the claim it wants to make (although one should keep in mind here Gadamer's analysis of "prejudice"). Therefore, the "classic" status of a text is not definable as: "the text's ability to speak directly and meaningfully to our present-day questions and concerns." The classic is no more capable of this than is any traditionary text. What, then, is the classic status of the text?

A classic text is a text which consistently demonstrates its power to question us directly. The classic, I would suggest, can even overpower its effective history insofar as that effective history stands between us and the text. The classical, then, is different from traditionary texts insofar as it operates despite the operations of its effective history. It speaks directly to the present. The classic breaks down effective history's grip upon us (although that operation is itself a re-affirmation of the text's effectivity, and thus its effective history).

If this is the way that Gadamer can be understood, however, then other questions begin to arise. Specifically,
it begins to appear that Gadamer is claiming that a classic text uproots us from our own concerns and forces its own claims upon us—our response to which can only be submission. It is a provocation to which we must respond. The classic text, it would seem, enjoys its own relationship to truth, which may generate a revolutionary response on our part. In breaking down tradition, the classic is actually breaking down that which first conditions our cares and concerns about what matters (die Sache).

Gadamer himself would probably be more comfortable with a somewhat weaker claim: what actually occurs is that the classic text "delivers" die Sache—the matter at issue—to us. Nevertheless, rather than participating with us in a dialogue led by what is at issue, on this reading the classic text would demonstrate its superiority and power by this capacity to elicit die Sache into presence. How are we to account for this power? How is this power of delivery compatible with the other aspects of Gadamer's hermeneutics: those which, for example, stress the process of dialogue and mediation? On this reading our understanding of the classic achieves a mediation that has not been required to suffer

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8 This description of the operations of a classic text bears a striking similarity to Popper's reference to the "problem" that conditions our revolutionary conjectures. The similarity to Kuhn is not nearly as striking: Kuhn's reference to normal science generating "anomalies" that may provoke crisis and revolutionary responses is comparable, but, as we shall see, it is more compatible with Blumemberg's position.
the pains of the process--although it was precisely this which is demanded of Popper!

Does Gadamer threaten the integrity of his own hermeneutics? It may very well be that he has introduced an unnecessary tension between two claims that, from another perspective, are more compatible than may immediately be realized. If Gadamer has distinguished too cleanly between the operations of classical texts and those of traditionary texts (1990:577), then it may be because the threat he perceives is the kind of historicism that makes them indistinguishable once more.

Hans Blumenberg's work, I suggest, offers a remedy to this unnecessary tension. His position distinguishes between a classic and a traditionary text insofar as the former can provoke a revolutionary response, whereas merely traditionary texts provoke responses that fall within the parameters of "normal" research (either scientific or interpretive). If we look to the past of a particular culture for guidance, it is a past that, in the words of Arendt, exemplifies something universal. This timeless past can be invoked by revolutionaries to liberate the present from an oppressive and parochial past. (Ingram 1995: 357)

Arendt herself speaks at one point of the "definite, though undefined" past to which the American revolutionary Thomas Paine appealed as authorizing "revolution" (1990:45).
of text still requires the operation of the logic of question and answer. How does Blumenberg carry this out?

We can forward two versions—a strong and a weak—of Blumenberg's general claim, in terms still very close to those of Gadamer, in order to get a sense of his development of this issue. The strong version would say that it is not that Plato, for example, speaks to us only through, or as conditioned by, tradition, but also speaks despite the tradition that normally mediates our understanding. The weaker version would suggest that Plato can still take a meaningful stance within the context of die Sache, a stance that is not simply a repetition of what we have already taken him to say. Said more positively: we can still discover an element in Plato that wants to say something over and above what the tradition has already taken him to have said. Both versions express the claim that Plato can speak to us despite tradition, but with important differences.

For Blumenberg, the undiscovered element in Plato is not a case of surplus meaning, unless we mean by that only that Plato remains a conversation partner whose position is never wholly taken up into, nor rejected from, our own. He remains a conversation partner who opposes us, yet opposes under the guidance of a matter at issue in which we too "participate". Plato therefore challenges our understanding of the issue—and the more he challenges the more he
undermines, deflects, and transforms his own effective history (which itself has conditioned our understanding both of the issue and of his position on the issue).

This way of stating things is noteworthy if for no other reason than it points out the "negative" aspect of tradition and effective history. Effective history can stand between us and the claims of die Sache as much as enable us to hear that claim. Similarly, when Gadamer claims that we are not emancipated from effective history simply by being made conscious of it, that does not mean that "emancipation" is thereby excluded from Gadamer's lexicon (Gadamer 1976:34). In light of Gadamer's appreciation for the negativity of experience, it may be more accurate to say that tradition is at least as much a handing down of falsified understanding as it is a handing down of beliefs we would like to confirm.

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10 Some interpreters have portrayed Gadamer as having an exclusively, or at least excessively, rosy picture of tradition. But the aspects which indicate that effective history is sometimes operated against— even if it is not a condition from which we could be totally emancipated— point to a more balanced view of Gadamer. If Gadamer can be credited with a rehabilitation of effective history, this should not be taken to mean that the enabling function of effective history for understanding is now its exclusive function. An exclusively "enabling" function too easily legitimates an "appreciation" for the past that could so "charm" or "captivate" us that we quickly find ourselves submitting to the concerns of that past. (Although Gadamer does use these terms (1990:490).

11 As Paul Ricoeur points out in "The Hermeneutical Function of Distanciation (1973), some critical distance is required.
There is then, in all of this, a considerable overlap between Blumenberg and Gadamer. Blumenberg does not so much oppose Gadamer as develop aspects of his position—particularly those which are required for his answer to the question: how is the matter elicited by the classic text validated as a matter with which we too should be concerned? Specifically, Blumenberg develops the logic of question and answer (provocation/response) through two means: the concept of "reoccupation" (Umbesetzung)—which I will introduce in the next section—and the claim that the history of what led up to an event conditions the history of its effects (Vorgeschichte conditions Wirkungsgeschichte).

**The Logic of Question and Answer as a Process of Reoccupation**

Blumenberg's concept of "reoccupation," I suggest, should be understood as the result of applying the model of dialogue to historiography. Gadamer's understanding of the logic of question and answer has, as a fundamental principle, the idea that due weight should be given to the

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12 Although mention should be made of their "debate" over Blumenberg's critique of the secularization thesis in *The Legitimacy of the Modern Age*. See Gadamer's review in *Philosophische Rundschau* 15 (1968), 201-209. Blumenberg's response has been incorporated into the latest English translation of *Legitimacy* (1983).

In addition, as I shall point out in more detail shortly, in *Work on Myth* (1985), Blumenberg relates favorably to the "aesthetic of reception" of Hans Robert Jauss, a student of Gadamer's. In *The Genesis of the Copernican World* (1987), Blumenberg puts tremendous emphasis on the idea of "reception."
strengths of another's position. "Reoccupation" is deployed to account for the consequences for inquiry when inquiry attempts to abide by this dialogical principle. What does Blumenberg mean by reoccupation?

All change, all succession from old to new, is accessible to us only in that it can be related--instead of to the "substance" of which Kant speaks--to a constant frame of reference, by whose means the requirements can be defined that have to be satisfied in an identical "position." That what is new in history cannot be arbitrary in each case, but rather is subject to a rigor of expectations and needs, is the condition of our being able to have such a thing as "cognition" of history at all. The concept of "reoccupation" designates, by implication, the minimum of identity that it must be possible to discover, or at least to presuppose and to search for, in even the most agitated movement of history. In the case of systems of "notions of man and world" (Welt- und Menschenansicht: Goethe), "reoccupation" means that different statements can be understood as answers to identical questions. (Blumenberg 1983, 466)

So reoccupation is first and foremost a category of continuity--the attempt to discover a minimum of identity. This identity, furthermore, is to be understood in terms of the relationship between question and answer--that is, dialogically. What is new in history is subject to a rigor of expectations--that is, it must be understood as an answer to a particular question.

It is instructive for our purposes to note that when Blumenberg first uses the term "reoccupation" in The

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13 Recall Blumenberg's critique of Kuhn's concept of "scientific revolution," mentioned in Chapter One (Blumenberg 1983:465). "Reoccupation" is intended to fulfill the requirements Blumenberg demands of Kuhn.
Legitimacy of the Modern Age, he opposes it to the idea of "transposition" that is at the heart of the secularization theory of Karl Lowith.

What mainly occurred in the process that is interpreted as secularization, at least (so far) in all but a few recognizable and specific instances, should be described not as the transposition [Umsetzung] of authentically theological contents into secularized alienation from their origin but rather as the reoccupation [Umbesetzung] of answer positions that had become vacant and whose corresponding questions could not be eliminated. (Blumenberg 1983, 65)

One should note that Blumenberg does not use the word Versetzung [displacement] to make his claim. Displacement is not flexible enough to bear the nuance Blumenberg intends, since it too exclusively implies "supplanting" or "actively removing." This is not to say, however, that Umbesetzung does not carry a sense of "displacement," for it can also be translated as "reshuffling." The deployment of the term "Umbesetzung", I would suggest, is intentionally ambiguous. Blumenberg plays of the dual sense of reoccupation and reshuffling. "Reoccupation" implies the satisfaction of obligations that are imposed upon us--a "problem-pressure," as he puts it, from which we cannot simply escape (Blumenberg 1983:48, 64ff). "Reshuffling," on the other hand, implies activity on the part of the recipient of such questions.

The operations of this duality can be understood better by linking them up to our earlier problem: the operations of the classic text. What Blumenberg's position suggests is
that the operations of such a classic text can be differentiated depending upon from which of two perspectives those operations are approached. One perspective would understand the operations of the classic as that which perpetually overwhelms and displaces our concerns by means of a claim upon what should be our "real" concerns. The second perspective would understand the operations of the classic as that which perpetually proves itself capable of speaking to our concerns (i.e., to the particular problems the tradition faces in its understanding of die Sache). The operations of the classic, then, will be variably evaluated, given the difference between these perspectives.

What the concept of Umbesetzung allows us to see, is that the operations concealed by one perspective are revealed by the other: From the perspective that sees the classic text primarily as an innovative claim that disrupts our concerns, and attempts to replace these concerns with its own determination of what is--or should properly be--at issue, what gets concealed is the way that such disruptions (Popperian conjectures) may actually contribute to a proliferation of questions, rather than an amputation of questions that would allow easy access to the matter at issue. Such proliferation may actually serve to block access rather than guarantee it. From the perspective that sees the classic text primarily as a pre-eminent traditionary text, on the other hand, the uncovering of a
hitherto unsuspected element of substantive meaning—which
deductively contributes to the totality of its effective
history—conceals the "shift" that Blumenberg calls
"Umbesetzung." Particular questions—some of which were
raised only because the text was supposed to have been
capable of being applied to them, and thus of answering
them—are reoccupied with new answers. What gives the
appearance of a continuity in content is actually a
continuity in the framework of answer positions.

When the classic text is seen primarily as disruptive of our concerns—as revolutionary—then it is easy to think of Umbesetzung as simply a displacement of one set of concerns by another set which now claims to be that which should occupy us. But how can such a displacement take place? Does the classic miraculously produce these new concerns like a magician pulls a rabbit out of a hat? What would create the conditions for our "enchantment" here? 14

On the other hand, when the classic text is seen primarily as speaking directly to us in our concerns—as pre-eminent in our tradition—then the "preservation" of these concerns of ours by means of the classic's operations can too easily be understood—if it involves any change at all—as at most a transformation, modification, or

development in content, motivated by the acknowledgement and appropriation of the new claims. But at this point appear the two threats I mentioned earlier, which both arise from the failure to be sufficiently critical of our standards for concern: anachronism and distortion.

With the idea of "Umbesetzung," Blumenberg is attempting to overcome the weaknesses of both of these positions. In so doing, he draws our attention to the determinations we have made about our concerns--that is, to the answers we have given to the questions that concern us. Questions do not always precede their answers, says Blumenberg.

We are going to have to free ourselves from the idea that there is a firm canon of the "great questions" that throughout history and with an unchanging urgency have occupied human curiosity and motivated the pretension to world and self-interpretation. Such a canon would explain the changing systems of mythology, theology, and philosophy by the congruence of their output of assertions with its content of questions. (65-6)

Blumenberg's comment suggests that we should not always be led by the approach which evaluates answers only as alternative to a particular question. In other words, we should not simply ask: which answer is better to this question? We sometimes need to ask: to what question does this answer function as an answer? If the claim made by the answer is to be preserved, it will be preserved only insofar as it functions as an answer to a question--or, can still function as an answer to a question.
The idea Blumenberg is driving at, then, is that an answer position within a framework of questions may be "preserved," but now it is understood "better": it may now be seen to serve a different function within the interplay of questions than it was understood to have served; and its answer may serve as an answer to a different question, or serve a different application.

In short, the issue with which we understood a text to be concerned--the issue concerning which the claims of the text were made--can receive a new determination, insofar as the answer the text is understood to be providing itself can receive a new determination. It can thus be with a fundamentally new interplay of issues that the text is seen as being concerned.

**Summary**

Insofar as the purpose of this section was only to introduce Blumenberg's concept of "Umbesetzung" as a development of the logic of question and answer, these last comments should be understood as anticipations. The purpose of Blumenberg's development of the logic of question and answer, which has not yet been achieved, is to answer how the matter at issue elicited by the classic text could be validated as a matter with which we also are/should be concerned. To this point, the dual sense of Umbesetzung as reoccupation/reshuffling has been highlighted, a duality which attempts to forge a path between the two
the text's appearance. Conditions the History of Its Effects

The title of this section can be re-stated as: Pre-history (Vorgeschichte) Conditions Effective History (Wirkungsgeschichte). This is a central claim made by Blumenberg, and it needs to be understood in conjunction with the idea of reoccupation. The upshot of the last section was that a classic text can be revolutionary—as long as the claims it makes fill (reoccupy) the positions of the questions (and answers) it disrupts. In other words, even a revolutionary text does not simply, or straightforwardly "disrupt." It does not generate, or self-activate, its own, authoritative, effective history. There are conditions for such disruptions—conditions for the possibility of the text's reception, of the text's effects. Blumenberg's claim is that the history of what leads up to a
text's reception conditions its effects.

The phrase is itself taken from the title of one of the chapters of Blumenberg's book, *The Genesis of the Copernican World* (1987). Blumenberg states the nature of his concern:

My objective was to show that in the history of philosophical thought and of its role in the foundation of modern science it cannot only be a matter of presenting the derivation and development of particular ideas and hypotheses, and of bringing to light what stimulated them, and their early forms. Instead, we need to begin one level lower, with the origin of the scope or latitude in which those new conceptions first became possible at all, and within which both the affinities that gave them an effect and the means by which to formulate them arose. (my stress) (Blumenberg, 1987: 167)

Blumenberg is here challenging the hegemony of "histories of influence," which attempt to narrativize the origin and development of particular theories by searching for prototypes and forerunners whose ideas often "take hold" in dramatic—though for that very reason often inexplicable—fashion. These same narratives also search for the appearance of key words or names, and take these appearances as evidence of "effects" justifying the continuation of the narrative. Blumenberg's logic of question and answer rejects the idea that a narrative of how theories are generated and "progress" is sufficient. He claims that his own approach

...does not answer the question as to how the Copernican system arose. It only removes the isolation of that issue from the preconditions of the fact that Copernicus did not become the Aristarchus of the sixteenth century, a thinker without any effect. (167)
It is understanding precisely this lack of effect which is central to Blumenberg's concerns.

Our subject is.....the conditions of the possibility of the fact that there is any such thing as a history of Copernicus's effects--which is by no means a matter of course, since there had not been such a history in the case of Aristarchus of Samos. (131)

The task is to identify the background conditions of the assurance that enabled Copernicus to have any expectation at all that his readers would consent to the work's full claim to truth.....How could he, for his part, avoid his forerunners' and predecessors' manifest failure of encountering an audience that is not only uncomprehending but committed to incompatible assumptions? (128)

First and foremost the claim that pre-history conditions effective history affirms the importance of the logic of question and answer for our understanding of texts--particularly "classic," or "revolutionary" texts. By means of this phrase, Blumenberg suggests that a classic text does not respond only to a determinate set of problems, but in fact responds also to a broader horizon of expectations.15 The classic text operates both upon the determinate questions to which it responds, and the horizon of expectations to which it brings its own claims. In a way that still needs to be explained, it is by these means that the text serves not only as a source of answers, but also as a source of new questions--which constitute its effects.

It is important to note at this point that these new

15 This horizon of expectations relates to what was earlier called the "strength" of the other's position--what Popper calls "the real power of a theory."
questions do not just open "in front" of the text. The classic text opens up questions both in front of itself and behind itself. The pre-history of a text, then, conditions what that text opens both in front of itself and behind itself. Pre-history does not just condition the effects the text may have on the future--it does not just condition the questions it directs to the future. It also conditions the effects the text may have in opening new insights into the past--that is, in directing new questions at the past, or to the past, or even in opening a conversation with voices of the past that have been silent (or silenced). The effective history of a text, then, in a sense operates forward and backward. It not only opens a future for us through the questions it directs to us, but also opens the past (for us as well) through the questions it directs to us.

The matter at issue which the text thus elicits into presence is a fundamentally "new" matter, not only (or even

16 Parallels to numerous thinkers present themselves here. For example, this aspect of Blumenberg's position (and, in fact, much of the entire idea of reoccupation) is shared with Arendt: emancipation can operate retrospectively. The insights mentioned here do not need to be so dramatic, of course. Paul Ricoeur, referring both to Jauss and (somewhat more indirectly) to Blumenberg, provides another kind of example:

It is after the fact, by a recoil-effect of Mallarme's lyrical hermeticism, that we are able to release virtual meanings in baroque poetry that had hitherto remained unnoticed. (Ricoeur 1988:172)
primarily) because it is directed to our age, but because it opens its own age and previous ages to us in new ways--i.e., raises new questions that can be directed at that age.

What is accomplished, then, is a renewal of the concerns of the past--but not in the sense that these concerns are directly validated for us; rather, these concerns are raised as questions for us, in the sense that they are not something from which we can simply "break" or move forward, because we are not clear about what it is from which we are distinguishing ourselves. We cannot "surpass" it unless and until we know what it is that is being "surpassed." In this sense, then, "crisis" is not a "conflict" of interpretations, since we are not sure that there is a conflict. Crisis is not definable as dispute between two clearly identifiable opponents--it is a lack of definition and clarity which represents the crisis. Crisis does not begin when problems receive more precise definition, but rather when such definition is lacking.

Blumenberg speaks of the context of crisis in terms of a "latitude in variation":

To speak, in the history of science, of a "latitude" for possible changes means to determine the breadth of variation within which certain theoretical actions are possible and others are excluded. The narrowness or broadness of this enclave of the possible inside the occupied territory of supposed necessities, with the restraint or the freedom that it imposes on or grants to intellectual motions, is determined by the stability or instability of the system of world-explanation in which it inheres. (my stress)
To link this up with the idea of a text's effects, we can say that the conditions of the possibility of a classic or revolutionary text opening up the future (and the past) consist of the stability or instability of the system within which variations are tolerated. It is here that Blumenberg finds some common ground with Kuhn.

The theory of 'scientific revolutions' describes, for the most part correctly, the breakdown of dominant systems as a result of their immanent rigorism, the 'pedantic' disposition of every schoollike mode of thought, which leads with fateful inevitability to the self-uncovering of the marginal inconsistencies from which doubt and opposition break into the consolidated field. (Blumenberg, 1983:467).

It is not just the weaknesses identifiable within a theory which constitute the latitude of variation possible, but also the possibility of opening new questions about the strengths of the theory. But this possibility of calling the strengths of the theory into question is not one which only an opponent of the theory would exploit. This possibility is also--even first--exploited by defenders of the theory--perhaps especially when the weaknesses of the theory have been revealed. Opening new questions about the strength of the theory--the possibilities of its applications--can for a considerable length of time be a common aim of both "dogmatists" and "critics." Both dogmatists and revolutionaries open new questions about the strength of the theory. Both must expect the theory to be capable of such applications, of such extensions, if these
questions are to be truly motivated (i.e., asked in good faith)--it is not always easy, or at all times possible, to distinguish the dogmatist from the revolutionary, a defender from an attacker.  

The concept of Umbesetzung, then, is deployed by Blumenberg to account for the burdens we accept and the strategies we employ as participants in inquiry. Specifically, as such participants we seem required to assess what it is we are capable of answering, and what it is that we must answer. Plutarch related the warning that we should not be hasty to destroy what we may not be capable of replacing or improving upon. The thrust of this warning is that we should not expose, or re-expose, ourselves to demands or needs that have been satisfied. On the other hand, the status of these needs need not be guaranteed simply because they have been satisfied. It is possible that they were satisfied only because there was no reason that they not be satisfied. We are all familiar with the political quip that some things are done only because they can be done. Blumenberg makes the point that some questions are introduced because there is no reason that they should not be answered.

The difficulty we face is in distinguishing such questions from those which raise questions of real need.

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17 In Part IV of *The Legitimacy of the Modern Age*, Blumenberg presents a detailed analysis of Nicholas of Cusa and Giordano Bruno as cases in point.
Blumenberg does not so much settle these standards as point out how certain questions are provoked. But in this case too the extremes of an intolerant dogmatism and enthusiasm are, as Popper pointed out, to be avoided: the first because it believes it to be its good fortune that it can at least answer the questions it must, the second because it believes it must answer the questions it can. When stated like this, the modern age does seem to have a closer relationship with "enthusiasm" than it does to dogmatism. The advantage of describing ourselves as situated best when we can avoid both of these extremes is that it allows us to recognize and face the revolutionary's dilemma head on: the conjecture of new ideas requires rebellion, but this seems to require that we simultaneously restrain the rebellion of others—a rebellion which may too easily destroy that which it cannot replace. Revolution, therefore, issues "reproaches" both forward and backward, and may justifiably fear nothing more than an enthusiastic reception of its

18 Blumenberg spends much the entirety of Part Three of The Legitimacy of the Modern Age—"The Trial of Theoretical Curiosity"—making a similar point. We might say that insofar as curiosity has been rehabilitated by modernity, it opens the way for the possibility of revolution. At the same time, its position within the medieval schema as a vice—as a concern with the superfluous, with things unnecessary for salvation—is reoccupied by "enthusiasm."

19 This point also invites comparison with Arendt's On Revolution, for she also points out the revolutionary's need for authority.
activities. Dogmatism and revolution, insofar as they do avoid the extremes, seek to hear what is strongest in the other: The demands of authority are less problematic for dogmatism than is the demand for openness—-but openness is a problem for it. On the other hand, the demand for openness is less problematic for the revolutionary than is the demand of authority—-but authority is a problem for it as well.

This underscores the relationship between Blumenberg's analysis of reoccupation and his emphasis on pre-history. Reoccupation highlights not only the ways that ideas function as answers to questions, but also much of the "novelty" of ideas that is traceable to such functions. For instance, Blumenberg seeks to understand how it is that a particular idea or theory seems to function successfully as an answer at one time but not at another—-in other words, how it is thrust into the spotlight at one time, yet relatively neglected at other times. Blumenberg is not denying that such theories are always "available," but he is

Stanley Cavell makes a similar point with respect to an audience's response to a text. Rather than use the terms dogmatism, revolution, and enthusiasm, as I have done here, Cavell distinguishes between readers with whom the text is "genial" and those who are the text's "feared" readers, for whom it sets up reproaches against their approach (Cavell 1989:11-12).

I interpret Blumenberg's article, "On a Lineage of the Idea of Progress" (1974)--together with his claim, in The Legitimacy of the Modern Age, to the effect that the idea of "progress" was placed into service as an answer to the question of the totality of history--to be undertaking this kind of investigation.
denying that we can answer the question concerning the "rise" of a theory to prominence by simply tracing a "substantive progression" (or self-activating tradition) in a direct line from its earliest formulations and forerunners to the "mature" product. Instead, Blumenberg suggests that a theory comes to prominence only when it can function as answer--much like Kuhn's exemplary problem solution--to a variety of questions (within a network of questions which it has, to some extent, extended). In other words, we should not be fooled into thinking that the power of a theory resides simply in the directness of its claims, as a direct response to an immediate and identifiable problem in the field. Nor should we try to give account of the strength of these claims by tracing their "development" over many generations until they have been honed into the instrument they now are. Rather, for Blumenberg the power of a theory is its power of reoccupation--its ability to undergo the kind of reshuffling that allows one to see it as serving in hitherto unrequired capacities--but unrequired only because these had been previously satisfied by other means.

Summary

Blumenberg makes the claim that the questions which a text opens behind itself are those which unsettle answers,

22 Blumenberg's point might be fruitfully compared to Steve Fuller's critique of narrative progressions (1991). In this regard, see the comments made in Ingram (1993:30-1), and Ingram (1994).
such that these answers can be understood as directed (or be free to be directed) to the determination of other questions. These answers, in other words, are directed to different matters than those to which we had understood them to be directed. At the same time, the questions to which we believed these answers had responded now require a new answer--a new determination--which is part of what we understand the text as attempting to offer (another part being its answers to questions which are pressing--which have not received a solution). The importance of opening questions both before and behind itself, therefore, is that the text's novelty cannot be understood as a disposition that is pointed only forward. Instead, the disposition of the text toward closing the gap between the determinate questions to which it has responded and the indeterminacy of expectations for which it has offered some determination is one which opens questions in both directions: it sheds new light on the past and past texts, and raises new questions concerning these, as well as opens new questions which point forward.

A classic text, then, is not defined as a text that deals with eternally unsettled questions or pressing problems--a canon of great questions. A classic text is one which both broadens our concerns by opening questions that lead into the past--and into "tradition"--and also opens questions that reorient our present concerns.
The argument of this paper has been that Hans Blumenberg's model of the logic of question and answer provides a better solution to the problem of the complementarity of dogmatism and criticism, raised by Kuhn, than do the alternatives offered by Popper and Gadamer. Gadamer's solution to this complementarity ends up accommodating both dogmatism and criticism as aspects of a dialogue (experience) that takes place within, and reforms (re-confirms) the framework of tradition. Popper's solution to the complementarity ends up accommodating dogmatism and criticism as aspects of a dialogue which perpetually breaks free from that framework. If the problem with Gadamer's solution is that it threatens to suppress the emancipatory possibilities offered by revolution, and thereby makes us ideological supporters of tradition, then the problem with Popper's solution is that its affirmation of revolutions in permanance threatens to undercut the requisite continuity and confirmation (reception of authority and reception by authority) necessary to keep revolutions from being reduced to a mere succession of self-cancelling, mutually impotent, episodes. In other words, the transfer of power and
authority necessary to sustain revolution cannot occur unless, and until, the determination of "where the real power of our previous theories lies" is made. But an attitude of perpetual framework-breaking all-too-easily overlooks and dispenses with the positive, enabling conditions of our frameworks, and thereby undermines an important component necessary for the deployment of the idea of progress. In short, Gadamer tips the balance of the complementarity toward tradition, and Popper tips it toward revolution.

In this conclusion, I will make a brief attempt to make clearer how Blumenberg and Kuhn keep the balance from permanently tipping to one side or the other. Furthermore, I will attempt to state more explicitly not only the parallels between Kuhn's and Blumenberg's positions, but the developments for our understanding of the ideas of progress and revolution which result from their interaction.

The "essential tension," so dubbed by Kuhn, between dogmatism and criticism, tradition and revolution, can be a rather unhappy mix. Blumenberg, no less than the others considered in this paper, recognizes this. I have called attention to Blumenberg's misreading of Kuhn's understanding of scientific revolutions at various points in this paper. I want to renew their dialogue here--and at the same time focus on Blumenberg's understanding of the essential
tension--by citing the claim made by Blumenberg just before he offers his critique of Kuhn.

In the progress of a science, the same thing takes place--in exemplary fashion, almost as though in a test tube, with greater clarity--that in more diffuse manifestations keeps the general historical process in motion: An established system produces for itself the instruments with which to secure itself thoroughly and to extend the sphere of objects that it comprehends, and in the process continually refines the forms in which it is justified and applied, with the result that in this way the system itself brings to light and accentuates the data that go beyond what it is able to master and to enclose within the prescribed frame of the accepted assumptions. This is the description of a logical situation that Aristotle had already put under the heading of aporia [difficulty of passage, lack of resources] and that Kant had discussed as the fundamental "transcendental dialectic." In both cases the process of cognition itself forces the abandonment of its presuppositions and the introduction of new elementary assumptions, which, while they do remove the situation from which there was no way out, do not require the shattering of the identity of the overall movement that gave rise to the situation. (Blumenberg 1983:465) (my emphasis)

I will now offset this passage with two taken from Kuhn.

The early attacks upon the resistant problem will have followed the paradigm rules quite closely. But with continuing resistance, more and more of the attacks upon it will have involved some minor or not so minor articulation of the paradigm, no two of them quite alike, each partially successful, but none sufficiently so to be accepted as paradigm by the group. Through this proliferation of divergent articulations (more and more frequently they will come to be described as ad hoc adjustments), the rules of normal science become increasingly blurred. Though there still is a paradigm, few practitioners prove to be entirely agreed about what it is. Even formerly standard solutions of solved problems are called into question. When acute, this situation is sometimes recognized by the scientists involved. (Kuhn 1970: 83) (my
In the sciences....new approaches do not ordinarily emerge simply when old ones stagnate, cease to produce novelty. Instead, they await a time when the traditional approach is seen to have failed in resolving problems acknowledged to lie within its competence. (Kuhn 1980:190) (my emphasis)

Given the expositions of Blumenberg's and Kuhn's positions throughout this paper as a backdrop, if one were to compare the highlighted portions of these passages to one another one would uncover a significant complementarity in their respective understandings of the dynamics of the "essential tension."

For both Blumenberg and Kuhn, aporia (defined as difficulty of passage, or lack of resources) ultimately motivates scientific revolutions. However, as Blumenberg suggests, although a "way out" is required, that way out does not require the shattering of the identity of the dynamic that gave rise to the aporetic situation. Were we to place this claim within the context of Kuhn's comment that the "acuteness" of the aporetic situation is only sometimes recognized, then I would suggest that for both Blumenberg and Kuhn revolutions are often "invisible" for the following reason: New determinations of a question by means of a novel (revolutionary) answer often conceal the fact that such answers occupy old question positions. For both Blumenberg and Kuhn, successful revolutions are often invisible because they are not intentionally undertaken by
agents following the demand of an imperative for revolutions in permanence. Revolutions, for Kuhn and Blumenberg, are not simply the imposition and expansion of a new framework of questions in place of the old framework—they are not framework-leaping or framework-breaking events. Instead, revolutions succeed when they reoccupy the positions of the old framework.\footnote{Revolutions do, of course, introduce additional questions which "extend" the framework.} Blumenberg's fundamental criticism of Gadamer is that tradition is not re-confirmed, but re-occupied. Finally, because such reoccupation is concealed, progress begins to look like either a simple accumulation (or addition) of answers and problems (which overlooks precisely how such answers may actually reoccupy old question positions), or radical framework-breaking—by which we always move into "roomier" frameworks, as Popper says. For Blumenberg and Kuhn, the idea of progress can still be deployed, though it means something very different from either of these two alternatives: reoccupation of the framework of question positions, not its confirmation, satisfies the conditions of continuity against which progress (unblocking the way) can be identified.
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VITA

Mark Tazelaar

Education

My area of specialization is twentieth-century continental philosophy, comprising hermeneutics, phenomenology and existentialism, and the Frankfurt School of Critical Theory. My areas of competence include: philosophy of science, philosophy of history, history of philosophy, philosophy and religion, and social/political philosophy.

My doctoral candidacy examination (1992) was on the topic "Universal-historical Processes of Rationalization." The examination committee consisted of Drs. David Ingram, chair, Adriaan Peperzak, Hans Seigfried, David Schweickart, and Hent de Vries.

My language proficiencies are in German and Classical Greek. I have competencies in French and Latin.

My M.A. in philosophy is from Loyola University Chicago (1992). The concentration was in historical studies, with an emphasis on contemporary continental philosophy. My Master's Candidacy Examination was on the topic, "Proofs for the Existence of God." Central figures treated included Aristotle, Aquinas, Descartes, Ayer, and Ricoeur.
I received a B.A. from Trinity Christian College (1983). The major field of study was philosophy, with minors in German, history, and theology. I also studied Classical Greek for two years.

**Employment History**

From the Fall term of 1991 to the present I have been contracted as an Adjunct Instructor in philosophy at Loyola University Chicago, Trinity Christian College, Triton College, and Morton College. During that time I have taught a total of 25 courses. Among the courses I have taught are the following: Introduction to Philosophy (10), Philosophy of Human Nature (3), Action & Value: Society (3), Logic (3), Philosophy of History (2), Ethics, Business Ethics, Social & Political Philosophy, and Ancient & Medieval Philosophy.

**Invited Papers**

To date I have accepted invitations to deliver three scholarly papers. In December, 1991 I delivered a Humanities Lecture at Trinity Christian College, entitled, "Hans Blumenberg and the Legitimacy of the Modern Age." In April, 1993 I participated in a colloquium at Calvin College, at which was discussed a paper of mine entitled, "Rorty's Appropriation of Kuhn and Gadamer: A Critique." Also in April of that same year I delivered a paper at Wheaton College entitled, "Balancing Temporal Distance: Blumenberg on Effective History."
Written Work

In addition to the dissertation to which this Vita is appended, I have published an article, "Sanitized Language is not Enough," TCC Bulletin (XXXI, 1), Fall 1994. I have also completed, and am submitting for publication, an article entitled, "Situating Blumenberg Between Gadamer and Habermas: A Rejoinder to Robert M. Wallace." Finally, I am working on a paper with the title, "Hans Blumenberg on Theoretical Curiosity and Theoretical Economy."

Honors

I was awarded a Graduate Assistantship in the Philosophy Department at Loyola University Chicago, 1983-86. As an assistant I worked both as an Assistant Editor, and as a Teaching Assistant.

At Trinity Christian College, I was awarded an Honors Scholarship for the years 1980-83. I was also chosen by the faculty to serve as the Student Representative to the Academic Affairs Committee, 1982-83. I was also chosen to be the Philosophy Department's Student Assistant, 1982-83, and the Theology Department's Student Assistant, 1981-83.
The dissertation submitted by Mark Tazelaar has been read and approved by the following committee:

David Ingram, Ph.D., Director  
Professor, Philosophy  
Loyola University Chicago

James Blachowicz, Ph.D.  
Professor, Philosophy  
Loyola University Chicago

Hans Seigfried, Ph.D.  
Professor, Philosophy  
Loyola University Chicago

The final copies have been examined by the director of the dissertation and the signature which appears below verifies the fact that any necessary changes have been incorporated and that the dissertation is now given final approval by the committee with reference to content and form.

The dissertation is, therefore, accepted in partial fulfillment of the requirements for the degree of doctor of philosophy.

October 23, 1996  
Date  

[Signature]  
Director's Signature